CUL-H03134-46-02459.63.

THE AMERICAN ECONOMIC REVIEW

VOLUME LIII

46

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THE AMERICAN ECONOMIC ASSOCIATION
Executive Office: Evanston, Illinois
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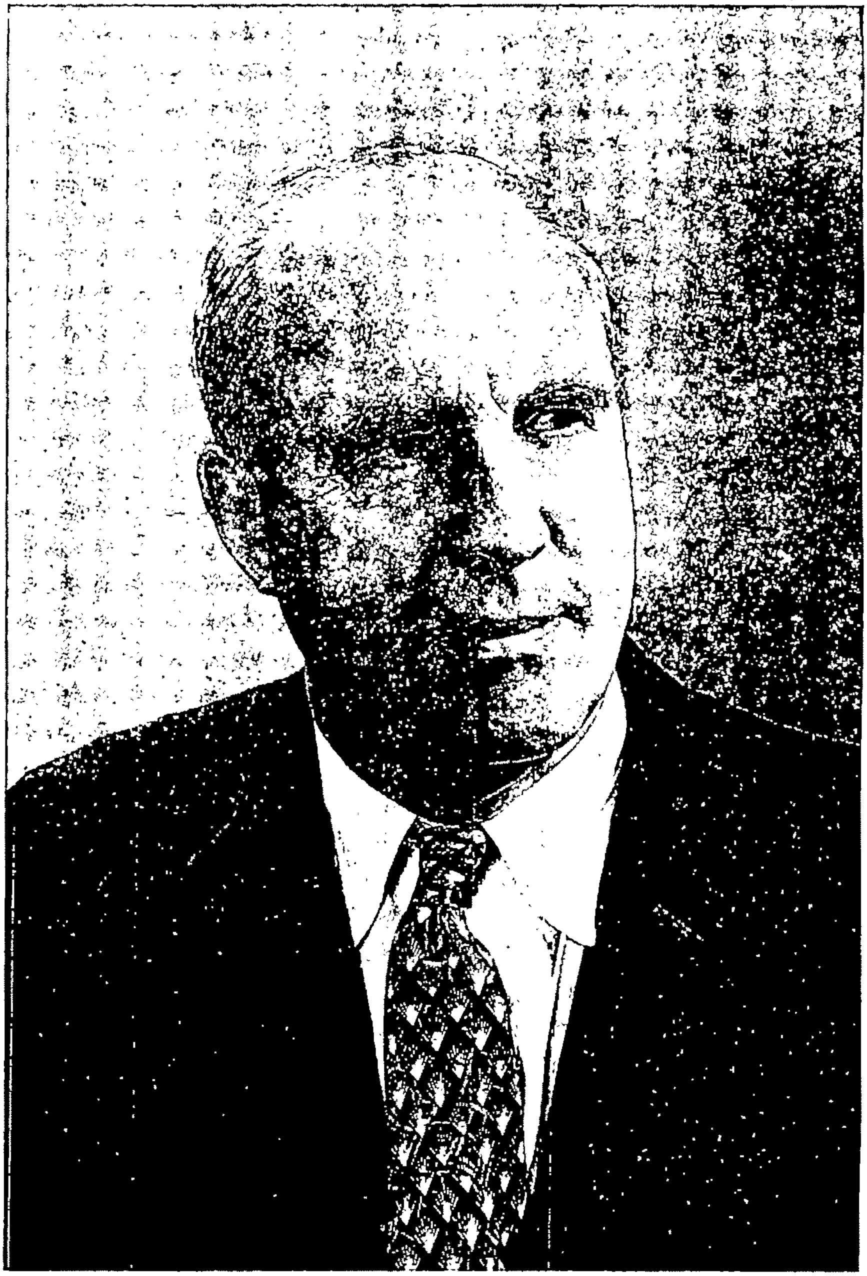

Edward S. Mason

The American Economic Review

VOLUME LIII MARCH 1963 NUMBER ONE, PART I

INTERESTS, IDEOLOGIES, AND THE PROBLEM OF STABILITY AND GROWTH*

By Edward S. Mason

The question I propose to consider is whether or to what extent the current relations between government and various interest groups in the United States prevent, or at least handicap, the attainment of stability and growth. That this question is both serious and difficult requires no argument. It is now six years since we have experienced a level of unemployment as low as 4 per cent of the labor force. During this period the growth of national income has been substantially below the long-term trend. During this same period nearly all of the advanced countries of Western Europe have established an employment record much better than ours and most of them have attained a growth rate which, though it may not persist, is currently at least double the U.S. rate. Are the relations between government, business, and labor in these countries more conducive to stability and growth and, if so, is there something we could learn from them?

Stabilization, of course, must be interpreted within the context of a continuous increase in the labor force and the employment of capital. It is a necessary but not sufficient condition to the attainment of whatever is considered to be a feasible and adequate growth rate. The movement from a relatively high to a relatively satisfactory level of unemployment is not a once-and-for-all step towards higher national income but a preliminary condition to a growth rate that is probably somewhat higher than is possible with substantial slack in the economy. Obviously stabilization policy does not encompass the whole of the growth problem. Denison has made us familiar with some of the steps that would need to be taken if we seek to raise the full employment growth rate [2]. The prospects for growth can also be affected by the choice of routes toward full employment. I am not, however, concerned here with an examination of the full range of growth considerations. I link stabilization with growth policy because there is in fact a connection. The investment, pricing, and innovation practices of firms operating at high levels of capacity would appear to be more conducive to growth in the economy as a whole than policies adapted to substantial slack. But it is only in this sense that I am here concerned with growth problems.

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* Presidential address delivered at the Seventy-Fifth Annual Meeting of the American Economic Association, Pittsburgh, December 27, 1962.

The relations of special groups to government as they affect stabilization and growth policies are usually discussed under the headings of "interests" and "ideologies." Are there objective economic interests represented by particular groups having sufficient political power to influence public action which impede or prevent effective stabilization or growth policies? Are there casts of thought or opinion motivated (subconsciously) by interests, and permitted to survive by ignorance, that effectively handicap sensible action? Keynes held firm views on these questions and in a famous phrase asserted that "it is ideas, not vested interests, which are dangerous for good and evil." On the other hand he painted a picture of the role of government in a mature society together with the "euthanasia of the rentier" that was not calculated to appeal to their interests as conceived by all groups. My colleague, J. K. Galbraith, has written in the same vein and has succeeded in antagonizing a good part of the profession by describing as "conventional wisdom" the orthodox ideas he conceived as impediments to progress. I suppose that the opposite of conventional wisdom is enlightened wisdom which, if we possessed it, would certainly help in the solution of the problems before us. But, in addition to interests and ideologies, I should like to introduce another range of considerations that needs to be taken into account. I refer to sheer ignorance.

When one considers the outcome, during the last year or two, of short-term forecasts undertaken by some of the most respected of our colleagues both in and out of government it is difficult to resist the conclusion that there are some things about the functioning of the economy that we do not yet know. Let me illustrate this point with another example. What would be the probable effect on the U.S. balance of payments if we attempted by appropriate fiscal and monetary measures to attain a 4 per cent unemployment rate by, say, July 1, 1964? It is certainly possible to say something about what would be involved in sectoral growth rates and to proceed from this to a reasonable estimate of the effect on commodity imports. One view, held in respectable quarters, is that such a development would bring with it an increase in gross national product of about $30 billion (over and above the growth that would be expected if the percentage of unemployment did not change) and an increase in commodity imports of perhaps $1 billion. This adverse effect on the balance of payments would certainly be offset to some extent by a decrease in the flow of long-term investment capital abroad though this is much more difficult to estimate.

The difficulties are compounded when one attempts to estimate the effect of the proposed action on export earnings and on the flow of short-term funds. How will expansion of output affect the competitive position of U.S. exporters? Clearly much depends on what happens to wage rates, wage costs, and wage-price relationships. During the period 1955-57 when unemployment averaged 4.3 per cent the price level increased by 2½ per cent a year. On the other hand there were special influences at work, particularly in the durable goods area, that may make this period an inappropriate guide to future prospects. If some significant general rise in wage rates does take place and if prices are adjusted to this increase, the extent of the adverse effects on export earnings will depend, among other things, on what, in the meantime, happens to prices in the principal export markets. Clearly this is an area in which estimates will be subject to a sizeable margin of error. Finally, how will the Swiss bankers and their friends in Wall Street react to the particular combination of fiscal and monetary policies deemed necessary to attain a 4 per cent level of unemployment? Will short-term funds move out of the United States and will there be a large conversion of dollars into gold?

These difficulties and uncertainties are mentioned not to convey the impression that there is no solution to the problem but to suggest that there are several solutions, each one of which may be supported by intelligent and honorable men not obviously influenced either by interest or by dogma. It is certainly possible to support with strong arguments the position that the balance of payments comes first and that until confidence in the dollar has been restored it would be unwise to undertake any very significantly expansionary monetary or fiscal policy. It is also possible for sensible men to espouse the opposite position; that we can, by a proper combination of fiscal and monetary measures, move within a reasonable period of time to a position of full employment without inflicting on our balance of payments a strain that no practical expansion of international liquidity can manage.

This matter of ignorance is introduced into the discussion because it modifies quite substantially the significance to be attached to "interests" and "ideologies" in relation to stabilization and growth policies and because it has a bearing, I believe, on the questions whether and how a more satisfactory relationship between government and special groups may be established. It is possible to argue convincingly that vested interests of one sort or another prevent a solution of a problem only if it can be shown that in the absence of these interests the problem would in fact be solved. Certain modern Marxists, for example, with a Keynesian slant, hold that persistent stagnation must be our fate because, although government spending could fill the gap supposedly caused by an inadequacy of investment opportunities, private interests in the perpetuation of traditional government-business relations will not permit it to play this role. But to make a case for this view, it would be necessary to demonstrate that, (a) in the absence of a large increase in public spending, investment opportunities will in fact fall short of absorbing the intended volume of savings; (b) under these circumstances business interests will be better served by persistent stagnation than by any practical change in the role of government that could overcome stagnation; and (c) business has sufficient political power to impose its interests on the public. To the extent, however, that each of the propositions is shrouded in doubt, the significance to be attached to the role of vested or special interests becomes far from clear.

Parenthetically, it is a matter of some interest that the Marxian analysis of capitalism seems to have subtly shifted from an emphasis on the economic "laws" of capitalist development which must inevitably bring to a close this stage of history to an emphasis on the socio-economic structure of political power which will inhibit the use of any governmental measures that might be expected to make capitalism work.

To say, however, that the recognition of a substantial area of ignorance somewhat modifies our interpretation of the role of interests and ideologies is not to deny that interests and ideologies significantly limit and influence the choice of stabilization policies. As any reader of the public prints or of testimony before the Joint Economic Committee knows, quite different analyses of the reasons for the current stagnation and quite different recommendations for action emanate from business and labor groups. And quite different explanations of the "secret" of Western European growth rates are advanced.

It is difficult to characterize what is after all a fairly wide spectrum of business opinion, but typically two considerations dominate the business arguments: the burden of taxation on savings and investment and the power of trade unions to push up wage rates. It follows that any significant revamping of the tax structure should reduce the corporate income tax and the upper brackets of the personal income tax and if possible shift some of the burden from direct to indirect taxes. Even with this additional inducement to invest the economy will quickly tend to run into inflation unless something is done to curb the power of trade unions to push up wage rates. Since investment rather than consumption is the crux of the difficulty, monetary policy is to be preferred to fiscal measures designed to increase aggregate demand. If fiscal measures are nevertheless used they should clearly emphasize tax reduction rather than increase of government expenditures. And in no case should fiscal measures prevent a balancing of the budget over the business cycle. The business case tends to emphasize growth, mainly through increase in plant-and-equipment expenditures, rather than stabilization. Indeed, there is some tendency to deny the existence of a stabilization problem in any other sense than that which relates to the price level and balance of payments. The high unemployment figures tend to be attributed to the vagaries of U.S. employment statistics or to the unwillingness of labor to accept employment in the areas in which it is offered. Higher growth rates in various European countries are attributed to tax and monetary policies that are more adapted to a "capitalistic" environment than those of the United States.

The analysis of the problem as presented by labor spokesmen tends to rely heavily on the purchasing power argument. Increasing purchasing power by increasing wage rates is now, perhaps, somewhat passé because of the rather too obvious connection with wage costs and prices. Fiscal policy is currently the preferred route and increase in government expenditures has much to recommend it in view of deficiencies in services that only government can provide. If, however, tax reduction is preferred, cuts should be limited to the lower end of the scale where tax relief will increase consumption rather than savings. Investment is considered to be a function of output and profits will be fully adequate both as a source of savings and as an incentive to invest when the economy is operating at close to capacity. Collective bargaining as it presently functions is essentially satisfactory and there is no case either for reducing the bargaining power of unions or for administrative control of wages and prices. The balance-of-payments problem tends to be somewhat neglected in labor pronouncement, except as it is affected by the investment abroad of U.S. capital.

It is not my purpose here to evaluate either the business or the labor argument. It would be difficult, however, for such different analyses and recommendations to continue to exist side by side were it not for a substantial area of ignorance concerning the relation of magnitudes relevant to stabilization policy. Furthermore, the presence of ignorance seems, in a perverse sort of way, to harden conflicts and to exacerbate group relationships. It might be thought that the existence of considerable uncertainty concerning the effects of a proposed course of action might lead to a more cautious statement of claims and counterclaims. In fact, it seems to produce the reverse behavior. Since each group can rightfully claim that the opposition's case is not demonstrably correct or in certain respects is incomplete, the weakness of the case is attributed to the influence of special interests or willful misunderstanding. And under attack conceptions of interest tend to narrow and ideological positions to harden.

It must, of course, be recognized that institutional relations and ideologies do change under the pressures of circumstance. The 1930's witnessed a quasi-revolutionary shift in the relationship of government, business, and labor in the United States and this shift was accompanied by the public acceptance of substantial changes in ideas concerning the functioning of the economy and the role of government therein. A sobering question that now confronts us is whether it will require a longer period of stagnation and deeper depression than we have experienced over the last six years to force changes in institutional relationships and ideologies and, if so, what shape are those changes likely to take? Or is it possible through some process of social invention and adaptation to accomplish necessary changes without waiting for the build-up of irresistible pressures?

One of the difficulties in the current situation is that 5½ to 6 per cent unemployment and an annual growth rate of a little more than 2½ per cent do not seem too serious. Before the advent of unemployment insurance, unemployment meant misery. Now it is interpreted to mean lost output, a concept that presumably has some significance for statisticians, economists and such ilk but does not impinge heavily on the consciousness of most people. With per capita incomes at $2,800 and average family incomes between $6,000 and $7,000 it is also somewhat difficult to get excited about the lagging U.S. growth rate. Furthermore, it is easy for apologists of the *status quo* to point at any given time to the fact that output, employment or almost any figure you care to name is, in a favorite phrase, "the highest in our history." One is inclined to sympathize with the plaintive enquiry of Senator Javits who, after listening to testimony of this sort, was moved to ask "Why, if all of our indexes are up, are we very worried?" [12, p. 13].

It is, however, becoming increasingly difficult to deny that U.S. unemployment figures mean what they purport to mean. Even after adjustment to U.S. reporting techniques the unemployment rate in 1960 in France was 1.9 per cent, Great Britain 2.4 per cent, Germany 1 per cent, Sweden 1.5 per cent, while ours was 5.6 per cent [11, p. 220]. The divergence of our growth rate from those of the principal Western European growth rates, and indeed from our own long-term trend, has been too persistent and too glaring to be ignored. Moreover, though the record of the last five years may be brushed off as atypical, it is difficult to brush off the longer-term implications. If stagnation persists the labor pressure for a thirty-five hour week is going to assume dangerous proportions and what this would do to our balance of payments problem is not pleasant to contemplate. Furthermore, as firms operate at relatively low levels of capacity, the expectations on which price and investment policies are based are bound to be affected. There is some evidence of this already, and there is enough in the administered price doctrine to justify a fear that in many markets adjustment of price policies to lower use of capacity is a relatively easy matter. A real danger of stagnation is that in an economy in which economic units are large enough to resist the pressure of market forces it will induce policies and practices that insure the persistence of stagnation.

But, having recognized that no one of the numerous protagonists is likely to have a monopoly of the truth, how is the relationship of the various interest groups to each other and to government in the United States likely to affect the choice among various possible stabilization and growth policies? To deal with this question it is necessary to say something about these relationships, to consider what limitations, if any, they impose on policy choices, and to offer an opinion on the question whether, in view of these limitations, the possibilities that seem to be open are adequate to the problems that confront us.

There is an old saying to the effect that every country gets the socialists it deserves. It is equally true that every country gets the government, the type of business leaders, and the forms of labor organization it deserves. This is no more than to observe that institutions and group relations are shaped in an historical context that differs substantially from country to country. It is clear to the most obtuse observer that there is a much more distant relationship between business and government leadership in the United States, than, say, in Britain, France, or the Netherlands. The Federation of British Industries has an entree to government, whether Conservative or Labour, that neither the National Association of Manufacturers nor the Chamber of Commerce has or, for that matter, would desire. A British businessman can say, "Some of my best friends are civil servants," and really mean it. This would be rare in the United States.

In France the relationship, at least since the war, has been even closer. The type of planning favored in France brings together government and business decision-makers, with their technicians, on a long series of critical economic issues. I shall want to say something later about the relevance of French practice to the United States but it is obvious that what is called indicative planning in France demands intimate government-business collaboration. In the Netherlands, government, business, and labor collaborate on a national wage policy and government and business participate jointly in other aspects of aggregative planning.

The relationship between government and business in the United States can only be described as one of latent hostility which occasionally, as in the past year, breaks out into rather more open hostility. The traditional visitor from Mars becoming privy to significant domestic events of the last twelve months including the steel decision, the behavior of the stock market, antitrust actions, and speeches of various high government officials, would be hard put to it to explain the bitterness expressed in many quarters. He could only conclude, I believe, that these expressions arose from frustrations and resentments lying deeper in the environment than the surface mirrored by these events. A search for the source of these deeper manifestations leads one, I would suppose, to consideration of certain obvious facts of geography and physical environment, of the still undigested consequences of the revolutionary change in the role of government brought about by the events of the 1930's and the war, and going further back, to consideration of the unique position attained by business in the United States in the period after the Civil War.

What significance is to be attached to the fact that Washington is a purely governmental city without industry and that the industrial centers in the United States are generally far removed from this political focus, I do not know. The fact that London and Paris are both political and business centers no doubt helps to give the old school tie greater scope than it would otherwise have. But the size and geographical dispersion of our population would in any case prevent this kind of concentration even if our business and political centers were one. The dispersion, however, does not adequately explain what seems to me a much sharper separation of elite groups, government, business, professional, artistic, academic, etc. than one encounters in most Western European countries. Despite the continuing rapid vertical social mobility in the United States horizontal penetration appears to encounter greater difficulties. Recently two outstanding businessmen have commented on certain aspects of this phenomenon. Clarence Randall has alleged that as business executives move up in the hierarchy their circle of contacts narrows and that they tend to reach a stage at the top where they talk only to themselves. A Bostonian might be entitled to feel that this is even worse than talking only to God. Ralph Lazarus has taken note of the suburban golden ghettos in which well-to-do business and professional men preserve their families from any contact with more sordid, but perhaps livelier elements of American civilization. However this may be, it seems probable that the size, geographical dispersion, and racial heterogeneity of population have some part to play in explaining the relative distance between government and business in the United States.

Much more important, in my opinion, is the fact that the really revolutionary changes in the role of government and in the relation of various groups to government produced by the great depression and the war have not yet been fully accepted in this country. Where counterrevolution is still considered to be a possibility no one is quite prepared to lay down his arms. This, of course, puts the matter too strongly, but there continues to be a conflict between what in fact—and quite demonstrably—has happened and the views that are held in various quarters concerning the possibilities of action in the light of these facts. Characterizing the situation in the broadest possible terms I am inclined to emphasize the following developments: Government expenditures as a percentage of gross national product have, of course, increased enormously since the 1920's. If one lumps federal, state, and local expenditures together they are currently running at twice the volume of gross private capital formation in the United States. It is obvious, therefore, that variations in these expenditures and in the relation between tax revenues and expenditures can have an effect on aggregate demand, output, and employment that was out of the question in the 1920's. At the same time the role of government in the process of economic decision-making, its "weight," if I may use the term, has in many ways not increased commensurately with its share in total outlays.

Although the relative growth of the public sector has necessarily meant a relative decline in the position of the private sector there has been no significant change in the predominant role assigned to business in the process of capital formation and the direction of investment. Even at the depth of the depression in the United States when business leadership was in nearly total eclipse the voices raised in favor of expansion of government ownership or a larger role in the direction of resource use were feeble and ineffective. The Tennessee Valley Authority was about the only "socialistic" action that can be attributed to the New Deal. The lasting changes of the 1930's were mainly in the fields of social welfare, labor-management relations, and the regulation of capital markets. The great increases in government expenditures came, of course, during and after the war. An observer of the current scene can hardly fail to be impressed by the absence of doctrinal dispute on the proper role of business. Government ownership is a dead issue if, indeed, it ever was a live one. Government regulation of public utilities on which much hope was placed at the turn of the century is in at least partial eclipse. The demand, particularly in the field of transportation, is for less rather than for more. Government has gone very far in turning over to, or permitting business participation in, decisions which elsewhere are made only by government. This tendency has been most pronounced in the field of research and development but it extends also to the loan insurance and loan guarantee schemes of the Federal Housing Authority and is part of a more general trend toward "contracting out" functions that government proposes to support. This is one reason why federal employment has not increased as rapidly as federal expenditures. But it also suggests that there is very little disposition in this country for government encroachment on traditional areas of business responsibility.

Part of the reason for a failure of both government and business to adjust adequately to the change in this relative command over resources is the rapidity with which the change was accomplished. If one examines the history of federal expenditures as a percentage of gross national product there was a continuous decline from about 5 per cent in 1870 (when federal finances still bore some of the Civil War Impact) to roughly 2 per cent in 1912, 1913, and 1914.[1] The First World War, of course, greatly increased this percentage but it is remarkable how rapidly and completely the share of the federal government expenditures in GNP declined after the war. Despite a sizeable increase in interest payments on the public debt the share of total federal expenditures in GNP was only 4.6 per cent in 1922 and declined to 3 per cent in 1929. In that year federal expenditures were less than half of state and local expenditures but all government expenditures as a percentage of national income were distinctly lower than in most Western European countries. The rise in government expenditures began much earlier in Western Europe, and this longer period of growth, it may be conjectured, has had something to do with an obviously more complete assimilation of the changed role of government by the community as a whole.

The great increase in federal expenditures both as a percentage of GNP and in relation to state and local expenditures began only in the 1930's. The share of federal expenditures in GNP increased from 4.1 per cent in 1930 to around 10 per cent by the end of the decade. This high percentage reflected, of course, both the large public expenditure for newly introduced social services, and the low level of GNP still laboring under massive unemployment. Federal civilian expenditures as a percentage of state and local expenditures reached a high point of 52 per cent in 1948 and have declined somewhat since then. It is interesting to note that federal civilian expenditures as a percentage of GNP have not increased since 1938 [5, p. 50]. After a very rapid reduction in military expenditures following the war the disturbed international situation has produced a level of federal outlays that shows little tendency to decline. National defense expenditures accounted for 85 per cent of federal purchases of goods and services in 1961 and total federal expenditures amounted to over 19 per cent of GNP. This is to be compared with the 3 per cent figure in 1929. Big Government in the sense of a big sector of the economy is a comparatively recent phenomenon in the United States and there is evidence that we have not yet completely accommodated ourselves to this fact.

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[1] The figures in this paragraph are taken from Kendrick [5, pp. 10-11].

If one wishes to push the analysis of government-business relations farther back in time he would need to take account of the extraordinary position acquired by business in the society after the Civil War. It is probably not too much to say that its supremacy was contested by no other elite. The Civil War in destroying the plantation aristocracy had eliminated one potential rival. And, as Schumpeter has emphasized, U.S. business developed in an environment in which it did not have to contend with an aristocracy by birth from whose numbers have come so many members of the ruling class in Western Europe. As late as the 1920's Calvin Coolidge could say with conviction and pride that "The business of America is business," and be understood by his fellow citizens.

So far as labor organizations are concerned, the growth of trade unions with government encouragement and assistance has, of course, had a profound effect on labor-management relations not only in the area of unionization but outside. Furthermore, I think it can safely be said that unions are here to stay. Concerning the relation of American unionism to stabilization and growth policy two observations seem pertinent. It is clear by now that one cannot look to American labor for anything in the way of social or political initiative. As one observer puts it, "The labor movement in the United States has taken pride in the fact that it is non-ideological. It has revelled in its bread-and-butter or pork chop personality [1, p. 124]." Higher wages, more job security and, if substantial unemployment persists, a demand for a shorter work week, pretty much define the area of union initiative. Secondly, the development of U.S. unionism has been such as to make the formulation or implementation of anything like a national wage policy extraordinarily difficult. Power is more decentralized than in almost any Western European labor movement and all informed students agree that a nationally organized and nationally effective trade union organization is a necessary, though not sufficient, condition to a successful national wage policy. It may be expected that labor leadership will pronounce in favor of increased government expenditures and of tax reduction concentrating on the lower income brackets, though how much effect this has on the voting tendencies of the rank and file it is difficult to say. Bearing these considerations in mind it is perhaps fair to say that organized labor's role with respect to stabilization and growth policy is apt to be passive rather than active. It can obstruct certain courses of action; it can support others; but it is unlikely to initiate.

How does this melange of special interests, conflicting ideologies, and ignorance affect the prospects for stabilization and growth in the United States and the choice of means thereto? The influence of what, for want of a better word, I shall call institutional considerations seems to me large; larger than is sometimes assumed by our "aggregative" colleagues. They not only help to explain differences in the response in different economies to objectively similar stimuli but in a particular economy bring about changes over time in the relationship of various magnitudes that make the life of forecasters and policy planners difficult. More important, these considerations tend, at any particular time, to set definite limits to what it is desirable or feasible to attempt in the direction of stabilization policy.

The French approach to stabilization and growth, or even the Dutch, both of which, at least superficially, seem eminently successful in their own environment, have very little relevance for U.S. conditions. To begin with, it would have to be recognized that it is impossible to determine to what extent the recent growth rate in France is to be attributed to so-called "indicative" planning. Growth rates in Germany and Italy have been at least as striking without the benefit of anything that could properly be called planning. Indeed, the German economy seems, in some respects, to be less "directed" than our own. The projections of various economic magnitudes with which we are so liberally supplied by the Council of Economic Advisers and other agencies are almost completely lacking there. In France numerous other influences, in addition to planning, may have a bearing on stabilization and growth. One could mention the monetary reforms of 1958; the current and anticipatory effects of the Common Market; the opportunities for technological advance offered by postwar reconstruction and capital replacement; the development, with higher per capita incomes, of economic activities in which scale economies are important; the continued existence of bottlenecks providing lucrative investment opportunities; and, no doubt, many others. Indeed, as one regards the metamorphosis of the French entrepreneur, within a relatively few years, from the traditional "lazy man of Europe" into his present bustling, expansive incarnation one wonders whether economic analysis can provide all the answers.

Insofar, however, as French planning has made a significant contribution to stability and growth its main impact, it appears to me, has been on the degree of uncertainty surrounding both public and private investment decisions. French planning does seem to have been able to make growth targets credible to the people who make investment decisions. This appears to have been made possible by an extraordinarily close association of business men and public officials assisted by competent technical advice. The tentative investment decisions are binding neither on the government nor on business but since representatives of both have participated actively in the planning process there are reciprocal expectations that become difficult to ignore. And as expectations have tended, by and large, to be fulfilled, uncertainties surrounding new decisions diminish.

Reflection, however, on the institutional arrangements that make such an enterprise possible and fruitful in France serves only to suggest how unpracticable this model is for the United States, even if it were considered to be desirable. Government accounts for from 35 to 40 per cent of capital formation in France and consequently public investment decisions are matters of moment to business. A long cartel tradition has accustomed French business to industry planning. The postwar necessities of reconstruction and replacement required government-business cooperation, and relationships useful to present planning activities were developed.

Inadequacies of capital markets in France have encouraged government financing of private enterprise and close control of security issues. French labor has either not been strong enough to inject itself effectively into the planning process or has been willing to leave these matters to government and business. Possibly the softening of partisan activities and the downgrading of parliamentary procedures under a quasi-dictatorial regime have encouraged a certain degree of *dirigisme* in the economy. In any case it should be obvious that there is little in the French situation that is directly relevant to U.S. practice.

The same thing must be said of the Netherlands, though the Dutch do not attempt anything as ambitious as the direction of investment among sectors. The so-called aggregative planning of the Netherlands is scarcely more than effective stabilization policy with growth left to take care of itself. Even so, the institutional relationships that make this policy effective are largely lacking in the United States. Although there has been no great pressure for public ownership in the Netherlands, government operation of railways, public utilities, and mines and extensive participation in land reclamation schemes assures a large share for government in total capital formation. Wages have been subject to comprehensive control since 1945 and the advisory Labor Foundation, representing centrally organized management and labor groups has played a dominant role in the formulation of wage policy [6, pp. 41-42]. The very great dependence of the Netherlands on foreign trade makes the balance of payments a critical element in stabilization policy and helps to assure that fiscal and monetary measures designed to promote stability are not undermined by conflicting wage and price policies. Dutch planning, moreover, is assisted by highly competent technical advice which tends to be heeded by policy-making authorities.

When one considers these and other examples of successful stabilzation and growth policies in Western Europe it becomes clear that their transfer to the United States would be impractical without very large changes in institutional relationships. This does not mean that successful stabilization policy is impossible in the United States but it does mean that certain policy instruments are not available. Undoubtedly the lack is most striking in the wage-price field which Samuelson has called "the biggest unsolved economic problem of our time [8, p. 360]." That this problem is not limited to the United States is indicated by the plea of the OEEC experts on "The Problem of Rising Prices" for a national wage policy.[8] Nevertheless, both the difficulties and our lack of progress in this area seem more striking than elsewhere. In Norway and the Netherlands government participates with centrally organized labor and management groups in the formulation of a relatively successful national wage policy. In Sweden, although wage rates are nationally negotiated without government participation, much attention is given to a wage policy designed to stabilize prices at full employment [7, p. 93]. None of the countries of Western Europe has been able wholly to avoid "wage-drift" or an upward price creep but this has tended to take place at 1.5 or 2 per cent unemployment rather than at 4 to 5 per cent.

Certainly the difficulties confronting anything like a national wage policy in this country are formidable. We lack the nationally organized management and labor groups which are a necessary though not sufficient condition. The significance of relative balance in international payments which has made wage-price stability of critical importance in a number of Western European countries has only recently become apparent in the United States. Opposition in this country both in management and labor circles to government intervention in the wage-price field is stronger than elsewhere. And in general it would have to be said that there is a distrust of governmental initiative in the whole area of stabilization policy that is lacking in Western Europe [4, p. 108].

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[8] "In the view of the majority of the group, the essential element, to be stressed first of all, is that the stabilization authorities must have a wages policy for dealing with the problem of wages—just as they must have monetary and fiscal policies for dealing with the problem of demand" [10, p. 56].

How significant the absence of a wage-price policy is likely to be for effective stabilization measures appears to me to depend principally on two relatively unknown considerations. The first has to do with the question whether the forces that in the period 1953-58 produced an annual 3 per cent per man-hour increase in output, a 4½ per cent increase per hour in total compensation, and a 2.1 per cent increase in wholesale prices have significantly changed [3, p. 137]. That there have been important changes bearing on this problem there can be no doubt. The attitudes and expectation suggested by the phrase "inflation as a way of life" have certainly suffered a severe blow. In a number of basic industries which have hitherto been leaders in the wage push a combination of relatively slow secular increase in output with massive technological change having serious employment effects will inevitably moderate wage demands. In the very important field of national defense industries national security considerations tend inevitably to damp both wage and price initiatives. Finally, persistence of a serious balance-of-payments problem will force government intervention in certain wage or price decisions regardless of labor or business attutudes.

The second consideration has to do with whether an approach toward fuller use of capacity will release, with increased investment, that flood of technical improvements the expansionists foresee. It is certainly quite possible, if the wage-price creep can be held to modest proportions, that the competitive position of U.S. exports would be improved rather than lessened by a closer approach to full employment.

However that may be, it is probably correct to say that the absence of a national wage policy does and will continue significantly to affect our stabilization policy. The effect is already clearly visible in the current definition of full employment as 4 per cent unemployment. The expectation of a wage-price creep helps to explain why full employment does not enjoy the high priority in the stabilization trinity of employment, price level, and balance of payments it appears to enjoy in Western Europe. And it probably means that monetary and fiscal measures will tend to be employed tentatively and step by step pending a testing of wage and price effects.

The second major policy instrument that tends to be blunted by the group interests and ideologies here under consideration is fiscal policy.

I neglect the question of monetary policy for what seem to me the adequate reasons that neither the quasi-independence of monetary authority nor the interests and ideas of special groups offer a serious obstacle to a monetary policy oriented toward stability and growth.[8] The same thing cannot be said of fiscal policy. Minor reasons for a lack of flexibility in this instrument are such restraints as debt ceilings and interest limitations on government securities. The major difficulties, however, are deeply held views on the desirability of budgetary balance, suspicion of almost any increase in government spending, conflicting views of interest groups on proper changes in the tax structure, and, above all, a general distrust of government as a manager of the economic environment. As one observer has noted, "Countercyclical finance is attacked by conservatives in the United States because of fear of irresponsible extension of government power while in Europe it has been recognized as a conservative alternative to more extreme government intervention [9, p. 605]."

It is somewhat difficult to understand the conservative American view since there is no discernible support in the United States for government ownership or control of activities now in the private sector. Indeed, as I have pointed out, there is a strong disposition to "contract out" to private enterprise services that tend elsewhere to be performed by government. It seems possible that, over the longer run, this absence of ideological conflict in the traditional sphere of government versus private ownership may facilitate the development of a flexible fiscal policy. In the shorter run, however, the obstacles are formidable. Perhaps the critical questions are these: Will predilections for a budget that is balanced, at least over the business cycle, prevent a shift in fiscal policy toward a budget that is balanced only at full employment? And will the tax reduction that may be involved in this process strike some kind of an acceptable balance between the incentives to investment demanded by the business group and the stimulus to consumption demanded by labor? If not, the prospects for successful stabilization policy are much darker than they need be.

In any case, the relative weakness of policy instruments in the United States and the latent hostility apparent in government-business relations tends to deprive growth targets of any operational significance. It may be useful to project a 4 per cent growth rate at full employment as a rough indication of the extent to which actual performance falls short of potential but it cannot be assumed that any one is going to do anything about it. The limitations that our institutional structure plus current balance-of-payments considerations place upon the relevant possibilities of action tend to deprive such targets of the credibility they would need to possess in order significantly to affect business decision-making.

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[8] Although the monetary authority is certainly subject to strong pressures, its quasi-independent status appears to provide enough insulation to permit effective resistance.

It seems probable that any one of the "radical" plans for handling the balance-of-payments problem such as increasing the price of gold, devaluation, or expanding the reserve functions of the International Monetary Fund, might open up new opportunities for stabilization policy. It seems probable that none of these courses of action will be followed. It is more likely that, if relief is provided, it will take the form of a reduction of foreign, particularly economic, aid programs. But whether this happens or not the balance of payments as an essential part of the stabilization problem promises to be with us for a long time.

In spite of the obstacles to effective stabilization and growth policies stressed in this paper, the United States may nevertheless move toward a satisfactory growth rate within a context of relative stability. The acceleration of family formation foreseen for the mid-'60's, an unexpectedly rapid exploitation of new technological possibilities, or even unplanned changes in labor-management or other group relationships, might make such a development possible. I have too little confidence in economic forecasts including my own to deny this possibility. But it does seem clear that any approach to a satisfactory situation with respect to stability and growth will owe much less to deliberate policy and much more to autonomous developments in the economy than has been true in recent years in Western Europe. And for this the historically determined relationships between government, business, and labor are largely responsible.

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12. U.S. Congress, Joint Economic Committee, *State of the Economy and Policies for Full Employment.* Washington 1962.

REGIONAL ECONOMICS: A SURVEY

By JOHN R. MEYER*

The creation of a new area of specialization is always a matter of interest in any academic field and the phenomenal rise of what has come to be known as "regional analysis" in economics is no exception.

The manifestations of this emergence are many. Within the last decade both a new professional association and a journal dealing with problems of regional development have come into being. This new journal, published by the Regional Science Association, has moreover all the hallmarks of academic success; it has consistently grown in size, scope, number of contributors, and even in quality of typography. Also within the last decade, a full-fledged and highly successful research institution, Resources for the Future, has been created which is greatly concerned with problems that conventionally are described as falling within the province of the new field. This organization reported in 1957 [102], furthermore, that no less than about 140 U.S. universities had serious research or education programs underway in regional studies and there are substantial reasons for believing that the extent of these activities has probably enlarged rather than contracted since that time. Similarly, conferences, almost innumerable, have been organized to discuss, evaluate, and promote regional science studies both here and abroad. Indeed, perhaps one of the most impressive testimonials to the appeal of the new field is that it has enlisted the official or semi-official interest of such diverse governments, among others, as those of Italy, France, Spain, Greece, Argentina, Turkey, Venezuela, India, Colombia, and Pakistan.

Economists and economics have long had, of course, special surges of interest or, less euphemistically, fads and fashions in their studies and research. Furthermore, if we construe economics as an applied science and argue, at least within limits, that most of the great steps forward in economic understanding and theory have come in response to specific policy challenges, this tendency toward temporarily pursuing certain lines of research with seemingly exaggerated zeal is not necessarily unfortunate. It may be, rather, the very substance and requisite accompaniment of intellectual progress in a field where progress often is concerned with generating principles or concepts for the understanding and solution of practical problems. Understanding such research and its conceptual results or creations normally requires, in turn, an appreciation of the policy problems that have provided the stimuli.

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* The author is professor of economics at Harvard University. This is the sixth survey in a series on recent developments in economics financed by a grant from the Rockefeller Foundation. The author wishes to thank the staff of the David Lubin Memorial Library at the United Nation's Food and Agricultural Organization in Rome for their patient assistance with compiling and verifying the bibliography.

It would be wrong, though, to infer that identification of these stimuli would fully explain the rise and recent popularity of regional economics. Nor would these alone provide an adequate basis for understanding recent developments in the field. Regional economics has very obvious and important intellectual forebears, and indeed it seems highly probable that certain theoretical developments in general economics, particularly having to do with general interdependence systems, provided the conceptual basis without which regional economics could not have flourished. One very straightforward explanation of the surge in regional economics, in fact, is to say that it resulted from a fortuitous blending of many economists' desires to apply certain recently honed conceptual tools and policy makers' desires to seek more adequate and analytical answers to complex problems related to regional and urban growth.

Sensible discussion of either the stimuli or the conceptual precursors of regional economics requires, moreover, a definition of just exactly what constitutes this new field. Special semantic problems, unique to the new field, are also quickly encountered, not the least of which is simply defining a region. The definitional problems will be considered, therefore, in Section I. As will become clear there, definitions of and in regional economics are not always easily come by, a difficulty probably shared in common with almost all new fields of specialization. Following these definitional exercises, a more thorough consideration of the policy-problem stimuli will be undertaken in Section II. Then, in Section III, the theoretical foundations of regional economics, as borrowed from general economics, will be treated. This will be followed in Section IV by a discussion of the different approaches to regional economics that have emerged in actual regional studies. Finally, Section V attempts to summarize and provide some evaluation of all that precedes.

I. *Definitional Problems*

An almost unavoidable temptation when first coming to grips with the problem of defining regional economics is to assert that it is simply *all* of economics scaled to whatever level is required to adequately measure or forecast economic activity for a specific geographic area. There would be considerable truth moreover in such a characterization, for regional economics has been, if anything, ambitious in its selection of objectives. Also implicit in such a definition is a strong urge to define regional economics in terms of particular study and policy objectives, thus emphasizing its pragmatic origins.

Difficulty in giving regional economics a distinct character in terms of conventional economic disciplines also partially explains and is explained by the tendency to place regional economic studies in a broader, interdisciplinary framework. Almost from the beginning, the convention, at least in formal discourse, has been to speak of "regional analysis" and "regional science" rather than "regional economics." Included and welcomed have been such diverse fields as sociology, demography, geography, and history. Indeed, at times, *the* distinguishing characteristic of regional analysis has almost seemed to be its interdisciplinary aspect. This diversity combined with definitional uncertainty was very well expressed by Harvey Perloff in the introduction to the Resources of the Future survey already cited [102, p. v]:

> No fully satisfactory way of classifying regional studies was found—not unexpectedly. Regional studies tend to deal with many features and often involve the use of several academic disciplines. Thus, no general system of classification can be expected to provide self-contained categories; there is inevitable spill-over. The subject classification employed in this report sets up categories whose cores, if not boundaries, are identifiable and whose titles are widely used, with popular and technical meanings that are not too far apart. The topical categories employed are: (1) Physical elements and natural resources; (2) Population and human ecology; (3) Regional economic development; (4) Metropolitan studies and metropolitan planning, and (5) Regional history, literature, and socio-cultural elements. Two other categories were found necessary: (6) Methods and techniques of regional analysis, and (7) Comprehensive regional studies.

A somewhat narrower (but still staggeringly broad!) definition of regional analysis specifically in terms of its objectives has been provided by Walter Isard [70, p. 413]:[1]

> An analyst is perplexed with many problems when he looks at a region. One problem may be to identify specific industries which can individually or in groups operate efficiently and with profit in the region. Another related problem may be to improve the welfare of the people of the region, that is, to raise per capita incomes and perhaps achieve a more equitable distribution of income; the auxiliary problem of measurement of income and of the performance of a society is also present. Still another problem may be to avoid an industrial mix which is too sensitive to the ups and downs of national and world business, and which is com-

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[1] Isard's textbook [70], from which this quotation is taken, constitutes a good single-volume introduction to regional economics and contains excellent bibliographies arranged by subject.

> posed too heavily of old, slow-growing, or declining industries; this is the problem of diversification. Finally, a fourth problem which can be mentioned is to plan industrial development for a region, as part of a system of regions, in an internally consistent manner. . . . [Another] pressing problem . . . and which for many regions is the most critical, is the problem of how to put to best use a limited, if not a niggardly, endowment of resources.

A distinctive aspect of Isard's definition is its exclusive emphasis on what economists would normally construe or recognize as economic problems. This also gives it an immediate advantage as a definitional point of departure for organizing a survey article of this kind.

Still, it is much too broad—at least if the survey is to be short of monograph length and a complete tour of modern economics! Two simplifications, though, suggest themselves: these are to limit attention to (1) problems of regional analysis with unique conceptual characteristics, and (2) specific areas of particularly heavy interchange between conventional economic theory and regional economics.

The conceptually unique problems appear to be mainly empirical in character and center around the difficulties encountered in defining a "region." Traditionally three different approaches have been used in defining regions [21] [22] [34] [102] [119]. The first stresses *homogeneity* with respect to some one or combination of physical, economic, social or other characteristics; the second emphasizes so-called *nodality* or *polarization*, usually around some central urban place; and the third is *programming-* or *policy-oriented*, concerned mainly with administrative coherence or identity between the area being studied and available political institutions for effectuating policy decisions. Naturally enough, regional definitions as established in practice often represent a compromise between these different pure types. In particular, availability and limitations of data can and do dictate departures from "ideal type" definitions in many situations.

Strictly speaking, moreover, the three traditional definitions of regional type are not mutually exclusive. In fact, all regional classification schemes are simply variations on the homogeneity criterion and it is somewhat misleading to suggest otherwise. The only real question is what kind of homogeneity is sought. Thus, a so-called program or policy region is essentially homogeneous in being entirely under the jurisdiction of some one or a few specific government or administrative agencies. A nodal region is homogeneous in that it combines areas dependent in some trade or functional sense on a specific center. Some so-called homogenous regions are homogenous with respect to physical characteristics, like geography or natural resource endowment, while others are defined to be similar in their economic or social characteristics. Finally, homogeneity with respect to statistical compilations, as noted before, may often be the real determinant of regional boundaries for practical purposes.

Some of the most interesting work in regional economics in a purely theoretical or conceptual sense has been concerned with the definition of indexes for measuring homogeneity when more than one dimension or measures of such qualities exist. M. J. Hagood [42] [43], for example, has performed some highly suggestive experiments with the use of statistical factor or component analyses as a means of defining homogeneous regional groupings for the United States in terms of agricultural and demographic variables. In the process, incidentally, she developed some interesting contributions to the clarification of factor analysis procedures and to the development of objective methods for stratifying a statistical sample [44]. Some interesting extensions of this work have also been made by V. V. Almendinger in connection with defining politically and socially homogeneous communities for the New York metropolitan region and Penn-Jersey transportation studies [1] [123].

As interesting as these statistical procedures might be to the applied statistician, econometrician or psychometrician, however, it seems rather doubtful that they are of much tangible value to the regional analyst actually confronted with a problem of defining a geographic area for study. Given the pragmatic, problem-solving orientation of regional economics it would seem that Joseph Fisher of Resources for the Future was probably understating the case when he said [34, p. W-6]:[2]

> I should like to suggest that the most helpful region in many instances is what might be called the *economic development region* . . . [where] . . . the emphasis is on the development of policies, programs and actions to move the region from where it is economically toward pre-determined economic objectives.

The same view is echoed at length in the writings of the most extensive contributor to the regional definition problem, Jacques R. Boudeville [21] [22], although Boudeville also seems to be arguing at times that only a nodal region is useful for policy purposes. Most major exceptions to a policy approach to the regional definition problem will occur, moreover, for the exclusively pragmatic reason of having to make adjustments to data availability.

The problem of regional definition becomes crucial whenever attempts are made to obtain estimates of regional income and product accounts.[3] Such estimates are often essential because policy objectives are commonly set in terms of achieving a stipulated per capita income or production level for a region. The income estimation problem at the regional level, unfortunately, clearly involves more than simply scaling down national income and product accounts. Indeed, many regional analysts would argue that the only way to properly estimate regional income categories would be to adopt a completely fresh approach, developed on the basis of entirely new regional or local sources of information [61] [103] [104]. Among other justifications, a fresh approach is urged on the grounds that the objectives of regional policy planning are so very different from those encountered at the national level that different concepts are immediately needed in the measurement of regional income and product. Some regional economists, moreover, would rather see national income accounts built up progressively from regional accounts than vice versa. At the minimum a very compelling case can be made for keeping reasonably detailed and unaggregated records of local economic activity in order to create data at whatever level of aggregation is required for policy or research purposes [64]. In reality, though, practical considerations, such as limitations on available research resources, usually dictate that regional income estimates be derived from more readily available national income figures.

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[2] A very forceful endorsement of this pragmatic approach to regional definition can also be found in the writings of several others and particularly by E. M. Hoover [64].

[3] An excellent recent survey of the problems of regional social accounting can be found

The special problems of regional income estimation mainly derive from the simple fact that regional economies are almost invariably more open, in the sense of being more reliant on external trade and institutions, than national economies. The type of problem that bedevils the estimator of regional income is aptly illustrated by both the commuter, who lives in one region and works in another, and the large multiplant national corporation. Not only will large corporations typically have installations in different areas but ownership is likely to be dispersed in an unknown fashion over several regions. Government activities, particularly at the federal level, are also difficult to distribute on a regional basis. In general, regional income accounting is complicated by the fact that much needed data are often compiled only on a national basis, and by the conceptual problems associated with defining a resident, the commuter and large corporation being merely two of the more common examples of these difficulties. These difficulties, though, are not insurmountable since regional, state or other local income accounts suitable for many purposes have been or are in the process of being constructed [13] [14] [36] [46] [47] [60] [105] [107].

While regional definition problems do possess several reasonably

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in R. N. Stone [112]. Somewhat earlier but still very useful discussions can be found in [130], particularly W. Hochwald's paper on conceptual issues [62]. For a discussion of some of the same issues but in a "dynamic context" see Isard and G. Freutel [72].

unique elements not encountered in conventional economic analyses, they hardly would appear substantial enough to give regional economics a thoroughly distinct identity. If the search for that identity is directed to locating points of particular conceptual emphasis within regional economics, without much question regional economics, especially as it has evolved and developed within the last decade, uses both location theory and international trade theory more than any of the other conventional economic disciplines.[4] This is hardly surprising, of course, since both location theory and international trade theory emphasize economic relations between geographical areas. It is significant, moreover, that Walter Isard who has fathered much of modern regional economics, spent a great deal of his time in the late '40's and early '50's, when the recent surge of interest in regional economic analysis first began, on the problem of integrating or synthesizing location theory and international trade theory [73] [68]. His particular objective was to correct what he considered to be an intrinsic failing in traditional international trade theory, its failure to pay attention to the cost of overcoming spatial separation.

One common alternative approach to defining regional economics is, in fact, to allude to it as the economics of spatial separation. This, however, would seem to be unduly confining since the policy problems considered by regional economists have been considerably wider in scope than the location problems emphasized by such a definition. Indeed, in actual regional analyses spatial separation as such hasn't been so much the policy concern as the fact that resources, particularly labor, are not completely mobile between regions. The "economics of resource immobility" therefore might be a reasonably accurate, though still less than fully comprehensive, description of regional economics.

Before 1950, which would also be before the phrase "regional analysis" came into common professional usage, what generally was regarded as regional economics was heavily involved in business cycle theory and analysis. Indeed, in the 'forties studies relating to the interregional propagation and transfer of business cycles, those by R. Vining [116] [117] [118] and P. Neff [97] [98] being particularly important, were the dominant concern in regional economic studies. A discussion of this work will be omitted from this paper, however, on the simple grounds that this aspect of regional economics has been rather neglected in recent years.[5] The current emphasis, for better or worse, is on maximization under static conditions, a change almost inherent in the heavy introduction of international trade and location theory concepts into regional economics.[6]

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[4] In light of what has just been said about the difficulties of finding a unique identity for regional analysis, it is perhaps significant that international trade theory shares with regional economics a tendency to synthesize elements from almost all other phases of economic analysis.

[5] Symptomatic of the trend is the omission of cycle studies from Perloff's categorization scheme quoted above.

II. *Problems of Regional Economic Policy*

Modern regional analysis, as reconstituted with a major emphasis on static maximization problems rather than the dynamics of the business cycle, has apparently met a number of desperately felt policy needs. Among other sources, the apparent success in the 'thirties and 'forties of the Tennessee Valley Authority has been a major encouragement and inspiration, both domestically and abroad, to those who would undertake to improve the status of economically backward or handicapped areas by government action, a task to which much of the recent applied work in regional economics has been devoted. In the United States a special interest in regional economics in the older South [63] and New England [53] [54] [89] [23] has been very much motivated by such considerations. To a lesser extent, and with a greater emphasis on growth possibilities not realized rather than on immediate economic difficulties as such, the same is true of some of the interest in regional problems evidenced in the Rocky Mountain area [40]. Similarly, the Upper Midwest Economic Study, recently inaugurated at the University of Minnesota and already reporting research results [58] [77] [79] [110], has as one of its avowed aims formulating an "action program" to raise the region's rate of economic progress to at least parity with the rest of the country.

Interest in regional economic problems long has had a special source of stimulation in the United States because of the deliberately (and perhaps artificially) regionalized organization of the Federal Reserve System. Thus it is not surprising that much, perhaps even a preponderance, of the domestic empirical work in regional economics has been sponsored at least partially by Federal Reserve Banks. The already mentioned Upper Midwest Project and G. Freutel's work on interregional payment balances [36] are just two of the more important examples.

Much of the foreign interest in regional economic growth has had a similar source but with a special emphasis on a variation of the balanced growth problem. In many countries experiencing rapid economic development a not uncommon circumstance is the existence of a sharp disparity of fortunes between newly industralized urban areas and the rest of the country. An attempt is often made to ameliorate the political strains created by this so-called "economic dualism" by initiating special regional development programs. The efforts in Italy to expedite development of the Mezzogiorno, in Venezuela of the Guayana, in Brazil of the Amazonian North, in Argentina of Patagonia, in Chile of Aisen Province, are just a few examples of efforts to meet such political problems by either opening up new territory to settlement by economically oppressed groups or directly transferring more economic opportunity to such groups. Of course this same problem of balanced growth and attempts to effectuate income transfers to unfortunate areas are not unknown in the industralized parts of the world either;[7] the U.S. government worries about West Virginia, the French about Brittany, and the English about Glasgow. In fact, one of the postwar period's earliest and most ambitious schemes to geographically redistribute economic opportunity was undertaken by the British with their "New Towns Plan," the evaluation of which provides some of the better reading on problems of economic policy formation in regional economics [108].

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[6] Also eliminated from consideration here is the extensive work done in recent years on the development of benefit-cost ratios and similar measures of capital-investment effectiveness for water resource development. While this work has been of considerable interest to regional economists and closely related to certain aspects of their work, these contributions to capital budgeting procedures would appear to belong more to the realm of general capital theory since they possess no particularly unique regional characteristics.

Actually, urban problems and analyses have assumed an increasingly dominant role in empirical research in regional analysis in the United States. To a certain degree this has been a function of simple financial support since the interstate highway program made substantial funds available for research activities associated with the highway construction. The nature of this support has had, moreover, some interesting effects on the direction of regional research efforts.

Many early research expenditures made under the interstate highway program were primarily concerned with making urban traffic forecasts and estimates. Two techniques usually were employed for this purpose. The first was the highly expensive one of making origination and destination counts by going to homes or stopping automobiles on the highways and performing interviews, and by making controlled cordon counts of the number of vehicles and persons passing certain points in the city. When interviews were performed, information was sought not only on origins and destinations but also on trip purpose (e.g. whether for work, recreation or shopping) and a record was kept of the mode of transportation employed. The main objective was to estimate the level of demand for highway capacity at different points within urban areas so as to determine the needs for new capacity and to locate the capacity with, it was hoped, maximum effectiveness.

Design of these origin and destination interviews obviously poses few conceptual or empirical problems of interest to economists, though many possibilities for improvement do lie in the province of statistical sampling procedures. Origin and destination studies by interview tend, however, to be highly expensive and a full census of originations and destinations almost invariably was impossible. To fill the gaps a second estimation technique, known as gravity model analysis, normally was employed. Gravity models are based on simple sorts of analogues to physical models and they have had a long and controversial history in sociological analyses of regional characteristics [113] [124] [121] [26]. For gravity models in urban transportation analyses the traffic between two points is hypothesized to be positively related to the "mass" at each point and negatively to the "friction" operating on travel between the points. Friction can be measured in terms of distance, time, cost, and various other factors. Similarly, mass has been variously defined as population, the number of automobile owners and, in some more sophisticated models, as purchasing power or effective demand or even "potential" commercial or industrial "drawing power" (as reflected in retail and industrial employment or other measures of such activities).

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[7] A good summary of the sources of this problem and possible policy solutions can be found in A. H. Conrad [31].

When broadened in this fashion these models pose economic questions of some empirical interest and significance; without such broadening, of course, the models are rather puerile and mechanistic and devoid of behavioral content. It is not surprising, therefore, that much of the effort to broaden and improve gravity model applications to urban traffic analyses is traceable to the influence of economists who have worked on various urban transportation studies. Particularly pathbreaking in this respect were the transportation studies directed by J. D. Carroll for Detroit and Chicago and various papers that developed from those studies [125] [126] [24] [25]. An effort to incorporate economic and other behavioral information into the making of traffic projections also has been made as part of the Pittsburgh area transportation study [129] and the concurrent economic study of the Pittsburgh region conducted by the Pittsburgh Regional Planning Association; particularly interesting in this regard is a paper by I. S. Lowry containing a number of suggestions for improving gravity models [87].

Considerably more revolutionary and experimental have been the efforts made in the Penn-Jersey transportation study to project future population distributions and thereby transportation demands by assuming that residential choices are made on the basis of certain rational economic considerations as embodied in a linear programming model [59] [48-52]. As will be explained in greater detail later, some critics would argue that with this linear programming model the pendulum has swung too far in the direction of letting economic considerations determine the projections. Be that as it may, it is clear that economists and regional economics are now thoroughly involved in urban transportation planning.

The interest of economists and regional analysts in urban problems has not been limited, of course, to transportation planning. The interdependence between urban transportation, land use, population growth patterns, and industrial development generally is considered to be so close that more purely economic studies of urban trends and patterns have become increasingly prevalent. Besides the already cited Pittsburgh Regional Planning Association effort, there was at an earlier date the New York metropolitan region study (which also included historical and political studies). Indeed, the reports growing out of this study form an excellent introduction to an understanding of urban economic problems [19] [28] [45] [55] [65] [81] [106] [109] [115] [123]. Similarly, the Upper Midwest study includes a special inquiry into the problems of the Twin Cities' metropolitan area. The new professional interest of economists in urban problems is, of course, symptomatic of a great expansion of concern about urban problems in general in U.S. society.

Transportation problems have also supplied a policy impetus to regional analysis in a larger geographic context than the urban area. Private intercity railroads, trucking lines, and local service airlines in the United States happen, for a number of historical and other reasons, to be regionally structured. To a lesser extent the same is true of the large commercial trunk airlines. Furthermore, recent "crises" in U.S. transportation have been positively correlated with railroad passenger volumes, short hauls (of both passengers and freight), and small volume shipments. These characteristics vary sharply from region to region with New England clearly being the most disadvantaged.

In general, regional analysis apparently has filled a void by developing tools applicable to economic planning problems at a time when economic planning has been increasingly in favor in many circles and goverments. Thus, the great strength or appeal of regional analysis would appear to be its essentially pragmatic character and, in particular, its willingness to integrate theory and data and to undertake empirically difficult analyses.

III. *Theoretical Foundations*

This emphasis on integration of theory and empiricism also shows itself in modern regional analysis' theoretical roots. These, in large part are "borrowings" of developments in general economics that occurred during the 1940's and can be listed under four headings: (1) a revitalization of location theory, particularly as contained in Lösch's work, published in English in 1953 [83] but available in German in the 'forties; (2) international and interregional multiplier theory as illustrated by the work of Metzler [88], Goodwin [41], Chipman [30], and others; (3) Leontief interindustry input-output analysis [83] [84]; and (4) mathematical programming.[8]

1. *Location Theory.* With the possible exception of Löschian location theory, these theoretical developments all share the important quality of having obvious empirical possibilities. Indeed, one could say that they almost beg for empirical implementation. By contrast, Lösch's theory not only is highly idealized and stylized but has few immediate or obvious empirical possibilities and has thus far been devoid of any important empirical implementation. One plausible (but not necessarily correct!) explanation of its appeal in regional analysis is that it partially satisfies the search for an identity in the new field. Location theory had, and has, been generally overlooked in general economic studies and by giving it special prominence, regional analysts partially distinguished their activities from the rest of economics. Furthermore, Lösch's theory still may prove to have empirical possibilities, as M. Beckman has forecast [16]. At present, however, the more eclectic and general, though less logically elegant, revisions of Weberian location theory developed by Isard [69] and largely anticipated by E. M. Hoover [64] would seem to have more practical utility.

2. *Multiplier Theory.* Interregional multiplier analysis, as an empirical device, is closely bound up in regional economics with the concept of an economic base. Furthermore, regional economists deserve at least some credit for pioneering the application of multiplier concepts. Homer Hoyt, a pioneer of urban economic studies, employed the concept of an economic base with its implicit multiplier concept as early as 1937 [127]. Revisions and improvements of the base concept have been stimulated, of course, by subsequent development of multiplier concepts in general business cycle theory.

The essential notions of the economic base-multiplier concept as applied in regional studies are the same as similar concepts used elsewhere in economics and are quite clear and simple, though subject to a number of modifications, qualifications or adjustments to provide greater meaningfulness or sophistication in actual application.[9] The first step is to define certain activities as being exogenous or determined outside of the economy under analysis. Translating conventional international trade theory into regional terms, the "economic base" of a region is that group of industries primarily engaged in exporting from the region under analysis to other regions. An empirical multiplier is determined by observing the historical relationship between this export activity and total economic activity in the region. This empirical multiplier is then applied to estimates of economic base to forecast total economic activity.

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[8] For bibliography and discussion of the literature on mathematical programming see [15] [32] [33].

[9] A comprehensive survey of the history, applications and concepts of economic base studies will be found in R. B. Andrews [3-12]. Other useful discussions of the base concept can be found in [66] [114].

A crucial aspect of the procedure is, of course, estimating the size of the economic base. In particular applications where the concern is so often with trying to improve the economic status of a specific region, attempts often are made to estimate the extent of new export activities that might have a chance of economic survival, thus creating a base for expansion in the general economy of the region under analysis. The usual procedure for defining new export possibilities is to make some sort of interregional production cost comparisons, and a common technique of doing this in regional analysis is to perform a so-called "comparative cost analysis" [70, pp. 233-44].

While based on ideas akin to those found in international trade theory, comparative cost analyses as applied in regional economics are different in that the cost comparisons usually are made directly and in absolute terms without reference to calculations of internal comparative advantage. Specifically, the comparisons normally are made strictly between regions on the apparent assumption of some sort of interregional market equilibrium existing in factor prices. Thus the procedure usually devolves to doing a set of cost comparisons for supplying certain products to specified markets from the given region as compared with alternative sources of supply for these same markets. The first step in the analysis is to look at factor or input costs that are particularly important or almost necessarily different between regions; thus transport costs are almost always analyzed and usually labor costs or costs of a peculiarly important input (e.g. power for aluminium, oil or gas for petrol-chemicals) are also analyzed.

The great virtue of comparative cost analysis is that of all partial analyses, empirical simplicity. It also suffers from the usual disabilities of partial analyses in that the needed preliminary simplifications may not be obvious or easily ascertainable in complex interdependent situations. Particularly likely to be ignored are such considerations as economies of scale, factor price changes induced by inelastic supply functions, and external economies attributable to urbanization or similar influences.

An attempt to correct many of these deficiencies in comparative cost analysis has been made by Isard, E. W. Schooler, and T. Victoriez with what they call "industrial complex analysis" [70, Ch. 9] [74].

The basic innovation of industrial complex analysis is a somewhat more sophisticated attempt to define meaningful industrial groupings or complexes for cost comparison by using input-output matrices. Also involved is a broadening of the considerations permitted to influence the cost comparisons, with particular emphasis on tests for the existence of scale economies.

The strongest objections usually made to economic base or multiplier analyses in regional economics are not concerned, however, with the legitimacy or illegitimacy of the cost comparisons by which the base is estimated. Some of the most vehement opposition to base analyses has been concerned with the question of simply finding a suitable definition of what is exogenous. Base analysis implies that a gap or discontinuity would be found if industries were arranged hierarchically according to the level of their export activity. However, in many cases where such an analysis has been undertaken, the ordering has been found to be essentially continuous.

Furthermore, even if a group of export industries can be identified readily, there are often reasons for denying that the multiplier coefficients will be stable over time. In fact, a very common objection made to base analysis is that regional studies usually are concerned with economic development of an area and one of the main objectives of that development is to change the relationship between export and import activities. It is quite obvious, moreover, that an economy can exist without exports and can grow without a growth of its exports, as must be true for the world economy taken as a whole. Quibbles also have been entered, sometimes quite legitimately, about the reliability of multiplier coefficients estimated from historical data.

While the economic base concept is thus subject to many serious reservations and objections, it still has had a most remarkable vitality. Indeed, some kind of economic base notion is to be found in almost all regional projection exercises, at least implicitly.[10] The substantive issue is really not whether a base or exogeneity concept is to be employed but how much openness or interdependence will be tolerated. Unquestionably, the remarkable durability of the underlying idea of exogeneity embodied in the base concept is simply that no other empirically implementable alternative now exists.

3. *Input-Output Analysis.* This is even true of the second applied technique of great importance in regional economics, input-output analysis and its closely associated matrix multiplier concepts. Even in these more sophisticated and interdependent models some choice usu-

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[10] A persuasive argument has been advanced by D. C. North [100], in fact, for defining regions, at least for purposes of historical analysis, so that they have homogeneous export bases.

ally must be made of an exogenous bill of goods to be entered as "final demand." In broad, idealized outline, the objective of an interregional interindustry input-output analysis is a matrix of input-output coefficients identified not only by industry but also by geographic areas or regions [70, Ch. 8] [67] [92]. Thus, if there were 50 industries and 5 regions, the ideal matrix would contain 62,500 coefficients decomposable into 25 submatrices of 2500 (50 × 50) entries each; for each region there would be 5 matrices, one of which defined its own interindustry relationships and 4 relating to trade relationships by industry with the other regions. Such an idealized table, however, is usually impossible to realize because of data insufficiencies. A central preoccupation of regional input-output analysis has therefore been the making of adaptations to overcome these data difficulties, and most of these adaptations involve aggregation aimed at eliminating some of the empirical detail needed to estimate the idealized table.

One obvious simplification is to treat each region as if it were an *almost* autonomous economc unit and proceed as with the estimation of an input-output table for a national economy by consolidating all inflows and outflows to other regions into an import-export sector. What this essentially amounts to is an aggregation over all rows and columns in the idealized matrix outside of the particular region of study, a record being retained mainly of the interindustry relationships within the region under study. This is probably the most common approach now taken to the estimation of regional input-output relations; two good illustrative examples of such tables can be found in the work of W. Z. Hirsch [60] and R. Artle [13].

A second approach, rather opposite in its aggregation procedure, is to completely forget about interindustry relations and concentrate on interregional trade patterns. This essentially amounts to aggregating over industries but not over regions. An illustrative example of such an approach is contained in the procedures proposed for the Upper Middle West study [58]. This procedure has the great advantage that interindustry coefficients for individual regions are usually very hard to come by, not uncommonly being estimated by simply assuming that the national coefficients apply within a region. Furthermore, an interregional trade-flow approach retains the emphasis where it usually is needed for policy purposes.

A third simplification is to define interregional trade coefficients for each commodity as an input, forgetting about interindustry differences in import patterns within each region. Thus, instead of having separate import input coefficients for each region and industry, a trade coefficient is constructed for each region by a particular type of input. This essentially reduces all the interindustry trade relationship submatrices between the specific region and regions other than itself in the idealized model to a single vector for each region. For example, if there were 50 industries in the model, instead of having 50 × 50 matrix of coefficients to define trade relationships between one region and another, there would be instead only a 50-element vector. The essential assumption lying behind this procedure is that if imports of a commodity are needed as inputs by industries of a given region, exactly the same relative imports of this commodity will be needed for all industries in the region. The aggregation is essentially over trade input coefficients since industry destination of imported inputs is ignored. One great advantage of this scheme is that it is empirically implementable with available Interstate Commerce Commission data on commodity flows. Illustrative examples of this approach can be found in the work of H. Chenery [27] and L. Moses [93].

A fourth approach to data simplification for interregional input-output models is the balanced regional growth model developed by Leontief and Isard [84, Ch. 4, 5]. It is based on the notion that a hierarchical arrangement or definition of industries is possible in which certain industries can be described as basically catering to national markets, and others to regional or local markets. The input-output analysis then proceeds according to this hierarchical ordering with activity levels for the national industries being estimated first. These national industry outputs are then allocated to the different regions according to regional participation coefficients to form part of the final demand for each region, the remainder being regionally produced commodities. Following this, the regional composition of interindustry activity can be estimated by conventional input-output methods; in fact, national interindustry relationships have usually been suggested for this exercise. The process can be carried down, of course, to as low a level of geographic disaggregation as data permit and objectives might require. As for applications, the New York metropolitan region study used a rather heuristic variation of this balanced growth approach in arriving at its projections [19] [115].

A fifth and most ingenious approach to data simplification in regional input-output analysis has very recently been advanced by Leontief and A. Strout [85]. The essence of the method is to use gravity-type structural equations to explain or estimate the magnitude of interregional flow. Specifically, the flow of a commodity from one region to another is assumed to be directly proportional to the product of its total output in a shipping region by its total input in a receiving region divided by the aggregate amount of the commodity produced and consumed in the entire economy, all multiplied by an empirical constant. (Leontief also permits every region to be both a shipper and receiver of a commodity which at the level of aggregation now employed in input-output he properly argues is a more realistic assumption than hypothesizing the economically rational solution of only one-way flows.) The constants can be statistically estimated by various methods or, what is most important from the standpoint of data conservation, by solving the equations of the regional input-output system, including the gravity-flow equations in the system. If this latter direct or "exact" solution, as Leontief calls it, is obtained, the existence of the balance equations inherent in input-output systems makes it possible to estimate the interregional flows without directly observing them; the only data needed are those on total outputs and inputs for all regions and the use made of each commodity as an input to internal production in each region. The crucial question, of course, is how well the flow estimates generated by use of the hypothesized gravity-flow structure approximate reality. In this regard initial experiments have been most encouraging. In fact, in tests on bituminous coal, cement and steel shapes the "exact solution" has performed about as well as least squares on projections and in some senses even better.

Input-output techniques are subject, of course, to a number of criticisms since they embody so many important and controversial simplifying assumptions. These are so well known, to users of input-output as well as its critics, and so extensively catalogued elsewhere [91] [93] [128] that no repetition will be made here. Suffice it to say that regional input-output models tend to have all the problems of national models plus some additional ones of their own. In particular, there is at least some limited evidence that interregional trade coefficients may be even more unstable over time than interindustry coefficients [93]. Such instability almost surely applies as well to the regional participation coefficients used in the balanced regional growth model.

The fact still remains that with all its problems and difficulties input-output does have the great advantage of being an empirically workable model that provides an organizational framework and set of consistency checks that are difficult to achieve with less formal techniques. The danger does exist, though, that preoccupation with the empirical detail involved in establishing these models may lead to an oversight of importance, perhaps of such importance that it leads to grossly inaccurate estimates. Thus, it is often argued that an understanding of basic historical trends and forces is more essential to making good projections than accurate interindustry and interregional trade coefficients. Still this is not an intrinsic shortcoming of the model as such. Rather it is a question of what constitutes a proper allocation of research resources in regional analysis and, as will be observed in the next section, this is a question very much in dispute. Finally, it must be recognized that input-output and economic base analyses, with all their shortcomings and deficiencies, are the tools almost invariably relied upon at the present time when actual empirical work in regional economics must be performed.

4. *Mathematical Programming.* The fourth basic theoretical tool commonly employed in modern regional analysis, mathematical programming, is without question the best from a strictly conceptual point of view if one believes in a reasonably pervasive economic rationality.[11] It suffers, though, from two serious handicaps. The first is that of simple data availability; the data requirements for a good linear programming model are often staggeringly large and even farther beyond what is currently available than the requirements for input-output. The second difficulty is that economic rationality is often either not obviously pervasive or, more accurately, is too complex to be readily incorporated into presently operational programming models.

The most ambitious effort to date in the use of linear programming techniques in a regional project has been the Penn-Jersey study's household location model, and their experiences illustrate both basic difficulties [59] [48-52]. They started with the highly appealing and simple notion that different types of households have certain specific amounts that they can budget for "the bundle of services" associated with a particular type of house. Included in the definition of housing type was the amount of land consumed so that the selection of different housing types gives rise to different land-use patterns. Families differently located in the income scale and with different numbers of children or other characteristics are, moreover, hypothesized to have different willingnesses to pay for specific types of housing as expressed by the size of their budgets available for each housing type. Later the model was modified to make the budgets not only specific to housing types but also to areas on the grounds that it is more realistic to assume that families consider the residential location and its amenities as well as the structure and size of yard when deciding the amount that they are willing to pay for a particular house.

Still another modification, introduced in order to effect a considerable reduction in the computational burden, was simply to preselect the housing type in every geographic area that seemed most suitable for each family type in that area on the grounds that this selection was usually obvious without running the model. After the budgets had been estimated for each family type in each area and for every housing type, all costs for each housing type and location *not* associated with land acquisition were deducted from the budget estimate to determine the net rent-paying ability of each family for each housing type in a specific location. That is, transportation, structure, and amenity (schooling, public services, etc.) costs were subtracted from the total available budget to determine how much was left for land acquisition or rent. Households were then hypothesized to compete with one another for the available land, bidding rents up to a maximum (the objective function) subject to the constraints that all households get located and that total land use does not exceed total land available. One immediate advantage of this research design is that it yields an estimate of the rent surface as an output rather than being required as an input, a considerable gain since urban land values or rents are difficult data to obtain.

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[11] The conceptual aspects of applying mathematical programming to location and regional analysis problems are discussed by T. C. Koopmans and M. Beckman [78], Beckman and T. Marschak [16], L. Lefeber [80], B. H. Stevens [111], and L. N. Moses [94]. Actual applications of such techniques are reported by Henderson [56] [57], F. T. Moore [90] and K. A. Fox [35].

The striking thing about this model is the degree to which the important behavioral considerations are built into the model prior to its actual running. This immediately raises at least a research resource allocation problem of whether it is wise to devote considerable effort to building such a model until the reliability of its inputs has been established. Particularly important among the prior questions are estimating the budgets by household type and location and choosing a particular housing type in each area for each family. Indeed, this choice of particular housing type in each locale is crucial in determining the actual land-use pattern. For example, if in the preselection process, housing types are chosen so as to yield the greatest per-acre rent-paying ability, a bias is built in toward finding high density land-use patterns. The converse is true if the housing yielding the greatest per-family rent-paying ability is preselected. In recognition of these difficulties, the Penn-Jersey group are now considering the preselection of housing types so as to provide the greatest per-family net rent-paying ability as determined by subtracting a prior estimate of the initial rent surface from the budget along with the other cost estimates. They would then proceed as before, maximizing rent-paying on a gross basis. This scheme, of course, immediately loses some of the advantages of not needing a prior rent surface but this deficiency can be at least partially overcome by iterating the model a few times if the rents estimated by the model differ substantially from the rents assumed a priori.

Purists, of course, might still raise a number of quibbles about the behavioral assumptions incorporated in this model even as modified. It is, however, a highly imaginative and heroic attempt to apply the techniques of mathematical programming to an actual location problem, and by any standards it is the most ambitious effort of its kind yet undertaken in regional economics. As such, its trials and tribulations are surely suggestive, and it is difficult to escape the conclusion on the basis of that experience that programming may still be beyond available data and research resources, at least given the present paucity of prior or established knowledge about urban economics and location patterns. It is difficult, on the other hand, to conceive how the necessary knowledge and insights needed for adequately implementing these models will ever be acquired without such experimental undertakings.

IV. *Types of Approach to Regional Studies*

It is probably apparent by this point that if the important issues associated with data collection are put to one side, the basic disagreements arising in the application of regional analyses tend to be as much a matter of viewpoint or philosophical orientation as of technical detail. Specifically, two rather different approaches are discernible. In one the emphasis is on historical and behavioral characteristics; in the other the orientation is more toward quantification, forecasting, and the development of a logically rigorous framework for the analysis. To be sure, these distinctions are at least somewhat arbitrary, as most such classifications usually are. Thus, those with an historical-behavioral inclination also often try to make quantitative forecasts and those with an analytical-quantitative approach normally will try to develop an understanding of underlying behavioral patterns. Furthermore, one of the dominant concerns in the field at the moment is with finding means of fusing or synthesizing the best elements in different approaches into better, more comprehensive procedures [70, Ch. 12] [71].

Still, very important differences in emphasis do exist. For example, those with an historical-behavioral orientation tend to stress the analysis of trends and evolutionary patterns. They try to "dig beneath" the data to an understanding of motivations, particularly those leading to significant changes in structure and conduct. Thus their concern tends to be more with structural change than with its continuity. They are not too disturbed, moreover, if this structural change makes forecasting hazardous or uncertain or if their forecasts are not formally rigorous and consistent in all details. Their involvement tends to be more with whether what they have said and forecast is plausible in light of current or apparent trends in behavior. In keeping with this, they often will emphasize such considerations as entrepreneurship, market structure, and external economies that normally are not conveniently incorporated into the more formal analytical frameworks.[12] Finally, they are often quite willing to sacrifice a good static analysis even for an admittedly poor dynamic analysis, for as one proponent of this point of view, B. R. Berman, has so eloquently put it [18, p. 300]:

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[12] For a further expression of these views see B. Chinitz's paper [29] on the implications of market structure and industry characteristics for regional development.

> It may be argued that dynamic models are harder to construct than static, or that we cannot begin to fashion dynamic models until we have a static model of some believability. But for practical purposes . . . a crude dynamic model may be better than a highly tooled, multi-jeweled static creation.

Two leading prototypes of regional analyses with this emphasis on behavioral considerations are the New York metropolitan region study and, to a lesser extent, the Pittsburgh area economic study.

By contrast, those of the analytical-quantifier persuasion will emphasize formal structure and consistency. Far from being fascinated by structural discontinuities, they often will seek to establish empirically that behavioral instability, at least of sufficient impact to modify their structural parameters, is more the exception than the rule. They will be interested in developing relationships to explain structural change only if confronted with obvious necessity. Similarly, they are usually willing to sacrifice some dynamics to obtain better static models. They are also likely to be much more concerned with problems of regional income and social accounting. A good one-sentence summary of the beliefs held by those adopting this more formal approach was once rendered by Isard in defense of input-output procedures as follows [70, p. 341]: "When combined with intuition and hunch, input-output projections yield results at least as good as those based on intuition and hunch alone." Most of the work with regional analysis in Europe and in underdeveloped countries displays these more formalistic tendencies [82] [70], and the Penn-Jersey study with its emphasis on a linear programming approach is surely of this kind. The Upper Midwest study, on the other hand, represents a very interesting compromise or hybrid of the two different basic approaches.

Some of these differences in approach are traceable to and explained by differences in policy orientations. The behaviorist seems to look to the making of piecemeal adjustments in existing institutions and public policy arrangements as the soundest procedure. He tends to formulate less ambitious goals and to think in terms of a series of short-run policy adjustments over time. This results in his emphasizing flexibility and adaptability. As R. Vernon stated when summing up the New York metropolitan study [115, p. 196]:

> No projection of the economic and demographic characteristics of a metropolitan area can be free of the risk of error; no public or private planner can afford to assume that the potential error is small. From a policy viewpoint, this may suggest that planners and investors should

> regard the preservation of flexibility as a virtue in itself, a virtue worth paying for at the seeming sacrifice of other standards of performance.

By contrast, those with the more formal outlook are oriented more toward drawing up comprehensive development plans. This at least partially explains the popularity of this approach in underdeveloped countries confronted with long-range problems of considerable magnitude and often facing a requirement to justify loans for large-scale development investments before world banking and development authorities. Indeed, an emphasis on long-range, comprehensive planning apparently done to the best of current technical capabilities has an almost inherent and undeniable appeal, and perhaps the appearance of indispensability when undertaking capital budgeting decisions. Something of the ambition and confidence in policy matters of those advocating a more formal approach to regional analysis is connoted in the following statement by J. R. Boudeville [22, pp. 11-12]:

> Regional Economy is a science of decision. It presupposes the determination of aims, the use of means and the choice of the most effective instruments to achieve those aims. . . . The problem of bringing to light the interrelations of the economic trends and the coherence of the means and aims requires the presence of a regional command familiar with the new techniques. . . . Admittedly, excellent regional schemes have been carried out without the aid of modern techniques. The Roman aqueducts and Gothic cathedrals did not wait for Strength of Materials calculations to be architectural feats. Similarly, we can admire the work of the pioneers of our time. Nevertheless, the role of the economist is to produce general rules superseding the old empirical approach and piecemeal work, which is always expensive.

The obvious, appropriately evasive comment for any reviewer attempting to maintain at least an appearance of neutrality when confronted with such contrasting viewpoints is, simply, that each approach probably has a role. In this case, moreover, such a statement is not only diplomatic but probably correct. Structural coefficients will be relatively stable in many circumstances while not in others. Even if not stable, moreover, structural estimates often will be required just simply to *begin* a rational analysis. However, if the initial analysis is not followed by studies aimed at understanding the patterns of change, the resulting forecasts and policy decisions are likely to be next to, but not quite, worthless. Furthermore it does seem rational to build flexibility into the decision process *but* in proportion to the degree of uncertainty attached to the crucial structural estimates used in making initial decisions. This degree of uncertainty also may very well vary for different aspects of the decision process and these differences too should be reflected in the degree of flexibility retained. In short, decision-making for economic policy is still a very crude science and it behooves the cautious man to be eclectic.

Whatever the orientation or preferences adopted on these issues, it seems rather difficult to deny that at the moment regional analysis tends to be somewhat stronger in the formulation of analytical frameworks than in fundamental understanding of any behavioral regularities at work shaping regional and metropolitan growth patterns. Particularly notable for their scarcity throughout the 'fifties were quantitative studies aimed at hypothesis-testing that were so common in most other fields of economics at that time. Indeed, ever since the interest in interregional analysis of business cycles diminished in the late 'forties, there has even been a relative lack of suggestions on hypotheses to be tested. Rather, the conceptual activity in regional analysis has been concerned mainly with the construction of logically consistent schemes for organizing and presenting data, particularly social accounts, with a secondary emphasis on normative policy prescriptions. Many of the hypotheses that have been developed from the theoretical work, moreover, have either been at a highly aggregative level, like those embodied in many economic base multiplier concepts, or have been such obvious abstractions as to not easily elicit serious statistical study, like Lösch's location theory. Accordingly, the work of those with an historical-behavioral bent at least serves the extremely useful purpose of beginning to fill this hypothesis void. Furthermore, this work illustrates the important point that bringing to bear certain aspects of conventional economic theory and thought now often ignored in regional economics can be highly productive of new insights and hypotheses in this field.

Further verification on this point has been provided recently by three excellent efforts [2] [96] [122] to develop formal models defining relationships between urban land-use patterns and explanatory economic factors. These models not only give an organization to the discussion that was badly needed but yield eminently testable hypotheses. Indeed, the only criticism that might be made is to regret that the authors haven't yet proceeded on to the testing, especially since the process probably would have sharpened the hypotheses. These theories all place almost exclusive emphasis on economic variables, like relevant prices and costs or proxies for costs such as "elapsed travel time" for access to places of work or other centers. It would seem highly probable that a number of sociological variables, like those commonly encountered in cross-section consumer budget studies, are required for a really adequate empirical explanation of locational choices. Of particular importance would appear to be variables defining family characteristics (e.g., number of children and employed individuals) when seeking explanations of residential choices.

The importance of these family-characteristic variables has been demonstrated, in fact, in results obtained by J. F. Kain [75] [76] in a study of Detroit residential location patterns based on data collected as part of the Detroit area transportation study. Kain, in collaboration with J. Niedercorn [99] and others, has also been testing other hypotheses about urban location patterns as part of the RAND study of urban transportation and land use. In this study the emphasis is on the relationship of urban transportation to metropolitan growth patterns, although other determinants of urban growth are also being investigated. The studies are, for the most part, formally econometric in character and based as much on intercity as intracity comparisons, thus taking advantage of the fact that the RAND project is liberated from the geographical and policy constraints that often have inhibited effective hypothesis testing in specific regional studies. The plethora of available cross-section data based on intercity samples makes it possible to entertain a considerably widened range of hypotheses and tests with these data.

Of a somewhat similar character are a series of recent studies on urban transportation characteristics recently completed by the Northwestern University Traffic and Transportation Institute [95] [101] [120]. These studies, among other contributions, embody the first really substantial attempt to estimate demand and cross-elasticity parameters that are essential to intelligent formulation of urban transportation policies. The evidence produced is not, incidentally, comforting to those who believe that creation of more and better public transit, by government subsidy if necessary, will relieve automobile congestion in urban areas by attracting commuters away from their private automobiles. The cross-elasticity of demand between private auto and public transit commutation would appear to be so low that actual payments might have to be made to transit riders to induce any considerable shift in patronage.

That regional economics will benefit from the confrontation of hypotheses with actual data is also suggested by the results of those few quantitative studies that have been directed to the testing of hypotheses in regional economics. These tests, incidentally, need not be formidable or involve formal econometrics to have a high yield. For example, much of the New York metropolitan region study's activities involved the formulation, reformulation, and testing of hypotheses against available data, though only occasionally with formal statistical procedures. The tests were formal or consistent in an economic sense, however, since they were unified by continual reference to the basic hypothesis that underlaid the entire project, namely that New York is what it is because of its peculiar attraction for "external economy industries" that "have a compelling need to be close to other firms in order to make sales or hold down costs" [115, p. 6]. The model is "closed," moreover, by an ingenious historical explanation of why New York has developed a particularly advantageous position in these external or agglomeration economies [115, Ch. 2].

These same traditions of combining historical and economic explanations and reformulating hypotheses as experience and data accumulate seem to have been perpetuated in the Pittsburgh area economic study, which has some of the same personnel and leadership as the New York study. In this study, though, the emphasis seems to be more on the special role played in regional growth by natural resource endowments; furthermore, the highly interesting hypothesis has been tentatively advanced that these resources, by inducing the growth of heavy, large-scale mass-production industries with a technological bias in their management requirements, have indirectly prevented Pittsburgh from realizing the same agglomeration economies for its size that other cities with more diversified, small-scale industries have experienced [29].

An orientation to hypothesis-testing is also evident in the background studies being prepared as part of the Upper Midwest study. These background studies, incidentally, are justified in the general research design of the main project both for their own sake and as a source of information on changes in coefficients needed to make desired long-range forecasts. The early focus in this study, for quite understandable reasons, has been on the recent technological revolution in agriculture and the implications of this revolution for labor-force migration. For example, L. A. Sjaastad in a highly interesting study of migration patterns in the Upper Midwest tests the hypothesis that since "a good part of what we consider human capital is an accumulation of skills and experience specific to an occupation . . . income differentials could persist over long time periods even in a market system with no lags or imperfections save lack of perfect foresight" [110, p. 45]. His preliminary empirical evidence on migration patterns by age groups and geographic areas within the Upper Midwest lends, moreover, considerable support to the hypothesis.

This very basic question of resource mobility also has been studied empirically by W. H. Miernyck [89] and G. H. Borts [20]. Miernyck, in an interview study of displaced New England textile workers, also finds that the problems of adaptation to technological change tend to weigh most heavily on older workers and are far from being automatically self-correcting, at least in the short run. In fact, Miernyck finds that re-employment in the textile industry is about the only real hope for many older unemployed textile workers in New England because of their high degree of skill specialization; and such re-employment opportunities appear only slowly in a declining industry.

Borts considerably broadens the analysis, explaining the persistence of regional wage differences in terms of regional differences in capital movements, birth rates, the marginal efficiency of new investments, and export industry composition, as well as any residual labor immobility. Borts also presents a more refined analysis of the interrelationships between wage differences and labor and capital movements. He builds a strong case for the hypothesis that capital movements, exogenously induced by an expansion in demand for the particular industries or exports of certain regions, have been the principal factor sustaining interregional wage differences in the United States.

Support for at least part of this hypothesis is also provided by a study of changes since 1929 in the distribution of manufacturing activities in the United States by V. R. Fuchs [37] [38] [39]. A major conclusion is [38, p. 177]:

> The most important redistributions occurred in labor-oriented industries such as textiles and apparel, or in industries oriented to natural resources such as chemicals, lumber and paper. The single most important locational development since 1929, the growth of the aircraft industry in the Southwest, is probably attributable more to climate than to any other factor.

Accordingly, Fuchs, like Borts, stresses the importance of exogenous factors in shaping regional growth characteristics. Indeed, a prime objective of Fuchs is to reject the hypothesis that locational changes in manufacturing can be explained endogenously by shifts in local market industries following demand into regions with newly expanded populations. In short, Fuchs tends to see changes in manufacturing locations much more as a cause than as an effect of population shifts.

A somewhat different view of these causal relationships is to be found in a study by H. S. Perloff, E. S. Dunn, E. E. Lampard, and R. E. Muth [105] on the relationships between regional growth, incomes, and resources. These authors who have something to say (historically, conceptually, and empirically) on almost every aspect of the regional income differentiation question, guardedly adopt the view that for manufacturing "the tie to resources has not been dominant" while access to "terminal markets" deserves heavy emphasis as an explanation of locational shifts [105, p. 394]. The discrepancies between the findings of Perloff and associates and those of Fuchs are at least partly attributable to differences in definitions and choice of analytical procedures. Specifically, Fuchs chose to analyze the location of market-oriented industries directly while Perloff and colleagues put a major emphasis on the direct analysis of natural-resource-using industries; furthermore, they define natural resources in the conventional fashion, thus excluding climate and low-cost labor, two factors which play a very prominent role in Fuch's explanations.

Perloff, Dunn, Lampard, and Muth also argue that capital flows have not been and probably will not be in the future sufficient to quickly eliminate interregional differences in wages for similar skills. Outmigration of labor is therefore seen as required to achieve interregional equilibration of incomes, and this migration has occurred at such "painfully slow rates" that they favor positive governmental action to alleviate the difficulties. They clearly suggest (pp. 606-7), moreover, that this action should extend beyond policies aimed at improvement of the over-all levels of national prosperity to specific regional aid programs. They grant, though, that general prosperity since 1940 has slowly but surely helped with alleviation of regional income imbalances. Furthermore, they associate the worst regional income imbalances with problems created by structural changes in agriculture, the major part of which may now be completed. The question therefore arises, but is left unanswered, of whether regional income imbalances will be as serious a problem in the future as in the recent past.

No short summary can do justice, however, to this complex, thoughtful and important study of regional problems. Among many virtues, it provides a wealth of data on regional changes and characteristics that have not been readily accessible previously. It will unquestionably be a standard reference for future empirical researchers in regional economics. Evaluated together with the other recent efforts to revive hypothesis-testing on regional problems, it leads to the almost incontestable conclusion that regional economics has both the data and hypotheses to become a behavioral as well as a normative science.

V. *Conclusions*

A basic theme of the preceding discussion is that regional economics has progressed significantly in the last decade and has been concerned, in a generally effective fashion, with contributing to the solution of a number of important policy questions. Some of this progress, however, has been achieved at the neglect of certain important questions and a diversion of effort away from fruitful research channels. In short, regional economics is incomplete in a number of important respects, apart from the mere existence of a number of untouched research questions and problems. Specifically, the suggestion has been advanced that regional economics has reached a stage where it could benefit from some redirection of effort away from the design of broad conceptual frameworks and accumulation of regional income accounts toward the formulation and testing of behavioral hypotheses, with the initial emphasis being placed on hypotheses that could be quite readily developed from the application of general economic concepts already available.

Some redirection of effort along these lines seems justified if for no other reason than that many of the sophisticated research designs now being attempted in regional economics require a considerable input of "behavioral understanding" to be effective. The Penn-Jersey linear programming model, with its highly complicated structure of behavioral assumptions, is illustrative; in fact, the most immediate contribution of the Penn-Jersey study may be a better understanding of some behaviorial characteristics because of research stimulated by the necessity to have reasonably realistic inputs in the model. Similarly, the validity and accuracy of many input-output applications obviously depends on a knowledge of the structural coefficients involved. These, in turn, are often undergoing continual change; and explaining these changes requires more knowledge of behavioral characteristics than is now normally available.

In a pragmatic, decision-oriented field like regional economics some attention also might be given to the highly practical question of when and whether more complex research designs yield sufficiently improved results to justify their costs. Particularly relevant would be some empirical tests of the effects of incorporating greater or less interdependence and aggregation into regional forecasting models. For instance, is a gain in accuracy always obtained from using a matrix multiplier in place of a highly aggregative composite coefficient of the type normally employed in so-called economic base analyses? If not, is there a pattern to the performance comparisons so that this information could be used to improve the allocation of research resources in future undertakings? In general, regional analysis might benefit from an incorporation of some of the ideas found in modern statistical decision theory and particularly the notion that the costs of obtaining better decisions should be compared with the obtainable yield.

The scarcity of hypothesis-testing in regional analysis is all the more regrettable when the availability of much excellent and highly relevant data is noted. Specifically, among other things, the nature of U.S. government census practices, the regionalization of the banking system, and the common requirement by the Bureau of Public Roads that a considerable proportion of any research funds made available under the interstate highway program be spent on local data-gathering and interview studies have resulted in a remarkable collection of cross-section data being available on regional, state, metropolitan, and even intracity characteristics. Indeed, it is hardly any exaggeration to say that regional economics is in many ways one of the most fortunately blessed fields in economics in terms of data availability.

Yet it gives an appearance, as noted previously, of being notably undernourished in data. The paradox is more apparent than real. The data problems in regional economics stem more from a choice of activities and interests by regional economists than from any other factor. With a few notable exceptions, regional economists have shown a great propensity for undertaking the difficult tasks first. For example, estimating input-output tables is difficult enough at the national level, even with all the statistics that are only available at that level of aggregation. Mathematical programming often encounters serious data problems when confined to the analysis of a limited optimization problem within an individual firm. Constructing good income accounts has proven a quite formidable task even for well-financed and -staffed national agencies. In fact, it is still true in the United States, where national income accounting has been pioneered, that a number of notable deficiencies or guesses exist in the available social accounts. It is at least relevant to ask, therefore, if constructing elaborate regional accounts as advocated by many regional economists, is a justifiable expenditure of limited research resources until the more important limitations in the national accounts are removed.

Regional data problems are not, of course, unimportant or undeserving of attention. Many of them must be solved, in fact, before certain important empirical investigations can be undertaken. Furthermore, there is obviously a considerable demand in some quarters for better regional accounts as an aid to policy-making. Nevertheless, a more nearly optimal allocation of research resources within regional economics would seem to involve less relative effort on income accounting and interregional trade-flow coefficient estimation and more attention to developing and testing hypotheses. In the short run, moreover, a number of interesting and highly pertinent hypotheses could be subjected to at least preliminary testing with data that are already available and of quite respectable accuracy.

In sum, regional economics is very much what it is today because it has stood ready to attempt analytical solutions to difficult policy problems. Its major contributions thus far have been to provide broad measures and frameworks needed to evaluate and organize these activities—and this is without question a considerable contribution. Further implementation or realization on these efforts will require, however, greater knowledge of regional growth processes and related behavioral patterns than is now available. To do this, regional economics almost certainly must become increasingly involved with hypotheses about the behavior and role of financial organizations, market structures, entrepreneurship, private and public investment decisions, taxes, fiscal policies, and all the other subjects normally encompassed in economics but now encountered only occasionally in regional economics. By contrast, relatively less effort is likely to be needed on studies concerned exclusively with constructing research frameworks and studying the effects of spatial separation for its own sake. Since these two latter areas have tended to be the distinctive elements in regional economics in recent years, this suggests that regional economics may increasingly be indistinguishable from the rest of economics. It is not clear, moreover, that such a development should be viewed with great alarm in a world made increasingly smaller and more homogeneous by political and institutional developments and by improvements in transportation, communications, and other technologies, with all that these imply for resource mobility.

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THE "LIFE CYCLE" HYPOTHESIS OF SAVING: AGGREGATE IMPLICATIONS AND TESTS

By ALBERT ANDO AND FRANCO MODIGLIANI*

The recent literature on the theory of the consumption function abounds with discussions of the permanent income hypothesis of Friedman and other related theories and attempts at their empirical verification. Friedman's formulation of the hypothesis is fairly well suited for testing against cross-section data, though numerous difficulties are associated with this task, and there is now a rapidly growing body of literature on this subject [5] [8] [11] [12] [14] [16] [17] [21] [25]. Friedman's model, on the other hand, does not generate the type of hypotheses that can be easily tested against time series data.

More or less contemporaneously with Friedman's work on the permanent income hypothesis, Modigliani and Brumberg developed a theory of consumer expenditure based on considerations relating to the life cycle of income and of consumption "needs" of households [34] [35]. Several tests of the Modigliani-Brumberg theory using cross-section data have been reported in the past including a comparative analysis of the cross-section implications of this hypothesis as against the Friedman model [8] [12] [32] [33].

Modigliani and Brumberg have also attempted to derive time series implications of their hypothesis in an as yet unpublished paper [34], and their theory appears to generate a more promising aggregative consumption function than does Friedman's. However, at the time of their writing the unavailability of data on net worth of consumers made empirical verification exceedingly difficult and indirect [6] [7] [20]. Since then, this difficulty has been partially eliminated as a result of the work of Goldsmith [18] [19].

In Part I of this paper, we give a brief summary of the major aggregative implications of the Modigliani-Brumberg life cycle hypothesis of saving. In Part II, we present the results of a number of empirical tests for the United States which appear to support the hypothesis.[1] The reader who is not interested in the derivation and statistical testing of the aggregate Modigliani-Brumberg consumption function may proceed directly to Part III, where we develop some features of the model which, in our view, make it particularly suitable for the analysis of economic growth and fluctuations, as indicated in our past and forthcoming contributions [1] [2] [3] [4] [29] [31].

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I. *Theory*

A. *Derivation of the Aggregate Consumption Function*[2]

The Modigliani and Brumberg model starts from the utility function of the individual consumer: his utility is assumed to be a function of his own aggregate consumption in current and future periods. The individual is then assumed to maximize his utility subject to the resources available to him, his resources being the sum of current and discounted future earnings over his lifetime and his current net worth. As a result of this maximization the current consumption of the individual can be expressed as a function of his resources and the rate of return on capital with parameters depending on age. The individual consumption functions thus obtained are then aggregated to arrive at the aggregate consumption function for the community.

From the above brief description, it is quite apparent that the most crucial assumptions in deriving the aggregate consumption function must be those relating to the characteristics of the individual's utility function, and the age structure of the population. The basic assumptions underlying the shape of the utility function are:

Assumption I: The utility function is homogeneous with respect to consumption at different points in time; or, equivalently, if the individual receives an additional dollar's worth of resources, he will allocate it to consumption at different times in the same proportion in which he had allocated his total resources prior to the addition.[3]

Assumption II: The individual neither expects to receive nor desires to leave any inheritance. (This assumption can be relaxed in either of two ways. First, we may assume that the utility over life depends on planned bequests but assume that it is a homogeneous function of this variable as well as of planned consumption. Alternatively, we may assume that the resources an individual earmarks for bequests are an increasing function of the individual's resources relative to the average level of resources of his age group, and that the relative size distribution of resources within each age group is stable over time. It can be shown that either of these generalized assumptions implies an aggregate consumption function similar in all essential characteristics to the one obtained from the stricter assumption stated here.)

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[1] Since this paper was submitted and accepted for publication, a model that bears some similarity to the one proposed here has been presented by Alan Spiro in "Wealth and Consumption Function," *Jour. Pol. Econ.*, Aug. 1962, *70*, 339–54.

[2] The theory summarized here is essentially the same as that developed by Modigliani and Brumberg in [7] and [34]. Because of the untimely death of Richard Brumberg in August 1954, the original paper by Modigliani and Brumberg [34] has never been published, and it is not likely to be published in the near future. Because of this, we present here a summary. However, the aggregation procedure developed here is different from that followed by Modigliani and Brumberg. This is because they are largely concerned with numerical prediction of parameters while we are interested only in exhibiting the conditions for existence of a particular form of the aggregate consumption function.

[3] This equivalence holds on the assumption that consumers deal in perfect markets.

These two assumptions can be shown to imply (cf. [35, pp. 390 ff.]) that, in any given year t, total consumption of a person of age T (or, more generally, of a household headed by such a person) will be proportional to the present value of total resources accruing to him over the rest of his life, or:

$$c_t^T = \Omega_t^T v_t^T. \tag{1.1}$$

In this equation[4] Ω_t^T is a proportionality factor which will depend on the specific form of the utility function, the rate of return on assets, and the present age of the person, but not on total resources, v_t^T. The symbol c_t^T stands for total consumption (rather than for consumer's expenditure) in the year t. It consists of current outlays for nondurable goods and services (net of changes if any in the stock of nondurables) plus the rental value of the stock of service-yielding consumer durable goods. This rental value in turn can be equated with the loss in value of the stock in the course of the period plus the lost return on the capital tied up. Finally the present value of resources at age T, v_t^T, can be expressed as the sum of net worth carried over from the previous period, a_{t-1}^T, and the present value of nonproperty income the person expects to earn over the remainder of his earning life; i.e.,

$$v_t^T = a_{t-1}^T + y_t^T + \sum_{\tau=T+1}^{N} \frac{y_t^{eT\tau}}{(1 + r_t)^{\tau-T}} \tag{1.2}$$

where y_t^T denotes current nonproperty income; $y_t^{eT\tau}$ is the nonproperty income an individual of age T expects to earn in the τth year of his life; N stands for the earning span and r_t for the rate of return on assets.[5]

In order to proceed further, it is convenient to introduce the notion of "average annual expected income," y_t^{eT}, defined as follows:

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[4] For the sake of simplicity, we shall not display the stochastic component of these relations explicitly in this section.

[5] To be precise $y_t^{eT\tau}$ is the income the person expects to earn at age τ, measured in prices prevailing in the year t, and r is the "real" rate of return on assets. In (1.2) the expected real rate is assumed to remain constant over time, but the formula can be generalized to allow for changing rate expectations.

$$y_t^{eT} = \frac{1}{N-T} \sum_{\tau=T+1}^{N} \frac{y_t^{eT_\tau}}{(1+r_t)^{\tau-T}}. \tag{1.3}$$

Making use of this definition and of (1.2) we can rewrite equation (1.1) as:

$$c_t^T = \Omega_t^T y_t^T + \Omega_t^T (N-T) y_t^{eT} + \Omega_t^T a_{t-1}^T. \tag{1.4}$$

To obtain an expression for aggregate consumption we proceed to aggregate equation (1.4) in two steps, first within each age group and then over the age groups.

If the value of Ω_t^T is identical for all individuals in a given age group T, then it is a simple matter to aggregate equation (1.4) over an age group, obtaining:

$$C_t^T = \Omega_t^T Y_t^T + (N-T)\Omega_t^T Y_t^{eT} + \Omega_t^T A_{t-1}^T \tag{1.5}$$

where C_t^T, Y_t^T, Y^{eT}, and A_{t-1}^T are corresponding aggregates for the age group T of c_t^T, y_t^T, y_t^{eT}, and a_{t-1}^T. If Ω_t^T is not identical for all individuals in the age group, however, the meaning of the coefficients in equation (1.5) must be reinterpreted. It has been shown by Theil [41] that under a certain set of conditions the coefficients of (1.5) can be considered as weighted averages of the corresponding coefficients of (1.4).[6]

Next, taking equation (1.5) as a true representation of the relationship between consumption and total resources for various age groups, we wish to aggregate them over all age groups to get the consumption function for the whole community. Consider the equation:

$$C_t = \alpha_1' Y_t + \alpha_2' Y_t^e + \alpha_3' A_{t-1} \tag{1.6}$$

where C_t, Y_t, Y_t^e and A_{t-1} are obtained by summing respectively C_t^T, Y_t^T, Y_t^{eT} and A_{t-1}^T over all age groups T, and represent therefore aggregate consumption, current nonproperty income, "expected annual nonproperty income," and net worth.

The theorems given by Theil again specify the conditions under which the coefficients in equation (1.6) are weighted averages of the corresponding coefficients of equation (1.5). In this case, it is likely that the conditions specified by Theil are not satisfied, because both net worth and its coefficient in equation (1.5) are positively correlated with age up to the time of retirement. However, a much weaker set of conditions can be specified which are sufficient to insure stability over time of parameters in equation (1.6). In particular one such set of conditions is the constancy in time of (i) the parameters of equation (1.5) for every age group, (ii) the age structure of population, and (iii) the relative distribution of income, of expected income, and of net worth over the age groups.

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[6] See Theil [41, pp. 10–26]. More precisely, the least squares estimates of the parameters of equation (1.5) will be weighted averages of the least squares estimates of the corresponding parameters of equations (1.4) only if the set of conditions specified by Theil in the reference cited above is satisfied. Roughly speaking, these conditions require that there be no systematic relations between parameters and variables of equation (1.4) over individuals.

B. *A Priori Estimates of the Coefficients of the Aggregate Consumption Function*

Modigliani and Brumberg [34], in order to obtain a priori estimates of the order of the magnitude of the coefficients of equation (1.6) implied by their model, introduced a number of rather drastic simplifying assumptions about the form of the utility function and life pattern of earnings, to wit:

Assumption III: The consumer at any age plans to consume his total resources evenly over the remainder of his life span.

Assumption IV: (a) Every age group within the earning span has the same average income in any given year t. (b) In a given year t, the average income expected by any age group T for any later period τ, within their earning span, is the same. (c) Every household has the same (expected and actual) total life and earning spans, assumed to be 50 and 40 respectively for the purpose of numerical computation.

Assumption V: The rate of return on assets is constant and is expected to remain constant.

Under these assumptions, if aggregate real income follows an exponential growth trend—whether due to population or to productivity growth—the sufficient conditions for the constancy in time of the parameters of (1.6) are satisfied. The value of these parameters depends then only on the rate of return on assets and on the over-all rate of growth of income, which in turn is the sum of population growth and the rate of increase of productivity.[7]

Table 1 gives some examples of the numerical value of the coefficients under the assumptions described above.

It should be emphasized that assumptions III to V have been introduced only for the sake of numerical estimation of the coefficients and are by no means necessary to insure the approximate constancy in time of the parameters in (1.6). A change in the assumptions would lead to somewhat different values of the parameters. But both a priori considerations and rough numerical calculations suggest that these values would not be drastically affected, and that it is generally possible to

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[7] Strictly speaking the values of the parameters would vary somewhat depending on whether the growth of income results from population or from productivity growth. However, for rates of growth within the relevant range, say 0 to 4 per cent per year, the variation turns out so small that it can be ignored for present purposes.

TABLE 1—COEFFICIENTS OF THE CONSUMPTION FUNCTION (1.6) UNDER STATED ASSUMPTIONS[a]

Yield on assets (per cent)	0	0	0	3	5	5	5
Annual rate of growth of aggregate income (per cent)	0	3	4	0	0	3	4
$\alpha_1+\alpha_2$	.61	.64	—	.69	.73	—	—
α_3	.08	.07	.07	.11	.13	.12	.12

[a] Missing values have not been computed because of the complexity of calculation.

infer the direction in which these values would move when a specific assumption is changed. The recognition of the estate motive would tend to yield lower values for both coefficients, especially that of assets.[8]

On the whole, then, the values shown in Table 1 should be regarded as a rough guide to the order of magnitude of the coefficients consistent with the basic model; i.e., radically different values would cast serious doubts on the adequacy of the life cycle hypothesis.

C. *The Measurement of Expected Income*

The last point that must be clarified before we proceed to the discussion of the empirical tests is the measurement of expected nonproperty income, Y^e, which, at least at present, is not directly observable. A "naive" hypothesis is to assume that expected nonproperty income is the same as actual current income, except for a possible scale factor. Thus, we have:

$$Y_t^e = \beta' Y_t; \qquad \beta' \simeq 1.$$

Substituting the above expression into (1.6), we obtain the aggregate consumption function

$$C_t = (\alpha_1' + \beta'\alpha_2')Y_t + \alpha_3' A_{t-1} = \alpha_1 Y_t + \alpha_3 A_{t-1}$$
$$\alpha_1 = \alpha_1' + \beta'\alpha_2' \simeq \alpha_1' + \alpha_2'.$$

We designate this formulation as hypothesis I.

A similar but somewhat more sophisticated formulation is to assume that expected income is an exponentially weighted average of past income, weights adding up to one, or slightly more than one in order to reflect the expected growth [15] [16]. But it is quite difficult to determine the weights from the data we have at our disposal, and Friedman, who favors this formulation, has acknowledged its shortcomings [15].

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[8] On the other hand, if we assume (i) that the preferred pattern of allocation of consumption and the pattern of income over life are the type suggested by the available cross-section data, (ii) that income expectation is consistent with the prevailing pattern of income, again suggested by the cross-section data over age groups, then the resulting coefficients of income and assets in equation (1.6) would be somewhat higher than those reported in Table 1.

The third possible formulation is a slight modification of the first. Under our definitions, Y, and expected income, Y^e, are nonproperty or labor income, excluding, for instance, profits. We may hypothesize that for those currently employed, average expected income, y_t^e, is current income adjusted for a possible scale factor, i.e.,

$$y_t^e = \beta_1 \frac{Y_t}{E_t} \tag{1.7}$$

where E_t is the number of persons engaged in production. We should expect β_1 to be quite close to unity.

For those individuals who are currently unemployed, we hypothesize that expected income is proportional to the average current income of those who are employed. The proportionality constant in this case represents three factors. First, as before, there may be some influence from expected growth. Second, and probably most important, the incidence of unemployment is likely to be smaller for higher-paid occupations than for lower-paid, less-skilled workers; hence, the average earnings the unemployed can look forward to, if reemployed, are likely to be lower than the average earnings of those currently employed. Third, it seems reasonable to suppose that some of the currently unemployed persons would expect their current unemployment status to continue for some time and, possibly, to recur. We shall therefore assume:

$$y_t^{eu} = \beta_2 \frac{Y_t}{E_t} \tag{1.8}$$

where y_t^{eu} is the average expected income of unemployed persons; and, for the reasons given above, we expect the constant β_2 to be substantially smaller than β_1. The aggregate expected income is then given by:

$$\begin{aligned} Y_t^e &= E_t y_t^e + (L_t - E_t) y_t^{eu} = E_t \beta_1 \frac{Y_t}{E_t} + (L_t - E_t)\beta_2 \frac{Y_t}{E_t} \\ &= (\beta_1 - \beta_2) Y_t + \beta_2 \frac{L_t}{E_t} Y_t \end{aligned} \tag{1.9}$$

where L_t denotes the total labor force.[9]

Substituting (1.9) into (1.6), we obtain the following variant of hypothesis I,

$$C_t = \alpha_1 Y_t + \alpha_2 \frac{L_t}{E_t} Y_t + \alpha_3 A_{t-1} \tag{1.10}$$

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[9] See Ando [1]. Mincer in [28] relied on a similar device, except that he used population in place of labor force.

where

$$\alpha_1 = \alpha_1' + \alpha_2'(\beta_1 - \beta_2)$$
$$\alpha_2 = \alpha_2'\beta_2; \qquad \alpha_3 = \alpha_3'.$$

We designate the formulation embodied in equation (1.10) above as hypothesis II.

Since β_1 is thought to be close to unity, we have

$$\alpha_1 + \alpha_2 = \alpha_1' + \beta_1\alpha_2' \simeq \alpha_1' + \alpha_2'. \tag{1.11}$$

The individual values of the observable coefficients α_1 and α_2 are, however, dependent on the nonobservable value of β_2, about which there is little we can say a priori.

II. *Empirical Verification and Estimation*

In this section we report results of a number of tests of our model for the United States.[10] Unless otherwise stated, the period of observation is 1929 through 1959 excluding the Second World War years 1941–46.[11] Consumption, C, labor income net of taxes, Y, and net worth, A, are all measured in billions of current dollars as called for by our hypothesis.[12]

In recent years, economists have become increasingly aware of the many sources of bias, inconsistency, and inefficiency that beset prevailing estimation procedures, e.g., the existence of simultaneous relations, errors of observations in the "independent" variables, spurious correlation, multicollinearity, and heteroscedascity.[13] As a result, the simple-minded and straightforward least-squares approach is being replaced by a host of alternative procedures. Unfortunately most of these alternative procedures are designed to cope with one specific source of difficulty, and they often do so at the cost of increasing the difficulties arising from other sources. Under these conditions, we feel that the best course is to utilize a variety of procedures, exploiting our knowledge of the structure of the model and the nature of data to devise methods whose biases are likely to go in opposite directions. By following such a procedure, we can at least have some confidence that the estimates obtained by different methods will bracket the value of the unknown parameters being estimated.

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[10] The data and the procedure by which they have been obtained will be found in Ando, Brown, Kareken, and Solow [3, Data App.]. The derivation of labor income after taxes is particularly troublesome and is based in part on methods suggested by [13] and [38].

[11] A few experiments were made using data including the Second World War years, and equation (1.6) appears to explain consumption behavior during these years better than any other consumption function to our knowledge. However, the fit is still not very good, and, at any rate, we do not feel that these years are relevant because of their obviously special characteristics.

[12] In this section, the time subscript will be omitted whenever there is no danger of confusion.

[13] See, for instance, Theil [40].

The main alternative procedures used and the estimates obtained are summarized in Table 2. Row (1) shows the results of a straightforward least-squares fit of hypothesis I.[14] The coefficients of both independent variables are highly significant and R^2 extremely high. But in other respects, the results are not altogether satisfactory. The coefficient of Y, which is an estimate of $\alpha_1+\alpha_2$, is somewhat higher and that of A appreciably lower than our model would lead us to expect. Furthermore, the Durbin-Watson statistic [10] falls considerably short of 2, suggesting the presence of pronounced serial correlation in the residuals.

As can be seen from row (2), the results do not change appreciably if we replace hypothesis I with II by introducing an additional variable $Y_{\frac{L}{E}}$. Although the coefficient of $Y_{\frac{L}{E}}$ has the right sign it does not appear to contribute significantly to the explanation of C. Meanwhile, it reduces still further the estimate of the coefficient of net worth, and increases the estimate of $\alpha_1+\alpha_2$ which, it will be recalled, is approximately given by the sum of the coefficients of Y and $Y_{\frac{L}{E}}$. Also, the serial correlation of the residuals does not change at all. As will soon become apparent, much of the difficulty with hypothesis II can be traced back to multicollinearity, which makes it rather hard to obtain reliable estimates of the individual coefficients.

Note also that in both (1) and (2) the constant term is very significantly different from zero by customary standards, a result which would seem inconsistent with the hypothesis tested. In our view, however, this result is not as serious as might appear at first glance. The constant term is numerically rather small, amounting to only about 5 per cent of the mean value of the dependent variable. Furthermore, we know that the least-squares estimate of the constant term is upward-biased in the present instance because of the simultaneous-equations bias as well as because of errors of measurement in the independent variables.[15] While the size of these biases cannot be directly estimated, we suspect it to be appreciable. Accordingly, on the basis of presently available evidence, we see no compelling reason to reject the hypothesis that consumption is in fact roughly homogeneous in income and assets. Under these circumstances, a more reliable estimate of the coefficients of these variables might be obtained by suppressing the constant term in accordance with the specification of our model.

The constrained estimation results in the equations reported in rows (3) and (5) of Table 2. A comparison of row (1) and row (3) shows that this procedure leads to estimates which are more nearly of the order

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[14] In this section, we shall refer to equations by the rows in Table 2.

[15] See footnote 17.

TABLE 2

ESTIMATES OF THE COEFFICIENTS OF THE CONSUMPTION FUNCTION

Rows	Hypothesis Tested	Mode of Regression[a]	Coefficients and Their Standard Errors of Estimates[b]						$\alpha_1+\alpha_2$	Standard Error of Dependent Variable	Standard Error of Estimate	R^2[c]	Durbin-Watson Statistic
			Constant	Y (α_1)	XY (α_{12})	$Y(L/E)$ (α_2)	A (α_3)	XA (α_{32})					
(1)	I	A	8.1 (1.0)	.75 (.05)	—	—	.042 (.009)	—	.75	88.289	2.233	.998	1.26
(2)	II	A	7.3	.65 (.11)	—	.13 (.13)	.037 (.011)	—	.78	88.289	2.188	.998	1.26
(3)	I	A	—	.56 (.09)	—	—	.081 (.015)	—	.56	88.289	4.414	.997	.33
(4)	I	A	—	.87 (.08)	—	—	.046 (.012)	−.018 (.003)	.87	88.289	2.826	.998	1.13
(5)	II	A	—	.59 (.16)	—	.19 (.12)	.036 (.017)	—	.78	88.289	3.443	.998	.74
(6)	I	B	—	.52 (.11)	—	—	.072 (.018)	—	.52	8.292	2.208	.929	1.85
(7)	I	B	—	.60 (.16)	−.11 (.13)	—	.074 (.018)	—	.60[d]	8.292	2.177	.931	1.92
(8)	I	B	—	.51 (.12)	—	—	.089 (.031)	−.017 (.024)	.51	8.292	2.184	.930	1.91
(9)	II	B	—	.44 (.12)	—	.24 (.15)	.049 (.022)	—	.68	8.292	2.090	.936	1.74
(10)	II	B	—	.39 (.13)	—	.31 (.16)	.076 (.029)	−.033 (.024)	.70	8.292	1.994	.942	1.91
(11)	I	C	—	.44 (.05)	—	—	.105 (.008)	—	.44	.092	.030	.899	.34
(12)	I	C	—	.69 (.07)	−.07 (.02)	—	.071 (.009)	—	.69[d]	.092	.021	.948	.93
(13)	I	C	—	.69 (.06)	—	—	.071 (.009)	−.012 (.003)	.69	.092	.020	.953	.95
(14)	II	C	—	.62 (.09)	−.06 (.02)	.10 (.08)	.065 (.011)	—	.71[d]	.092	.021	.948	.93
(15)	II	C	—	.63 (.09)	—	.09 (.08)	.066 (.010)	−.011 (.003)	.71	.092	.020	.953	1.03

[a] A: Regressions in which variables are used in the original form.
B: Regressions in which variables are used in the first-difference form.
C: Regressions in which variables are used in the form of ratios to labor income.

[b] Figures in parentheses underneath the estimates are estimated standard errors of respective estimates. Number of observations = 25. Where no estimate is shown, the variable is excluded from the equation.

[c] The ratio of the variance of the residual to the variance of the dependent variable.

[d] Figures given do not include the coefficient of XY. In other words, the estimate of the coefficient pertains to the period 1929–40.

of magnitude suggested by our model. Unfortunately the serial correlation is now so high that the reliability of the estimate is open to serious question. From row (5) it also appears that the addition of the variable Y_E^L is again not very helpful. Though its contribution is still hardly significant, it again lowers the coefficient of A, and the serial correlation remains high.

A common procedure in time-series analysis when serial correlation of errors is high is to work with first differences. In the present instance this procedure also serves to reduce drastically the degree of multicollinearity and provides a more meaningful test for the adequacy of the hypothesis as a causal explanation of consumption. The results, reported in rows (6) and (9) and in Figure 1, appear quite favorable to the hypothesis. The multiple correlation remains quite high and the coefficient of net worth is highly significant. Also the Durbin-Watson statistic improves considerably and there is no longer any reason to suspect that the reliability of the estimate is seriously affected by serial correlation of residuals. Furthermore, a comparison of row (6) with row (3) reveals a relatively minor change in the estimates of the coefficients of both Y and A, tending to increase our confidence in these estimates. Also from row (9), we see that the introduction of the variable Y_E^L produces now a much less drastic change in the estimates of the coefficients: the estimate of α_3 in particular is reduced only from .07 to .05, while that of $\alpha_1+\alpha_2$ is raised from .52 to .68. On the other hand, a comparison of row (5) and row (9) reveals that the relatively small change in the estimate of $\alpha_1+\alpha_2$ from .78. to .68 is accompanied by a marked shift in the relative size of the coefficients of Y and Y_E^L, the first falling and the second rising appreciably.

An interpretation and explanation of these various results is readily found. When we deal with actual values the movements of all the variables are dominated by their common trend. On the other hand, when dealing with first differences we are primarily focusing on short-run or cyclical variations. Now with respect to such variations, consumption is much more stable than income, even labor income. This stability is accounted for in our model by the fact that both net worth and expected labor income are more stable than current income in the short run, even though all the variables move together in the longer run. These considerations help to explain why in row (9) the coefficient of current income is appreciably lower than in row (5), while the coefficients of the two remaining, cyclically more stable, variables are correspondingly increased. Equation (9) suggests that consumption may be less sensitive to purely cyclical and temporary swings in current labor income than the estimates reported in row (5) indicate. At the same time the fact that both Y_E^L and A perform a similar function in stabilizing consump-

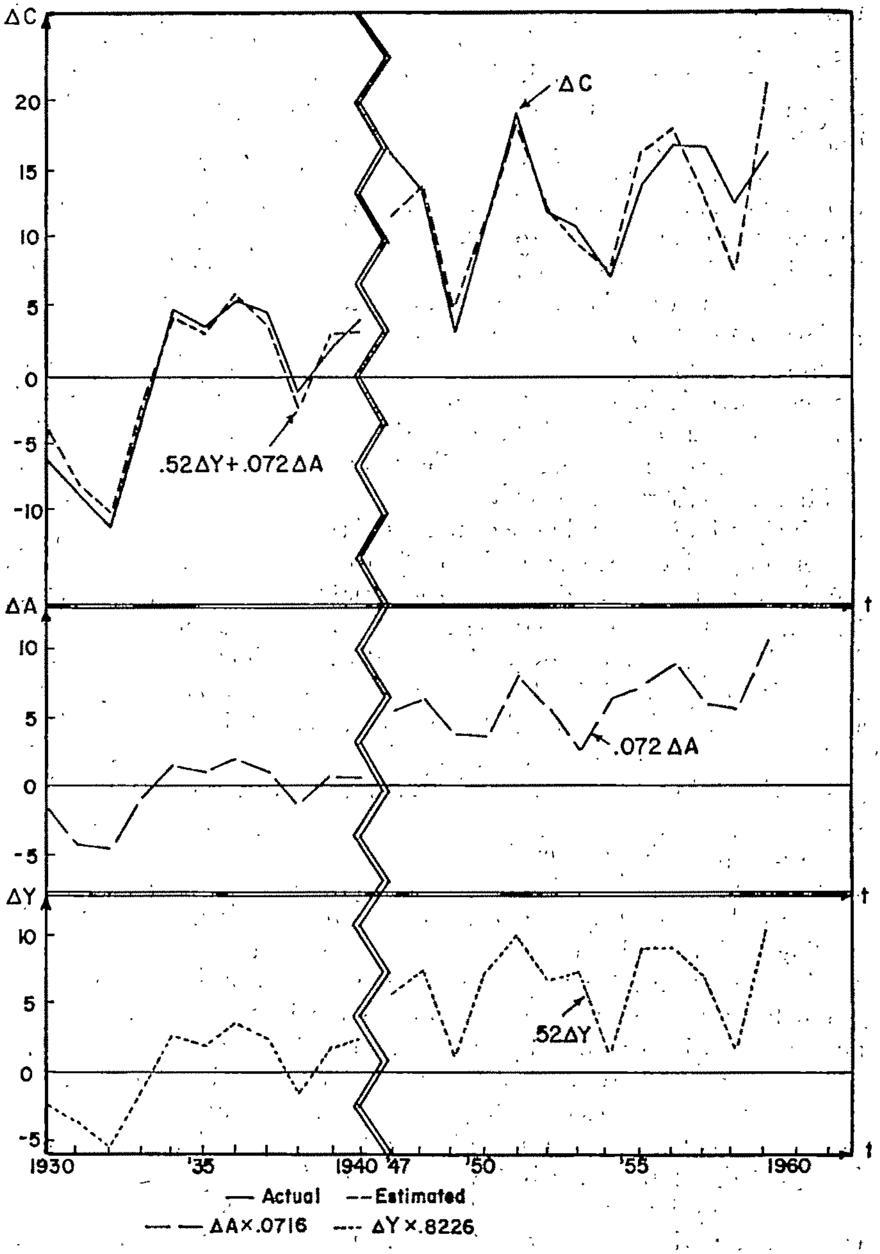

FIGURE 1. TIME PROFILE OF (6) AND ITS COMPONENTS

tion with respect to short-run variations in Y helps to explain why the addition of the latter variable generally tends to reduce not only the coefficient of Y but also that of A, even though its own contribution is statistically not very significant. It would thus seem that from the available data we cannot obtain a very reliable estimate of the role of each variable separately.[16]

All the estimates reported so far are based on the least-squares method applied to a single equation. As is well known this method leads to estimates which are biased, even in the limit, when one or more of the "independent" variables are related to the dependent variable by other simultaneous relations. In the present instance the variable A can be taken as predetermined, but the same is not true of labor income which is related to consumption via total income. That is, the true error component of the consumption function cannot be assumed to be uncorrelated with Y, and hence the least-squares estimates of its parameters are not consistent. Specifically, it can be shown that, asymptotically, the estimator of the coefficient of Y is upward-biased and that of the coefficient of A downward-biased.[17]

The only really adequate way of resolving this difficulty would be to construct a complete model of the U. S. economy and then apply an appropriate simultaneous-equations estimation procedure. This approach would lead at least to consistent estimates, except to the extent that the model was incomplete or misspecified and the exogenous variables were subject to errors of measurement. Furthermore, the effi-

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[16] The simple correlation between ΔA and $\Delta(Y^L_E)$ is .93, higher than that between ΔY and $\Delta(Y^L_E)$, .89.

[17] Let us denote the *true* error term in equation (1.6) by ϵ. Then, under the assumption that the correlation between Y and A and that between Y and ϵ are positive, while A and ϵ are uncorrelated with each other, it can be readily shown that

$$\operatorname{plim} \hat{\alpha}_1 = \alpha_1 + \operatorname{plim} \frac{\sum A^2 \sum Y\epsilon}{\sum A^2 \sum Y^2 - (\sum AY)^2} \geq \alpha_1$$

$$\operatorname{plim} \hat{\alpha}_3 = \alpha_3 - \operatorname{plim} \frac{\sum Y\epsilon \sum AY}{\sum A^2 \sum Y^2 - (\sum AY)^2} \leq \alpha_3$$

where $\hat{\alpha}_1$ and $\hat{\alpha}_3$ are the least squares estimates of α_1 and α_3, respectively.

The above formulae are stated for the case in which the constant is suppressed, but for the case where the constant is not suppressed, it is only necessary to reinterpret the symbols as deviations from respective means. The asymptotic bias for the constant term can then be expressed in terms of the above limits and the means of the variables involved. Denoting the constant term and its least squares estimate by γ and $\hat{\gamma}$ respectively, it can be shown that

$$\operatorname{plim} (\hat{\gamma} - \gamma) = \operatorname{plim} \frac{\sum Y'\epsilon}{\sum A'^2 \sum Y'^2 - (\sum A'Y')^2} (\bar{A} \sum A'Y' - \bar{Y} \sum A'^2)$$

where the primed symbols denote the deviations from the mean. The estimate from our data of the probability limit of the expression inside the parenthesis is positive and fairly large, so that the asymptotic bias of the least squares estimate of the constant term is most likely to be positive.

ciency of the estimates might be reduced, particularly if the theory and data relating to other sectors of the economy were less reliable than those relating to the consumption sector. Whatever the merits of this approach, however, we regard the specification of a complete model beyond the scope of this paper.

A compromise followed by some authors is to introduce an accounting identity relating consumption, saving, and income, note that saving is equal to investment, assume that investment is autonomous, and estimate the parameters of the consumption function from the regression of consumption on saving [39] [43]. Now in our view this procedure is likely to lead to bias in the estimates of the parameters which is more serious than that resulting from the conventional regression on income. The arguments supporting this conclusion are developed formally in Appendix, section B and can be summarized here as follows:

1. When the "independent" variable is subject to errors of measurement, the resulting estimate of the regression coefficient is biased toward zero, the more so the greater the variance of the error of measurement of the independent variable relative to its true variance. Now since personal saving is in the order of one-tenth of disposable income, it is reasonable to suppose that the variance of true (as distinguished from measured) personal saving is a good deal smaller than the variance of disposable income. At the same time, personal saving, as actually measured in the national income accounts, represents the difference between largely independent estimates of disposable income and of personal consumption. Hence the error of measurement of personal saving is likely to be even larger than that of disposable income. We can therefore be rather confident that the bias toward zero due to errors of measurement will be a good deal more serious when consumption is regressed on personal saving than when it is regressed on disposable income. Furthermore, given the estimating procedure, the error of measurement of consumption is likely to be negatively correlated with the error of measurement of saving, and this negative correlation will produce a further downward bias in the estimate of the true regression coefficient.

2. Personal saving is *not* identically equal to investment either conceptually or in terms of actual measurement, and investment is *not* exogenous, or independent of consumption, even in the short run, especially when account is taken of investment in inventories. At least over a very short period of time, such as a quarter or less, it is quite likely that random variations in consumption behavior will be accompanied by random variations in personal saving in the opposite direction, and, to the extent that this is true, an estimate of the propensity to consume based on the regression of consumption on saving will be seriously downward-biased, even in the absence of errors of measurement.

For the problem at hand, there exists an alternative and relatively simple way of securing consistent estimates of the parameters in hypothesis I. Relying on the specification that the constant term is zero and that the true error is uncorrelated with A, this procedure consists in (i) regressing income on assets, and obtaining computed values of Y from this regression, say Y_c; (ii) then regressing C on Y_c and on A. The coefficients of Y_c and A so obtained can be shown to be consistent estimators of the coefficients of Y and A respectively.[18]

Unfortunately the application of this procedure to our problem yields rather meaningless results (the point estimate of the coefficient of Y turns out to be negative!). This outcome is not entirely surprising in view of the high degree of multicollinearity present and the fact that this procedure is probably not a very efficient one. Since multicollinearity is reduced by first-differencing data, we have applied this procedure to the first differences, though the assumptions which assure the consistency of the estimates are no longer strictly tenable in this case.[19] The resulting estimates of α_1 and α_3 turned out to be .67 and .05.

Another possible way of coping with the problem of bias in the estimates resulting from a known cause but of unknown magnitude is to construct an alternative estimation procedure in which the same cause may be expected to produce a bias in the opposite direction. If this can be done, then the unknown parameters may be bracketed by the estimates generated by the two alternative procedures, and if they are close together, then we may conclude with some confidence that the bias in either procedure is not too serious. For this purpose, suppose that

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[18] Suppose that we estimate the parameters of the following equation by the method of least squares:

$$Y = b_y A + a_y + \eta_y \tag{a}$$

where η_y is the error term.

The substitution of the value of Y estimated by (a) into equation (1.6) yields

$$C = \alpha_1(b_y A + a_y) + \alpha_3 A + \epsilon = (\alpha_1 b_y + \alpha_3)A + \alpha_1 a_y + \epsilon. \tag{b}$$

Comparison of equation (b) with the regression of C on A

$$C = b_c A + a_c + \eta_c \tag{c}$$

results in

$$\alpha_1 b_y + \alpha_3 = b_c \tag{d}$$

$$\alpha_1 a_y = a_c \tag{e}$$

(d) and (e) can then be solved to give the estimated values of α_1 and α_3. These estimates are identical with those resulting from the two-stage least-squares procedure described in the text.

[19] Since $A_{t-1} = A_{t-2} + S_{t-1}$ and S_{t-1} depends on Y_{t-1} and C_{t-1}, it must be admitted that the possibility of a positive correlation between A_{t-1} and ϵ_{t-1}, and hence between ΔA_{t-1} and $\Delta\epsilon_t$, cannot be ruled out. However, it seems safe to assume that this correlation, if it exists, is reasonably small.

we divide both sides of equation (1.6) by Y, obtaining

$$\frac{C}{Y} = \alpha_1 + \alpha_3 \frac{A}{Y}$$

and then proceed to estimate the parameters of this equation by the conventional least squares method. It can be shown that, in so far as the bias due to the positive correlation between Y and the true error of the consumption function is concerned, the least-squares estimate of α_3 will be upward-biased, and hence, that of α_1 will be downward-biased. Thus, for both coefficients, the bias is in the direction opposite to that resulting from other procedures reported so far. This approach has also other desirable properties: it eliminates the difficulties arising from the presence of strong multicollinearity, and the homoscedasticity condition is more likely to be satisfied. Furthermore, it eliminates altogether the common trend in the variables, thus providing a rather stringent test of the relevance of net worth as a determinant of consumption. Its main drawback is that the above-mentioned bias in the estimates may be appreciably reinforced by error of measurement in the variable Y, although error of measurement in A will tend to work in the opposite direction.[20]

The results of this test reported in row (12) are rather mixed. The still remarkably high value of R^2 provides strong support for the hypothesis. Also, as expected, the procedure yields a higher estimate of α_1 and lower estimate for α_3. But the gap between the estimates obtained in this manner and those reported earlier is so wide as to provide little useful information as to the true value of these coefficients. Furthermore, the very high serial correlation of errors casts serious doubt on the reliability of the estimates.

The high serial correlation in the estimated errors, in this and to a lesser extent in other procedures, suggests the desirability of testing for evidence of a significant change in the parameters of our consumption function as between the prewar and the postwar period. Individual tastes as well as the demographic structure and the rate of return on assets, on which the theoretical values of the coefficients depend, may have changed sufficiently over the two periods to cause a measurable change in the parameters. In addition, there exists a statistical problem arising from the fact that the data, particularly the net worth estimates, are based on somewhat different estimating procedures for the two periods. To test the hypothesis of a shift in parameters we have computed a number of regressions involving a "dummy" variable X, with value zero for the years 1929–40 and value one for the years 1947–59.

The results given in rows (4), (7), (8), (10), and (12) to (14), which constitute a representative sample of the tests we have carried out, show that the coefficient of the dummy variable is consistently negative, suggesting a moderate downward shift in the "measured" consumption function in the postwar period.[21] In the first-difference test the shift is not statistically significant and the coefficients of the other variables are altered only slightly. In the constrained and the ratio estimates, the downward shift is generally significant, and the serial correlation of the observed errors is reduced appreciably though it still remains high;[22] there also occur some sizable changes in the coefficients of the other variables. At least in the case of the ratio estimates, these changes tend on the whole to increase the agreement between the various results, which we may now endeavor to summarize.

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[20] For possible problems arising from the use of ratios, see [26] and [37].

In the first place, all of the tests seem, by and large, to support the basic hypothesis advanced in this paper, and in particular, the importance of net worth as a determinant of consumption. Unfortunately, serious difficulties arise in the attempt to secure reliable estimates of the coefficients of the "independent" variables, although some tentative conclusions seem to stand out. First, the different estimation procedures when applied to hypothesis I generally yield a similar estimate of the coefficient of net worth for the period as a whole, somewhere between .07 and .08. [Cf. rows (3), (6), (7), and (12), but note (11).]

At the same time, the estimate of the coefficient of income is definitely

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[21] Our model provides one possible clue to this apparent downward shift. An examination of the figures reported in [3] reveals a distinct decline in the ratio of nonlabor income to net worth, which can be taken as a measure of the rate of return on capital. This is presumably attributable in large measure to increases in corporate taxes and in the extent and progressiveness of personal income taxation. An examination of the values of the coefficients of income and net worth implied by our model, reported in Table 1, suggests that both coefficients should tend to decline as the rate of return on assets declines. However, we do not wish to press this point, especially since we cannot even be sure whether the apparent decline in the coefficients reflects anything more than error of measurement.

[22] Another possible explanation for the high serial correlation in some of the tests is that consumption does not adjust fully to changes in income and assets within the arbitrary time unit of one year. To allow for this possibility hypothesis I might be written as

$$C_t - C_{t-1} = \delta(\alpha_1 Y_t + \alpha_2 A_{t-1} - C_{t-1}), \text{ or } C_t = (1-\delta)C_{t-1} + \delta\alpha_1 Y_t + \delta\alpha_2 A_{t-1},$$

where the constant δ, with the dimension 1/time, measures the speed of adjustment of consumption and may be expected to approach unity as the time unit increases. This hypothesis was tested in ratio form (to avoid increasing further the already extremely high multicollinearity prevailing in the direct form) with the following results:

$$\frac{C_t}{Y_t} = .46 + \underset{(.077)}{.177}\ \frac{C_{t-1}}{Y_t} + \underset{(.016)}{.072}\ \frac{A_{t-1}}{Y_t}$$

implying $\delta = .82$, $\alpha_1 = .56$, $\alpha_2 = .087$.

The relatively low coefficient of C_{t-1} (which is only moderately significant) suggests that a span of one year is long enough for most of the adjustment to take place. Also the estimates of the remaining coefficients are not greatly affected and move closer to the first difference estimates. However, somewhat surprisingly, the serial correlation is increased still further.

unstable [see rows (3), (4), (6), (8), (10), and (12)]. This instability is appreciably reduced under hypothesis II, where the third variable, Y^L_E, apparently helps to disentangle the effect of purely cyclical and transitory changes in nonproperty income from that of long-run or permanent changes. The various estimates for the long-run marginal propensity, $\alpha_1+\alpha_2$, are fairly consistent—between .68 and .71 for the first-difference and the ratio estimates [rows (9), (11), and (15)], and only moderately higher for the straight estimate with constant suppressed [row (5)]. There is however much less consistency in the values of α_1 and α_2 separately and hence in the estimates of the short-run marginal propensity to consume with respect to labor income, α_1. The first-difference approach yields figures of .39 and .44 [rows (9) and (11)], while the estimates from straight regression with the constant suppressed and from regression on ratios [rows (5) and (15)] are about .6. On the whole we are inclined to regard the first estimates as somewhat more reliable, in part because of the high serial correlation present in rows (5) and (15), but no firm conclusion seems warranted with the available data and methods. At the same time we observe that the introduction of the third variable, Y^L_E, tends to reduce somewhat the estimate of the coefficient of net worth. It would appear that the value .07 to .08 obtained for hypothesis I, where Y^L_E is not present, may be somewhat too high—since the cyclically sluggish variable A acts partly as a proxy for expected nonlabor income—and the true value may be closer to .06 or even somewhat lower.

Our tests also agree in suggesting a moderate downward shift in both parameters, $\alpha_1+\alpha_2$, and α_3 of the measured consumption function from the prewar to the postwar period, although it does not seem possible at present to estimate reliably the distribution of this downward shift between $\alpha_1+\alpha_2$ and α_3.

As indicated earlier, a few tests of hypothesis I have also been carried out for the period 1900–28. Because the data for this period are mostly obtained from different sources and are subject to very wide margin of error, we have seen little point in combining them with the series relating to the period since 1929. In fact, we are inclined to attach rather little significance to the results of these tests, which are accordingly confined to Appendix section A. For whatever they are worth, these results do not appear grossly inconsistent with those for the period after 1929, especially when account is taken of error of measurement and its likely effects on different estimation procedures. In particular, the contribution of net worth appears again to be significant, its coefficient being of the same order of magnitude as in the later period except in the first-difference test which is obviously most seriously affected by the error of measurement.

Finally, our empirical results are also roughly consistent with the a priori numerical predictions reported in Table 1. The fact that the coefficients of both variables, especially that of net worth, are on the low side may be accounted for by reference to the estate motive which was ignored in the numerical calculations for Table 1, while it probably plays a nonnegligible role at least for the high-income and/or self-employed groups.[23]

III. *Some Implications*

A. *Relation to the Standard Keynesian Consumption Functions*

The standard Keynesian consumption function [23] is usually written in the form:

$$C = \gamma Y^{*} + \gamma_0 \tag{3.1}$$

where Y^{*} denotes personal income net of taxes or disposable income and the γ's are constants.[24] A more sophisticated variant of this hypothesis, which has become quite popular of late, consists in separating income into two parts, disposable labor income Y, and disposable nonlabor or property income, which we shall denote by P. Thus,

$$C = \gamma_1 Y + \gamma_2 P + \gamma_0. \tag{3.2}$$

This variant, which reduces to (3.1) when $\gamma_1 = \gamma_2$, is usually advocated on the ground that property income accrues mostly to higher-income and/or entrepreneurial groups who may be expected to have a lower marginal propensity to consume. Accordingly, γ_2 is supposed to be smaller than γ_1 and this supposition appears to be supported by empirical findings.

It is immediately apparent that (3.2) bears considerable similarity to hypothesis I discussed in this paper, i.e.,

$$C = (\alpha_1 + \alpha_2) Y + \alpha_3 A. \tag{3.3}$$

The main difference lies in the constant term which appears in (3.2) but not in (3.3), and in the fact that the wealth variable A in (3.3) is replaced in (3.2) by a closely related variable, income from wealth, P. We can avoid dealing with the first source of discrepancy by working with both hypotheses in first-difference form,

$$\Delta C = \gamma_1 \Delta Y + \gamma_2 \Delta P \tag{3.2a}$$

$$\Delta C = (\alpha_1 + \alpha_2) \Delta Y + \alpha_3 \Delta A. \tag{3.3a}$$

Equations (3.2a) and (3.3a) are quite useful since they allow a straightforward test of the usefulness of the Modigliani-Brumberg hypothesis as compared with the standard Keynesian one. We have already exhibited in Table 2, row (6), the results obtained by fitting (3.3a) to the data. In order to complete the test we need to estimate the parameters of (3.2a). If the standard Keynesian version is correct, the net worth variable in (3.3) and (3.3a) is merely a proxy variable for the return from wealth, P, and hence substitution of ΔP for ΔA should improve the fit. On the other hand, if (3.3) and (3.3a) are closer to the truth than (3.2), then the substitution of ΔA by a proxy variable ΔP should reduce the correlation.

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[23] See for instance the results of cross-section studies reported in [24] and [32].

[24] Keynes' own formulation (See [23, Book 3]) was considerably more general than that contained in equation (3.1).

The estimate of P needed for this test is given in [3].[25] The definition of consumption on which we rely, however, is somewhat different from that customarily used in the standard Keynesian formulation in that it includes the current consumption—depreciation—of the stock of consumer durables, while excluding expenditure for the purchase of such goods.[26] The results obtained for hypothesis (3.2a) are as follows:

$$\Delta C = \underset{(.07)}{.93\Delta Y} + \underset{(.29)}{.07\Delta P} \qquad R^2 = .86. \tag{3.4}$$

Comparison of this result with those reported in Table 2, row (6), strongly suggests that net worth is definitely not a mere proxy for current property income. While the coefficient of P is positive and smaller than that of Y as expected, this variable is much less useful than A in explaining the behavior of consumption. In fact, its contribution is not significantly different from zero.[27]

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[25] Our estimates of Y and P do not add up exactly to disposable personal income as usually defined because we include in disposable personal income contributions to, instead of benefits from, the social security system. However, this discrepancy is quite minor.

[26] Also, our data are in current dollars, while the standard Keynesian version of the consumption function is usually stated in terms of constant dollars.

[27] For the sake of completeness several other variants of (3.2a) were tested by adding variables that were included in the test of our hypothesis and which are consistent with the spirit of the Keynesian model. The addition of the variable $\Delta Y\frac{L}{E}$, which might help to sort out the effect of long run from that of purely cyclical variations in income, yields

$$\Delta C = \underset{(.18)}{.47\Delta Y} + \underset{(.13)}{.49\Delta \left(Y\frac{L}{E}\right)} + \underset{(.23)}{.17\Delta P} \qquad R^2 = .921. \tag{3.4a}$$

If we also include the dummy variable X to allow for possible shifts from the prewar to the postwar period, its coefficient is uniformly less than its standard error, and in general hypothesis (3.2) does not fare any better. This conclusion can be illustrated by the following result which is the most favorable to that hypothesis among the battery we have run:

$$\Delta C = \underset{(.14)}{.46\Delta Y} + \underset{(.14)}{.51\Delta \left(Y\frac{L}{E}\right)} + \underset{(.28)}{.26\Delta P} - \underset{(.39)}{.21X\Delta P} \qquad R^2 = .921. \tag{3.4b}$$

The fact that in (3.4a) and (3.4b) the coefficient of the variable $\Delta Y\frac{L}{E}$ is a good deal higher and statistically more significant than in the corresponding tests reported in Table 2 is readily accounted for by the high correlation between this variable and A which, in the absence of A, makes this variable act partly as a proxy for A. (The correlation in question is .93. See also footnote 16.)

These results, besides supporting our hypothesis, serve also to cast serious doubts on the conventional interpretation of the empirical coefficients of ΔY and ΔP in (3.2); namely, that incremental labor income is largely consumed while incremental property income is largely saved. For our tests of the Modigliani-Brumberg model indicate that consumption is quite responsive to variations in the market value of wealth, which, in turn, must largely reflect the capitalization of property income. Note, however, that the market valuation of assets will be controlled by expected long-run returns, say $\overline{P}$, which will tend to be a good deal more stable than *current* property income, P. We suggest therefore that the coefficient of P in (3.2) is small not because property income is largely saved but because short-run changes in P are dominated by transitory phenomena and hence are a poor measure of changes in the relevant long-run, or permanent, property income, which will be reflected far more reliably in the market valuation of assets. Put somewhat differently, the low coefficient of P does not imply a low marginal propensity to consume out of property income but merely a low propensity to consume out of transitory income. Correspondingly the extremely high coefficient of labor income in (3.3) is equally misleading, reflecting the fact that Y acts partly as a proxy for the permanent component of property income, $\overline{P}$.

One might be tempted to estimate the marginal propensity to consume with respect to permanent property income $\overline{P}$ by relying on the estimates of the coefficient of net worth in (3.3a) provided in Table 2, and on the relation

$$\overline{P} \simeq rA \quad \text{or} \quad A \simeq \frac{\overline{P}}{r}\cdot$$

Following this reasoning, the coefficient of $\overline{P}$ in the consumption function would be given by $\frac{\alpha_3}{r}$, where r is the rate at which the market capitalizes the return from assets. If we are willing to approximate r with the average realized rate of return on assets, then, from the figures given in [3], we find that r was about .04 in the prewar period and somewhat lower (around .03) in more recent years.[28] Combining this estimate with our estimate of α_3, which is in the order of .06, we seem to be led to the conclusion that the marginal propensity to consume with respect to permanent property income $\frac{\alpha_3}{r}$, far from being low, is actually well above unity.

This result may appear preposterous if judged in terms of the standard Keynesian framework underlying (3.2), with its emphasis on the relation between flows. It is however possible to interpret this result in terms of the Modigliani-Brumberg framework. For, in this model, wealth affects consumption not only through the stream of income it generates but also directly through its market value which provides a source of purchasing power to iron out variations in income arising from transitory developments as well as from the normal life cycle. It is therefore not surprising that this model implies a marginal propensity to consume with respect to assets, α_3, larger than the rate of return r (cf. Table 1), an inference which, as we have just seen, is supported by empirical tests. It should be noted however that $\frac{\alpha_3}{r}$ should not be interpreted as the marginal propensity to consume with respect to permanent property income in the same sense in which $(\alpha_1+\alpha_2)$ can be said to measure the propensity with respect to permanent nonproperty income, for it measures the *joint* effect on consumption of a change in property income, r constant, and of the accompanying change in assets. It is not possible to infer the two effects separately from knowledge of α_3 and of the average value of r. Although we cannot pursue this subject here, we wish to point out that the effect on C of a change in $\bar{P}$ will be quite different depending on the behavior of A and hence r, as $\bar{P}$ changes.

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[28] See, however, our comment below on the shortcomings of our estimate of P given in [3] as a measure of return on assets.

B. *Cyclical versus Long-Run Behavior of the Consumption-Income Ratio —Relation to the Duesenberry-Modigliani Consumption Function*

As is well known, one of the major difficulties encountered with the standard Keynesian consumption functions (3.1) or (3.2) lies in the constant term γ_0. This constant term is needed to account for the observed cyclical variability in the saving-income ratio, but it also implies a long-run tendency for the saving ratio to rise with income, which is contradicted by empirical findings. The lack of any positive association between income and the saving-income ratio in the long run, at least for the U. S. economy, was first uncovered by Kuznets, and has more recently been confirmed by the extensive investigation of Goldsmith [19], focusing on the years 1896–1949. In his summary recapitulation, he lists as the first item: "Long-term stability of aggregate personal saving at approximately one-eighth of income, and of national saving at approximately one-seventh."[29]

The consumption function proposed here is capable of accounting both for the long-run stability and the cyclical variability of the saving-income ratio. In order to exhibit its long-run properties, let us suppose that Y were to grow at a constant rate n, in which case Y^e can be taken as equal or proportional to Y. Suppose further that the rate of return on assets r is reasonably stable in time. Then the consumption function

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[29] Goldsmith [19, Vol. 1, p. 22].

(1.6) implies that the income-net worth ratio, Y_t^*/A_{t-1} will tend to a constant h, related to the parameters of the consumption function by the equation:[30]

$$h = \frac{n + \alpha_2 - \alpha r}{1 - \alpha}; \qquad \alpha = \alpha_1 + \alpha_2 \cdot \tag{3.5}$$

When the ratio $\frac{Y_t^*}{A_{t-1}}$ is in fact equal to h, then income and net worth grow at the same rate, n, and the saving-income ratio will be a constant given by:

$$\frac{S_t}{Y_t^*} = \frac{A_t - A_{t-1}}{A_{t-1}} \frac{A_{t-1}}{Y_t^*} = n \frac{1}{h} \cdot \tag{3.6}$$

Similarly, we find:

$$\frac{Y_t}{A_{t-1}} = \frac{Y_t^* - rA_{t-1}}{A_{t-1}} = h - r \tag{3.7}$$

$$\frac{C_t}{Y_t} = \frac{Y_t^* - S_t}{A_{t-1}} \frac{A_{t-1}}{Y_t} = \frac{h - n}{h - r} \cdot \tag{3.8}$$

Thus the model implies that if income fluctuates around an exponential trend the income-net worth ratio will tend to fluctuate around a constant level h, and the saving-income ratio around a constant $\frac{n}{h}$.

The empirical estimates reported in Section II suggest that α is around .7, and α_2 close to .06. The average rate of return, r, is much more difficult to guess. If we are willing to rely on the ratio $\frac{P}{A_{t-1}}$ for this purpose, then r would be around or somewhat lower than .04. But this

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[30] Under the stated assumptions we have $Y_t^* = Y_t + P_t$, and $P_t = rA_{t-1}$. Hence, saving can be expressed as

$$S_t = Y_t^* - C_t = Y_t + P_t - C_t = (1 - \alpha)Y_t^* - (\alpha_2 - \alpha r)A_{t-1}. \tag{a}$$

We also have $S_t = A_t - A_{t-1}$. Substituting this definition for S_t in (a), dividing through by A_{t-1}, adding and subtracting n, and then rearranging terms, we obtain

$$\frac{A_t - A_{t-1}}{A_{t-1}} = n + (1 - \alpha)\left[\frac{Y_t^*}{A_{t-1}} - \frac{n + \alpha_2 - \alpha r}{1 - \alpha}\right]. \tag{b}$$

Comparison of (b) above with equation (3.5) shows that if Y_t^*/A_{t-1} were larger than h, the second term in the right-hand side would be positive, and hence net worth would grow at a rate larger than that of income, n, causing Y_t^*/A_{t-1} to fall toward h: and conversely if Y_t^*/A_{t-1} were smaller than h.

This argument is oversimplified and incomplete, particularly since it ignores the interaction between the behavior of consumers and the production process in the economy. A more complete analysis of this growth process is given in Ando [2].

ratio is very likely to understate the true value for r, since the estimate of P given in [3] corresponds to the conventional definition of personal income and omits a number of items whose exclusion is appropriate for the standard Keynesian model but not for the Modigliani-Brumberg model. Among those items, the more important are imputed net rent on consumer durables and undistributed corporate profits.[31] These adjustments suggest an average value for r slightly over .04. If we further take for n, the rate of growth of income, a value in the order of .03, then from (3.6), (3.7), and (3.8) we obtain the following estimates for the various ratios under discussion: (i) total income to net worth, $h \simeq .2$; (ii) nonproperty income to net worth, $h - r \simeq .16$; (iii) saving to income, $\frac{n}{h} \simeq .15$. It can be seen that the first two of the above figures are in fact close to the values around which the ratios

$$\frac{Y_t^*}{A_{t-1}} \quad \text{and} \quad \frac{Y_t}{A_{t-1}}$$

fluctuate according to the data given in [3] while the third, the saving-income ratio, is consistent with the findings of Goldsmith reported earlier.

Needless to say, these calculations are very crude and are given here primarily to bring out certain interesting testable implications of the consumption function discussed in this paper. Among these implications the long-run stability of the ratio of net worth to income is particularly significant, for it paves the way for an explanation of the historical stability of the capital-output ratio in terms of the supply of capital, thereby challenging the prevailing notion that the behavior of this ratio is explained by technological requirements [1] [2] [5] [29].

As for the cyclical implications of our model, we need only observe that at any given point in time net worth A_{t-1} is a given initial condition. Hence, retaining for the moment the assumption that $Y^e \simeq Y$, (1.6) implies that the aggregate consumption function for any given year is a straight line in the C-Y plane with slope α and intercept $\alpha_2 A_{t-1}$. It is shown in Figure 2 for the year 0 as the line labeled $\bar{C}_0$, and looks like the orthodox Keynesian version. Yet, it differs from the latter in one essential respect, namely, that its intercept will change in time as a result of the accumulation (or decumulation) of wealth through saving. As we have shown in preceding paragraphs, so long as income keeps rising on its exponential trend, the growth in net worth will shift the function in such a way that the observed consumption-income points will trace out the long-run consumption function (3.8) represented in

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[31] The rationale for including corporate saving in property income and personal saving is given in Modigliani and Miller [36].

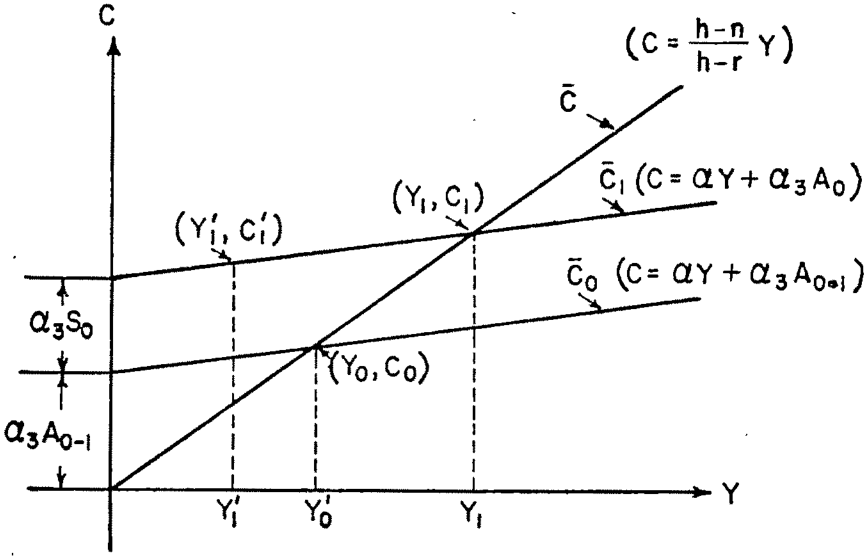

FIGURE 2. CONSUMPTION INCOME RELATIONS: LONG-RUN AND SHORT-RUN

our graph by the line $\bar{C}$ through the origin. This point is illustrated in Figure 2 for two years, 0 and 1. However, suppose that a cyclical disturbance caused income to fall short of Y_1, say to the level Y'_1. Then the consumption C'_1 given by the short-run consumption function $\bar{C}_1$ implies a higher consumption-income ratio and a lower saving-income ratio.

Thus, cyclical swings in income from its long-run trend will cause swings in the saving-income ratio in the same direction,[32] especially since the position of the function will not change appreciably when income is cyclically depressed below its previous peak due to the small or negative saving that would prevail.[33] After income has recovered beyond the previous peak, it may for a while rise rapidly as it catches up with its trend, running ahead of the slowly adjusting wealth. In this phase we may observe points to the right of $\overline{C}$, and the corresponding high saving will tend to make A catch up with Y.

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[32] This phenomenon will be further accentuated when we recognize the possibility that a cyclical fall in Y is likely to bring about a smaller change in Y^e. Also, because property income may be expected to fluctuate cyclically even more than labor income, the ratio of saving to total income will fluctuate even more than the ratio of saving to labor income. See footnote 30, equation (a).

[33] Some downward shift of the consumption function might occur even in the absence of dissaving, if there is some downward revaluation of the market value of assets, as the depressed level of property income tends to bring about less favorable evaluation of the long-run prospects for return from assets.

Because of this dependence of the value of assets on property income, the statement made earlier to the effect that A_{t-1} can be taken as a given initial condition in the year t is only approximately true. Also the relation $\Delta A_t = S_t$ does not hold in the presence of capital gains and losses. Note however that what is relevant in the present connection is the change in the value of assets in terms of purchasing power over consumption goods and not the change in terms of money value, which may be considerably more severe.

Thus the model advanced here may be expected to generate a behavior of consumption and saving which is very similar to that implied by the earlier Duesenberry-Modigliani type of hypothesis, in which consumption was expressed as a function of current income and the highest previous peak income (or consumption) [9] [30]. If we interpret the role of the highest previous income as that of a proxy for net worth, then the Duesenberry-Modigliani consumption function can be considered as providing a good empirical approximation to the consumption function discussed in this paper, and to this extent the empirical support provided for the Duesenberry-Modigliani type of hypothesis can also be considered as empirical support for the consumption function advanced here, and vice versa. At the same time the present model has the advantage that the hypotheses on which it rests are explicitly stated as specifications of the consumer's utility function. It is also analytically more convenient as a building block in models of economic growth and fluctuations, as we have endeavored to demonstrate in various contributions [1] [2] [3] [29] [31].[34]

APPENDIX

A. *Some Statistical Results for Earlier Years*

The following are the estimates obtained using data for 1900–28, excluding years 1917, 1918, and 1919. As stated in the text, the data used are very rough, and may not be compatible with the data for the period since 1929. The data and their derivation are described in Ando [2, App.], except for the adjustments needed for different treatments of the government sector. This adjustment is self-explanatory from the description given in [2]. The data presented in [2] are in turn based largely on [18] [19] [22] [27] and [42].

$$\text{(a)} \qquad \underset{(.134)}{C = .755Y} + \underset{(.020)}{.073A} \qquad R^2 = .995 \quad DW = 1.63$$

$$\text{(b)} \qquad \Delta C = \underset{(.180)}{.731\Delta Y} + \underset{(.037)}{.047\Delta A} \qquad R^2 = .44 \quad DW = 2.48$$

$$\text{(c)} \qquad \frac{C}{Y} = \underset{(.144)}{.505} + \underset{(.021)}{.112}\frac{A}{Y} \qquad R^2 = .51 \quad DW = 1.05$$

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[34] On the other hand, because up-to-date estimates of wealth are not readily available, at least for the present, some variant of the Duesenberry-Modigliani model may well be more useful for short-run forecasting.

B. *Biases in Estimating the Consumption Function by Regressions on Saving*

Suppose that true consumption c^* and true income y^* are related by a linear function (all variables being measured from their means),

$$c^* = \alpha y^* + \epsilon \tag{a}$$

and measured income and measured consumption are related to their respective true values by

$$c = c^* + \eta \tag{b}$$

$$y = y^* + \xi \tag{c}$$

where ϵ, η, and ξ are random variables. For simplicity, let us assume that ϵ is uncorrelated with y^*, η, and ξ; η and ξ are uncorrelated with c^* and y^*. We also have the definitions

$$s^* = y^* - c^* \tag{d}$$

$$s = y - c = y^* - c^* + \xi - \eta \tag{d$'$}$$

Using (a) and (d), we have

$$c^* = \frac{\alpha}{1-\alpha} s^* + \frac{\epsilon}{1-\alpha} \equiv \beta s^* + \epsilon'. \tag{e}$$

In order to concentrate first on the effect of errors of measurements, let us momentarily accept the unwarranted assumption that saving is equal to investment which in turn is truly exogenous. Under this assumption s^* can be taken as independent of ϵ' and therefore, if we could actually observe s^* and c^*, by regressing c^* on s^* we could secure an unbiased estimate of β from which we could in turn derive a consistent estimate of α. If however we estimate β by regressing c on s, then remembering that s is obtained as a residual from y and c, we obtain the estimate

$$\begin{aligned} \hat{\beta} &= \frac{\sum cs}{\sum s^2} = \frac{\sum (\beta s^* + \epsilon' + \eta)(s^* + \xi - \eta)}{\sum (s^* + \xi - \eta)^2} \\ &= \frac{\beta \sum s^{*2} + \sum \eta(\xi - \eta)}{\sum s^{*2} + \sum (\xi - \eta)^2}. \end{aligned} \tag{f}$$

The term $\sum \eta(\xi - \eta)$ arises from the fact that when there is a statistical error η in measuring consumption, there will be an error $-\eta$ in measuring saving, except in so far as this is offset by an error ξ in measuring income. This term will tend to be negative and introduces a downward bias into β. The term $\sum (\xi - \eta)^2$ in the denominator is the well-known result of an error of measurement of the independent variable, and it introduces an unambiguous bias towards zero into β.

However, as pointed out in the text, the assumption that personal saving is exogenous is completely unwarranted. In order to bring out the nature of the bias resulting from this misspecification, let us make the other extreme and equally unwarranted assumption that disposable income is truly exogenous. We can then regard y^* as independent of ϵ and by substituting (a) and (d) into (d$'$) we find

$$s = s^* + \xi - \eta = (1 - \alpha)y^* - \epsilon + \xi - \eta. \tag{g}$$

Equation (f) is then replaced by

$$\beta = \frac{\alpha(1 - \alpha)\sum y^{*2} - \sum \epsilon^2 + \sum \eta(\xi - \eta)}{(1 - \alpha)^2 \sum y^{*2} + \sum \epsilon^2 + \sum (\xi - \eta)^2}. \tag{h}$$

The presence of the term $\sum \epsilon^2$ in both numerator and denominator is the result of the fact, discussed in the text, that when there is residual error ϵ in the consumption-income relationship, there will be residual error $-\epsilon$ in the saving-income relationship. Because of the signs, this effect, too, will bias β downward.

Since all these biases are downward and there is no offsetting upward bias of any significance, it is not surprising that recent applications of this approach [39] [43] lead in a number of cases to a negative estimate of the marginal propensity to consume.

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PUBLIC POLICY PROBLEMS OF THE DOMESTIC CRUDE OIL INDUSTRY

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The domestic crude oil industry has been the object of much legislation and presently operates under (a) state production restrictions (prorationing), (b) federal controls (the Connally Hot Oil Act prohibits interstate commerce in oil and oil products that have been produced in violation of state prorationing laws), and (c) special tax considerations (percentage depletion). Prorationing and the Connally Act are expressions of a public policy which has emerged because the free competitive market has been unable to perform its primary function of husbanding existing oil reserves; while percentage depletion is often justified by arguing that in the absence of this tax provision, the free market would not allocate sufficient resources to exploration for new oil reserves.

For most of its history, the domestic crude oil industry has been characterized by wasteful production practices. Many members of the industry, who are normally among the most outspoken advocates of freely competitive markets, as well as many disinterested observers agree on the desirability of a public conservation policy. Investigators have, however, differed on what form this policy should take. Some observers have argued for some type of voluntary or mandatory production restrictions (prorationing) which would limit supply to demand at the going price, while others have advocated voluntary or mandatory field unitization, that is, a policy by which all the producers who draw crude oil from a single underground pool would form a unified management for the specific purpose of operating the wells in the field as a single production unit.

The state of the debate on the proper public policy for the domestic crude oil industry has been justly criticized in an important recent study as follows [3, p. 58]:

> Despite an abundance of statistical and historical information about the oil industry, dismayingly little will be found to have direct relevance to the public policy issues posed by the inquiry, and conclusions must be

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based on an informed judgment in which the intuition of different observers may lead them to widely different views.

In view of the analytical inadequacy of so much of the discussion, the purpose of this paper is to provide a complete model of the domestic crude oil industry. This model casts considerable light on the short-run question of the optimum level of production from existing facilities, as well as on the long-run question of incentives to add capacity by exploring presently nonproductive oil lands.

The first half of the paper uses a short-run model which takes the stock of production facilities as given. Under this assumption it is possible to show why the free competitive market improperly allocates oil production over time. It is also possible to derive simple economic criteria which can be used to formulate a public policy which will assure the optimum output of crude oil through time.

The second half of the paper deals with the longer-run question of the incentives to increase capacity by exploring presently nonproductive oil land. It is this long-run view which is relevant for the appraisal of the desirability of granting a tax concession in the form of percentage depletion to the crude oil industry. The major conclusion of this section is that percentage depletion primarily increases the income of landowners rather than encouraging exploration of nonproducing oil lands.

I. *Short-Term Expectations and Production Decisions*

We begin by examining the factors determining entrepreneurial decisions in the crude oil industry. The decisions can be classified as based on either short-term or long-term expectations. Decisions based on short-term expectations involve matters affecting daily output from existing wells; decisions to explore and drill new wells are associated with long-term expectations.

In analyzing the factors bearing on the decision process of the independent producers (who operate two-thirds of the oil wells in the United States[1]) we first assume no prorationing or percentage depletion. Then the effects of depletion and/or state production restrictions can be examined.

A. *Posted Prices*

Originally crude oil was traded on an organized exchange. By the turn of the century, however, most buyers of crude, following the action of the Standard Oil Company, purchased crude in the field under a posted price policy. Thus, when a purchasing company decided upon the price it was willing to pay, this price was posted on a board in the field where it could be seen by all who were interested. More recently, a few large integrated companies (who are also large crude producers) have announced through the press the posted price for crude oil in each field in which they purchase [3, p. 132].[2] When a price is posted in a field, all the producers of that field receive the same price for the same grade,[3] regardless of the volume purchased. This price is F.O.B. wellhead and does not include gathering and transportation costs.

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[1] There are at least 7000 companies operating 563,000 crude oil wells in 30 states [3, p. 12]. Some of these firms are integrated through all the stages of production, transportation, refining, and marketing, while others confine their activities to one or two stages. The overwhelming majority are not integrated at all.

The posted price expresses the buyer's judgment regarding the derived value of a complex raw material [3, p. 132]. The independent producer accepts the price as a datum. Analytically, the independent producer is in the same position as a seller in a purely competitive market, i.e., he faces a perfectly elastic demand curve.[4] Since the producer who maximizes profits will equate marginal prime cost to posted price, we must examine the nature of the cost function for crude oil production.

B. *The Cost of Crude Oil Production*

The total cost of crude oil production may be divided into supplementary costs which are here defined as all costs not directly associated with production, and prime costs, regarded as all costs associated with output changes.

For crude oil production, supplementary costs (really investment expenditures) consist mainly of (1) intangible exploration and development costs, (2) tangible exploration and development costs, and (3) leasehold costs.[5]

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[2] The posted price is also used as the accounting price for valuing the shipments of the integrated refiner's production department.

[3] Although crude oil is not a homogeneous commodity (it varies with sulphur content and specific gravity), we will assume for analytical convenience that only a single quality of domestic crude is produced.

[4] Cassady has emphasized the fact that posted prices are sometimes shaded and consequently actual price changes often precede posted price changes [2, pp. 119-20]. Cf. the *tatonnements* of the Walrasian system.

[5] Intangible exploration and development costs cover the expenses of drilling, providing roads to the well, the use of chemicals, etc.; these outlays which have no salvage value, may be, for tax purposes, treated as expenses during the year they are incurred [14, p. 1021]; but while following this practice for tax purposes, many firms capitalize these costs and subject them to depreciation charges on their financial statements and for managerial purposes. Tangible exploration and development costs, such as the cost of equipment and structures, approximate 30 per cent of total finding and development costs. They are always capitalized and subject to depreciation. Leasehold costs are subject to cost depletion on financial records.

Oil producing companies treat both depreciation and leasehold cost depletion charges

The prime costs of crude oil production consist mainly of: (1) operating costs, including factor costs of lifting the crude and special lifting costs for nonfree-flowing wells (e.g., gas or water injection), (2) royalties, and (3) user costs.

1. *Operating costs.* Approximately 20 per cent of total annual expenditures of the petroleum production industry are operating costs [3, p. 67, n. 13]. The variable costs of lifting oil from an underground pool vary with the type of well. Over 10 per cent of the wells in the United States are flowing under natural gas or water pressure [4, p. 284]. Another 20 per cent require pumping or some form of artificial aid to augment the outflow of crude. Finally, about ⅔ of all U.S. wells are "strippers,"[6] which barely cover production costs at prevailing prices [4, p. 284], and produce only about ⅙ of total U.S. production [3, p. 68]. Because state production restrictions affect only nonstrippers, however, this overestimates the importance of strippers in the potential output of the industry.

For the nonstripper wells factor costs are small relative to sunk costs. Under secondary recovery methods (i.e., the injection of either water or dry gas into the reservoir to increase crude flow), unit variable costs will be higher than on wells under natural drive, but investment in durable equipment will rise by even a greater proportion, so that the ratio of sunk to out-of-pocket costs is still high [3, p. 67]. Lifting costs are rather large per barrel of output only for stripper wells.

Thus, for most nonstrippers, total operating costs are generally small until capacity is approached. Empirical evidence suggests that marginal operating costs tend to decline in low ranges of output and are then small and nearly constant over a wide range.[7] As capacity is approached the marginal operating cost curve becomes very steep [4, p. 284].

To fix ideas and represent the estimated facts, Figure 1 shows an elongated U-shaped marginal operating cost curve (s_1) for a typical well. The ordinate distance (cd) between the X-axis and the minimum marginal operating cost will vary with each well and will depend on

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on a unit-of-recovery basis. This accounting practice has created some confusion as to whether these charges are fixed or variable costs. For example, DeChazeau and Kahn declare that "such charges are clearly variable" [3, p. 72, n. 23]. Nevertheless, it is our contention that exploration, development, and leasehold costs are sunk rather than prime costs, and they do not affect short-run production decisions. If producers expected the present price to continue into the future, it would pay them to produce even if marginal revenue did not cover book depreciation and depletion charges. DeChazeau and Kahn have confused book depreciation and depletion charges with the user cost inherent in all raw materials. This latter concept is developed in the following analysis.

[6] "Strippers" are old, marginal wells which require a good deal of mechanical pumping to bring oil to the surface.

[7] For a comprehensive discussion of the economies and diseconomies associated with output changes, see [13, Ch. 10], and for some cost data, see [13, pp. 495-96].

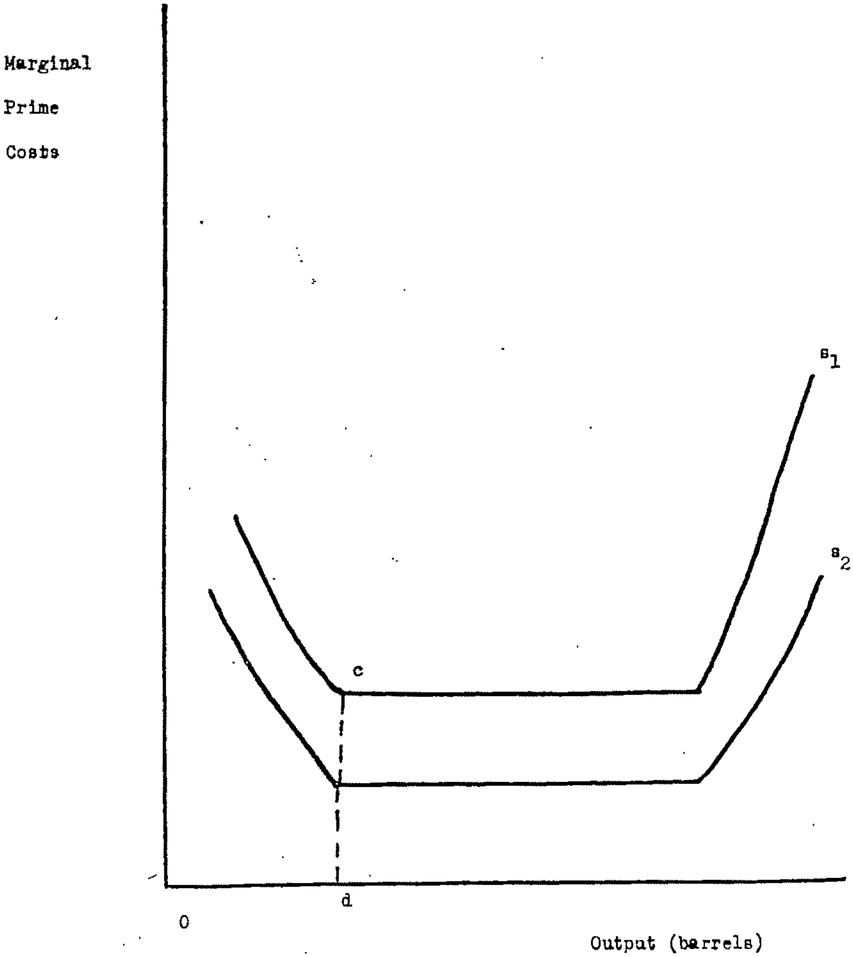

FIGURE 1. TYPICAL MARGINAL PRIME COST FUNCTIONS

the geological conditions in the reservoir, the age of the well, etc. For a young, highly productive, free-flowing well, for example, the distance, *cd,* may represent only a few cents per barrel.

Average and marginal operating costs of stripper wells are much higher than those of nonstrippers. In the short run, the marginal operating cost curve of a stripper will be U-shaped, but it will start from a much higher level than the nonstripper's curve, and it will tend to rise rapidly. At present prices, most stripper wells can profitably produce only four or five barrels per day [4, p. 284].

Assuming the industry draws upon output from the lowest-cost wells first, then the industry marginal operating cost curve, the lateral summation of the individual well curves, would tend to be relatively flat at

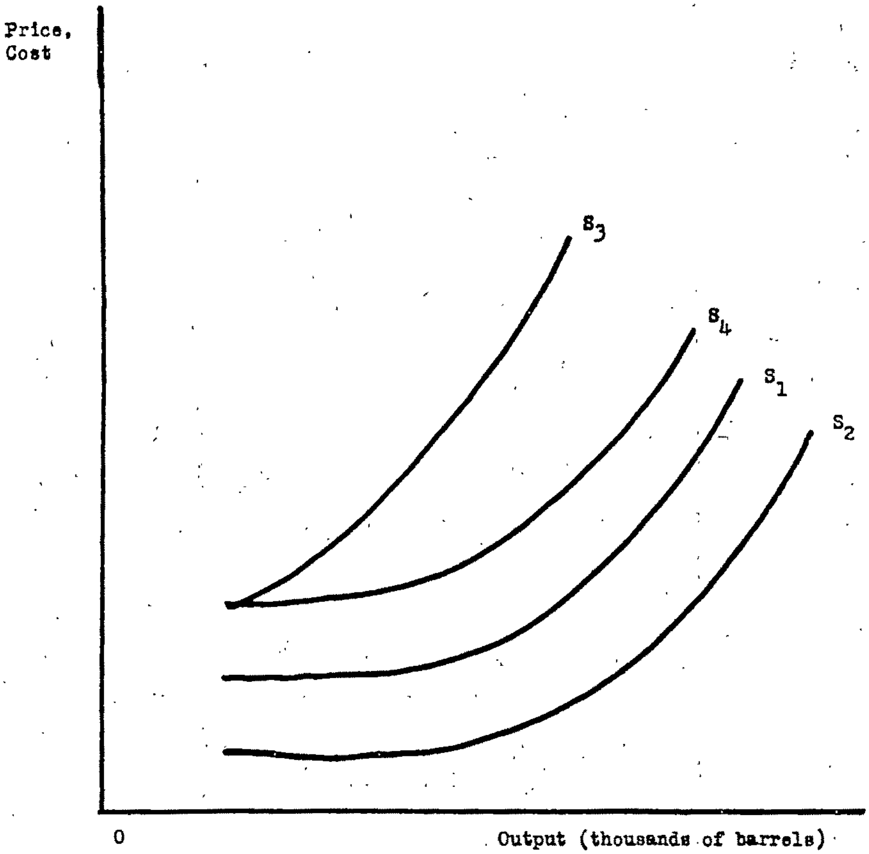

Figure 2. Alternative Industry Supply Curves

low levels of production and rise rapidly as more costly wells are brought into production (S_1 in Figure 2).

2. *Royalties*. Royalty payments to the mineral rights owner are usually fixed by contract before development is begun.[8] Normally a fixed proportion of gross revenue obtained from the well (typically 12½ per cent) is paid out in royalties, i.e.,

$$R = 1/8PQ \tag{1}$$

where R represents total royalty payments per period, P is the short-run supply price of crude, and Q denotes the barrels of output per period from a well. The marginal royalty cost is simply ⅛ of the supply price.

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[8] Royalties are an important component of the short-run supply function. Nevertheless, since royalties provisions are fixed at the outset and depend upon expectations of the future income stream from the well, royalties are, in the long run, price-determined rather than price-determining (see Section II below).

If marginal royalty and marginal operating costs were the only marginal prime costs, and since short-run profit maximization would require the firm to equate the sum of these marginal costs to price, then the short-run supply function for the firm would be obtained by elevating the marginal operating cost curve by 1/7, or 14 2/7 per cent, so as to include marginal royalty costs in the supply price.

3. *User costs.*[9] If user cost phenomena are ignored, then the industry marginal operating cost curve (S_1 in Figure 2), elevated by 14 2/7 per cent to cover marginal royalty costs, would be the short-run supply curve for the industry. But in fact marginal user cost dominates supply decisions in the crude oil industry.

User costs have been defined as "the change in the value of facilities of the firms due to operating as opposed to not operating during the week" [9, p. 58]. In the crude oil industry, one can identify three types of user costs: (a) user cost inherent in all raw materials (U_m); (b) user cost relating to ultimate recovery (U_u); and (c) user cost due to the rule of capture (U_c).

a. User cost inherent in all raw materials. For any particular producer oil in the ground is a stock. The more used today, *ceteris paribus*, the less there will be available tomorrow. Consequently, for production to occur under conditions of profit maximization, marginal revenue must not only cover marginal operating and royalty costs, but must also cover the highest present value of marginal profits given up by producing this week rather than later. Hence,

$$U_m = \beta Q \tag{2}$$

where β is the present value of the greatest potential profit per barrel because of an expected future price (or cost) change. Since

$$\frac{dU_m}{dQ} = \beta, \tag{3}$$

if prices are expected to rise substantially in the future ($\beta > 0$), the marginal user cost due to this factor will be positive and will tend to restrict present production. On the other hand, the expectation of a price decline will make user cost due to this factor negative, and hence expand present production. Thus, dU_m/dQ is the basic economic conservation guideline for deciding whether or not to substitute future consumption of oil for present use, i.e., for evaluating future benefits against the present costs of a slower rate of use.

b. User cost of ultimate recovery. Given the technology and the costs of production, only a fraction of the total volume of crude oil trapped in an underground pool can be brought to the surface during the productive life of the reservoir. Moreover, for any given set of geological conditions the quantity of crude that can ultimately be recovered from the pool will depend on the rate at which the oil is brought to the surface. In general after some point, the faster the rate of production, the lower the volume of crude that can ultimately be recovered.

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[9] In the typical theoretical analysis of supply price, user cost has often been neglected despite Keynes' warning that user cost phenomena should not be ignored in the analysis of the firm [6, p. 55, n. 1].

Public discussions about the conservation of crude oil have usually centered on the question of the quantity of crude that can ultimately be recovered from an oil pool. The economic criterion for conserving oil is different from the geological rule to avoid waste. The geologist desires that production plan which yields the maximum total volume of output over the life of the reservoir. The geologist has therefore developed the concept of MER (the Maximum Efficient Rate of Production). MER is a rate of output (determined by engineering and geological factors) which will maximize the ultimate recovery from the reservoir. The geologist desires a daily production rate which is equal to, or less than MER.[10] The economist, in contrast, seeks to ascertain the oil production plan which maximizes benefits relative to costs over time. If the economist's plan tends to abandon oil in the ground which might, under a slower rate of output, be ultimately recovered, there is no economic waste, only geological waste.[11]

Assuming equality between (1) the social and private rates of time discounting, and (2) social and private costs of production, it will be shown that unitization of oil fields and competition between sellers will promote economic conservation and will tend to result in the optimum allocation of resources over time.[12]

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[10] In their recent analysis of oil conservation, McKie and McDonald have defined MER as that rate which maximizes the difference between discounted future returns and discounted future costs of production, or the rate of production which maximizes profits [8, pp. 107-8]. While the concept of MER appears prominently in the jargon of the petroleum industry, it should be stressed, in contrast to the position of McKie and McDonald, that MER is not an economic concept. As one well-known authority in petroleum engineering noted:

> . . . [MER] is that rate which will realize the most efficient use of reservoir energy . . . the oil producer should strive to maintain this most efficient rate of production as nearly as possible, but it should be recognized that this is not necessarily the most profitable rate [13, p. 474].

For further discussion of MER, see [1, pp. 836-37] [3, p. 123, n. 2] [10, p. 30] [12, pp. 125-26].

[11] During the Second World War, for example, because of the scarcity of certain grades of crude, some oil fields in the United States produced at rates in excess of MER [1, p. 1147] [13, p. 577]. It was taken for granted that the social benefits of more production at that critical period greatly exceeded the social costs of lower ultimate recovery.

[12] Because of uncertainty in estimating future demands and costs there are many practical difficulties in assessing the maximum income stream of an oil property. DeChazeau and Kahn argue that "the mere statement of the problem . . . serves to support a general disposition to leave these complicated calculations to the self-interest of businessmen in competitive markets" [3, p. 236].

Let T be the total number of barrels of oil in an underground pool, while S_r is the amount that can ultimately be recovered from the pool for any given production rate. Given the state of technology and the geological conditions, there will be a maximum ultimate recovery of S_0 barrels of crude (where $T > S_0$), if the well is always operated at MER.

Empirical engineering studies have shown that if the crude oil is brought to the surface at a rate of flow that is less than MER, the gas pressure is dissipated as the tubing in the well acts as an oil-and-gas separator; hence, " 'dead' oil is left in the reservoir" [11, p. 66], i.e., ultimate recovery is reduced [13, p. 577]. Nevertheless, if a daily output level is desired which is lower than that which would be obtained by operating the well continuously at MER, it is possible to achieve this desired level without reducing ultimate recovery. The well can be operated at the maximum efficiency rate of flow until the desired number of barrels is obtained; then the flow from the well can be shut off

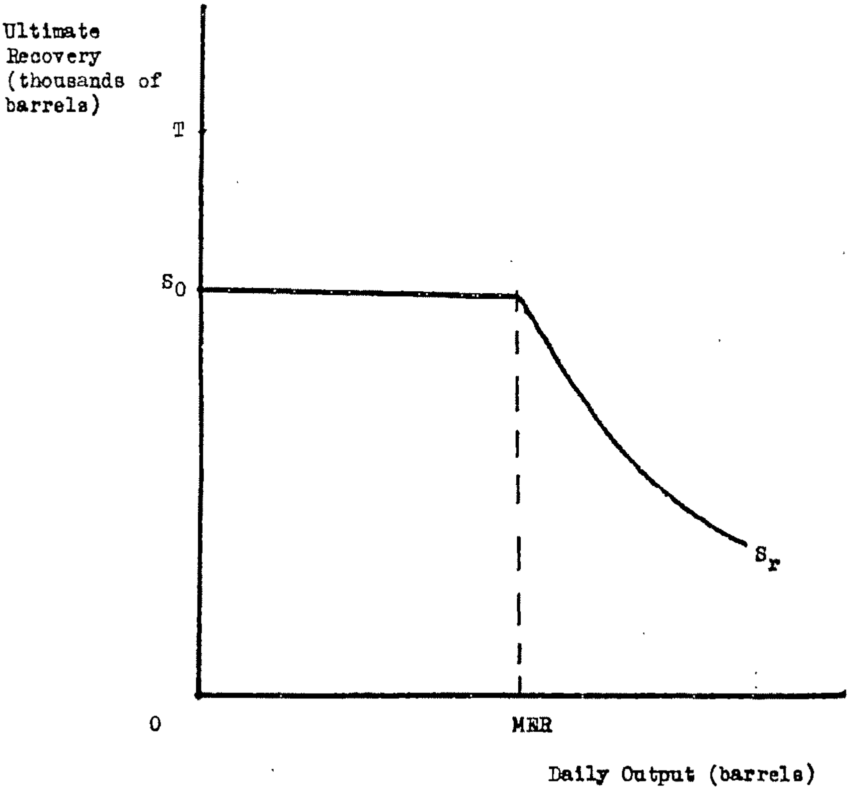

FIGURE 3. THE ULTIMATE RECOVERY FUNCTION

for the remainder of the day. Accordingly, for any output level less than MER, $S_r = S_0$.

If the rate of production exceeds MER, then S_r is a decreasing function of the rate of production. (Schematically this can be represented by the S_r curve in Figure 3.) Hence, $(S_0 - S_r)$ is equal to the number of barrels which ultimately would be sacrificed if the rate of production exceeds MER. The user cost of ultimate recovery is:

$$U_u = \gamma(S_0 - S_r) \tag{4}$$

where γ is the discounted net value of a barrel of oil which could have been produced in the future. The marginal user cost of producing above MER is:

$$\frac{dU_u}{dQ} = -\gamma \frac{dS_r}{dQ}. \tag{5}$$

If production exceeds MER, then $dS_r/dQ < 0$, and under conditions of profit maximization, marginal revenue will have to cover the net value of the barrels of oil that are sacrificed. Thus, dU_u/dQ is the basic economic conservation criterion for assessing the cost of reducing the total crude available over the life of the reservoir relative to the benefits of having more crude for present consumption.

c. User cost of the rule of capture. It is the common law of the rule of capture that makes the supply function of oil unique by including in it a large *negative* marginal user cost. Moreover, it is the rule of capture which prevents dU_m/dQ and dU_u/dQ from acting as the regulators of economic conservation in the market place.

Under the rule of capture, whatever a landowner can withdraw from an underground oil pool (which transcends surface property lines) is his to take and keep, irrespective of its effects on his neighbor. If there is more than one producer from a common pool, then adjacent producers are in danger of losing all "their" oil to their neighbors. Hence, it is in the self-interest of each producer to attempt to bring to the surface as much crude oil as quickly as possible, before his neighbor draws off more oil. In terms of marginal user costs, each additional barrel of oil produced reduces the possible loss of the value of a barrel of output to one's neighbor as the underground inventory is depleted.[13] If S_r is the number of barrels of oil which can ultimately be recovered from a given pool, while Q is the number of barrels of crude which the individual producer brings to the surface, then $(S_r - Q)$ represents the total possible barrels of oil that could be lost to the neighbor. Thus, the user cost due to the rule of capture is:

$$U_c = \gamma(S_r - Q). \tag{6}$$

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[13] Here is a classic example where marginal private costs are less than marginal social costs.

The marginal user cost is:

$$\frac{dU_c}{dQ} = \gamma \frac{dS_r}{dQ} - \gamma. \tag{7}$$

Since dS_r/dQ is either equal to zero (at or below MER), or less than zero (above MER), therefore $dU_c/dQ < 0$.

From equations (3), (5), and (7) we obtain the total marginal user cost curve as:

$$\frac{dU}{dQ} = \frac{dU_m}{dQ} + \frac{dU_u}{dQ} + \frac{dU_c}{dQ},$$

or

$$\frac{dU}{dQ} = \beta - \gamma \frac{dS_r}{dQ} + \gamma \frac{dS_r}{dQ} - \gamma = \beta - \gamma. \tag{8}$$

Thus the marginal user cost associated with the rule of capture not only tends to offset any positive costs associated with the marginal user cost inherent in all raw materials, but it also completely nullifies any positive marginal user cost associated with sacrificed ultimate recovery. Accordingly, under the rule of capture, producers will ignore the economic conservation criteria implied in dU_u/dQ and dU_m/dQ, and hence resources will not be optimally allocated over time by the market.

Moreover, the rule of capture makes the crude oil market inherently unstable with respect to new discoveries since there may be no positive price which can bring supply into equilibrium with demand in the short run. For example, assuming that initially the current price is expected to continue into the future so that $\beta = 0$, then equation (8) becomes

$$dU/dQ = -\gamma. \tag{9}$$

Consequently the inclusion of marginal user costs in the marginal prime cost curve will tend to pull the latter down (s_2 in Figure 1); and the industry supply curve will also be lower (S_2 in Figure 2). Thus, when a large new field is discovered, the number of gushers and high-producing free-flowing wells in the industry will increase and depress the supply curve. If the demand for crude is relatively price-inelastic, there may be a precipitous price decline. Expectations of further price declines could then develop (i.e., $dU_m/dQ < 0$) and the supply curve would be further depressed. Under such circumstances there may not be any crude price above zero which would restore equilibrium.[14]

Even though under such circumstances the energy-consuming industries may, in the short run, be able to buy their energy supply at "bargain" prices, the long-run supply situation is obscured. The chaotic price situation after a large discovery may lead to incorrect planning decisions in the consuming industries regarding their future energy supply costs. Such a situation is socially undesirable and would require governmental interference in the market to restore order.

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[14] With the discovery of the huge East Texas field in 1931, crude prices toppled from a level of over $1.00 a barrel to a recorded price of 10 cents per barrel in the field [3, p. 67]. Even at this low price, the market was not cleared.

C. *Regulation to Assert the Public Interest*

Basically, two different types of proposals have been advanced for altering short-term production decisions. They are (1) state-enforced production controls limiting production either to (a) market demand, or (b) MER; or (2) compulsory field unitization.

1. *Market demand prorationing.* The production control scheme presently used in most large oil producing states is some variant of market prorationing. Its stated purpose is to avoid waste, which is defined as production in excess of market demand (at the going price).

In Texas, for example, the state regulatory commission estimates market demand for crude oil and then subtracts (1) estimated production not under its authority (imports and production from other states) and (2) production that cannot be curtailed for economic and political reasons (mainly stripper wells and wells using secondary recovery methods). The residual is the total which will be prorated among fields and among wells within each field "in a manner calculated to preserve equity among producers and to prevent any well from producing beyond its Maximum Efficient Rate (MER)" [3, p. 123].

Thus the production path is altered so that the most efficient wells are not used intensively. Accordingly, the industry's marginal operating cost curve is shifted to the left, while the elastic portion of the curve bears the brunt of the production restraints (S_3 in Figure 2). Furthermore, the impact of the rule of capture is attenuated, for each landowner is restricted either by technology and costs (in the case of strippers) or by the state regulatory commission as to how much oil he can draw off the common pool. Thus, under market prorationing where $\beta = 0$ and $S_0 - S_r = 0$, we are entitled to infer that the relevant marginal user cost function is given by equation (9). Nevertheless, since producers of low-cost wells are not permitted to maximize profits, supply is determined by administrative decree within the constraint that each well produces at a point where marginal revenue is either equal to or exceeds marginal prime cost.

2. *MER prorationing.* If production prorationing limited wells only to MER, the low-cost wells would expand production and would supply a much larger proportion of the output than they would under market prorationing. Equation (8) would be the relevant marginal user cost function, although many low-cost producers might still be operating at

a point where marginal revenue exceeded marginal prime cost (at MER).

From an economic standpoint, it should be apparent that prorationing does not provide a scheme for optimally allocating crude oil resources over time. Market prorationing does diminish the effects of the rule of capture, but the social costs of using the inefficient wells to capacity while limiting the output from efficient operations is obviously uneconomic. Market prorationing is but a sop to practicality for it avoids the regimentation of a host of small enterprises. This, in fact, may be its best defense. One might argue that if the administrative costs outweigh the benefits of the MER prorationing scheme, then the present arrangements are tolerable.

Nevertheless, even if prorationing merely limited well production to MER, resources would not necessarily be optimally allocated over time, even though geological waste were to be prevented. The rule of capture would lead all producers to operate at MER and, when β is positive but less than γ, it would force producers to sell at a price below the value to them of holding oil underground for future sale [3, p. 239, n. 17]. On the other hand, governmental regulation would prevent producers from exceeding MER, even when the social benefits of such action exceeded the social costs. Thus, prorationing provides insufficient guidelines for the economic conservation of crude oil.

3. ***Compulsory field unitization.*** Some investigators have suggested compulsory field unitization as a way of avoiding the socially undesirable effects of the rule of capture. Unitization would "require the organization of companies or cooperatives in which all surface owners would share on an equitable basis" [10, p. 45]. The advantage of such an operation would be to void the rule of capture and allow the flow from the individual wells to be determined by other economic criteria. Under unitization $dU_c/dQ = 0$; consequently the marginal user cost function becomes:

$$\frac{dU}{dQ} = \beta - \gamma \frac{dS_r}{dQ} \cdot \tag{10}$$

Assuming constant price and cost expectations, when unitized field production is equal to, or is less than MER, marginal user cost would be zero. Thus, for profit maximization, marginal revenue would have to equal marginal operating plus marginal royalty costs. If production exceeds MER, dS_r/dQ is negative and therefore the second term in equation (10) is positive, implying that there is a positive cost involving lower ultimate recovery for these output levels. Nevertheless, under unitization, production could be pushed past MER in arriving at a profit-maximizing decision if the existing market conditions indicated that the marginal revenue would cover the marginal costs of the resulting geological waste.

On the other hand, if future prices are expected to differ from present prices, then under unitization a current price in excess of marginal operating plus marginal royalty costs at or below MER may not provide a sufficient incentive to produce at that level if there is reason to believe that the price tomorrow will be sufficiently higher to compensate for limiting the amount sold today. Under these circumstances, some oil reserves would be conserved for more valuable uses tomorrow at the cost of less valuable uses today.

Unitization completely eliminates the negative marginal user cost due to the rule of capture, and allows the two economic conservation criteria, the marginal user cost inherent in all raw materials (dU_m/dQ) and the marginal user cost of ultimate recovery (dU_u/dQ), to govern production decisions. Moreover, unitization would ensure that the lowest-cost wells were used intensively. Under unitization, the industry supply curve might be represented as S_4 in Figure 2. (The ordinate difference between S_4 and S_1 reflects marginal royalty costs and any marginal user costs that are associated with the present price and a given set of expectations.)

Nevertheless, supply price will fluctuate with discoveries, technological break-throughs, and cost conditions. Barring adverse price expectations, however, a short-run stable equilibrium at a positive crude price is possible.

D. *The Crude Oil Buyer*

Normally buyers have a vested interest in obtaining a commodity at the lowest price possible. Because of the depletion tax provision and the fact that many integrated refiners are large buyers[15] of domestic crude as well as producers and hence sellers of crude, some purchasers may have a desire for a high rather than a low price of crude.[16]

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[15] Most integrated refiners are not completely self-sufficient in regard to their supply of crude oil but must buy some crude in the field. The largest refiners are usually more nearly self-sufficient than the average. The top 20 "major" oil companies (18 of which are fully integrated) controlled about 55 per cent of U.S. production in 1955, and had over 80 per cent of domestic refining capacity [3, p. 18]. Hence, they accounted for over half of total crude sales (not including royalty sales) and over four-fifths of domestic crude purchased in 1955.

[16] DeChazeau and Kahn note that "there are at least four important considerations that incline large vertically integrated companies toward higher crude oil prices, even though they produce only about one-half of their domestic needs. . . . (1) . . . our tax laws make a dollar of earnings from the production of crude oil, *at home or abroad,* more valuable than a dollar earned from the sale of products. (2) World crude oil prices are geared to the American level, and price policies must take into account self-sufficiency ratios world-wide. (3) Production is the stratum of the industry most effec-

An integrated oil firm's after-tax profits are substantially affected by the balance between production of crude oil and refinery operation. Percentage depletion accords a preferential tax treatment to profits from production of crude oil which is not enjoyed by refining, marketing, or transportation profits. With an effective depletion rate of 27½ per cent, profits from the production of crude oil are taxed at an effective rate of 37.7 per cent, whereas profits earned in the other processes are taxed at the ordinary 52 per cent rate under existing tax laws [3, p. 222, n. 49].

Thus, as DeChazeau and Kahn have shown, a rise in the price of crude unaccompanied by a rise in the prices of petroleum products would increase the after-tax profits of any integrated oil company whose net self-sufficiency ratio[17] was greater than 77 per cent [3, p. 222]. Considering only domestic production, in 1956 there were eight firms who would benefit from a crude price rise without any increase in product prices. Despite the fact that the number of firms that would benefit from a rise in the crude price is small, it is just these firms that are typically the posted price leaders in many of the major producing fields. Thus, in regional markets where there is a good deal of oligopsony power, the oligopsonists may have a vested interest in a high crude price.

Moreover, if at least a part of the crude price increment can be passed on in higher product prices, then the breakeven self-sufficiency ratio is reduced. For example, if 50 per cent of the price increment is passed on, then all refiners with a net self-sufficiency ratio of more than 38½ per cent (in 1959, 27 out of the largest 34 refiners) would show an after-tax gain [3, p. 222]. Assuming that one-half of a barrel of crude can be refined into gasoline, then if the crude price increases by $.25 per barrel, the price of gasoline at the pump would have to rise by less than $.0075 per gallon for these 27 enterprises to obtain an increase in after-tax income per barrel of crude.

Price-movement data show that the typical pattern is for the crude oil price to follow product price increases much more promptly than product price declines [3, pp. 414-22]. It appears, therefore, that increased earnings due to product price increases are quickly passed down the integrated chain to the tax-sheltered production segment, as even oil companies with low self-sufficiency ratios tend to benefit from such action.

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tively controlled in global terms. . . . (4) Production is the stratum that *must* be controlled if final product prices are to be stabilized above costs" [3, p. 442, n. 37]. Thus, Cassady is incorrect when he argues that *all* refiners stand to gain from a drop in domestic crude prices [2, p. 127].

[17] The net self-sufficiency ratio is defined as the ratio of the firm's domestic crude production (net of royalty payments) to the amount of crude used in the firm's refineries.

The reaction to a product price decline is, however, different. If this decline in income is passed back to the production department, firms with high self-sufficiency ratios will tend to lose after-tax earnings. These companies can, however, resist downward pressures on crude prices that might be exerted by less self-sufficient refiners, because the former enterprises are usually the largest buyers in many of the domestic oil fields. Given the desire of the independent producer to obtain the highest price for his product, posted price oligopsony leadership rather than buyers competition will prevail in the crude market during the periods of slack demand.

Even the refiner with a relatively low net self-sufficiency ratio, however, may desire to maintain crude prices because of the ubiquitous phenomenon known as crude oil trading. A crude oil trade is a reciprocal agreement between two or more companies, in which one company agrees to deliver a given quantity and quality of crude oil to another refiner in return for a similar accommodation by the latter. Although the number of barrels exchanged in a given period may be the same, the number of dollars involved is almost never identical, because of quality differences in the crudes that are traded. Many refiners engage in a volume of trading transactions which exceed their own needs in order to obtain exactly what the refinery requires.

Besides eliminating costly cross-hauling from distant fields to a particular firm's refinery, crude oil trading provides the refiner with an exchangeable supply of crude as a hedge against price fluctuations. It is in the self-interest of each refiner to prevent the price of crude which he controls from declining, so that he may be able to trade at advantageous terms. Since refiners are usually the posted price leaders in the fields where they are the important producers as well as buyers, there is a built-in downward rigidity in crude oil prices.

Thus, most integrated refiners even if they are not completely self-sufficient have a vested interest in a high rather than a low crude price (cf. [3, p. 225]).

E. *Short-run Production Decisions—Summary*

Assuming that private and social rates of discount, and private and social costs of production are equal, compulsory field unitization will solve the problem of economic conservation on the supply side of the market. Unitization may, however, increase the oligopsonist strength of the large integrated refiners who are usually the large producers in each field. Thus, public policy would be required to assure that the price that refiners were willing to pay for crude accurately reflected the demand of consumers of petroleum products. Such a policy might involve either the divestiture of production departments by integrated refiners [10, pp. 119-123], or the removal of the depletion allowance, or both.

The incentive to move the profits of integrated firms to their tax-sheltered production departments by raising the posted price would be removed by eliminating the percentage depletion of the tax law. In view of the emotional reactions aroused by this issue, it is important to analyze the economic effects of the current depletion allowance and its reduction.

II. *Long-Term Expectations and the Decision to Explore*

Percentage depletion is a deduction from taxable income available to all producers of mineral resources. For petroleum, percentage depletion is computed as 27½ per cent of gross revenue from the property, but the deduction may not exceed 50 per cent of net income in any one year.[18] Hence, except for the net income restraint, percentage depletion is merely a negative ad valorem excise tax; and the effects of changes in the rate can be analyzed with the usual tools of tax incidence.

The usual belief of most advocates, as well as opponents, of the present depletion allowance is that it tends to raise after-tax profits in crude oil production vis-à-vis other industries, and therefore it encourages entrepreneurial decisions to explore and drill for oil on presently nonproductive oil land. Industry spokesmen maintain that national security requires an increased allocation of resources to domestic exploration and development [14, pp. 986-87] and that percentage depletion has been essential in encouraging such exploration.[19]

Actually, there are two alternative effects that can be attributed to the depletion allowance. These are: (1) to encourage additional exploratory activity, and (2) to increase the value of mineral holdings. Although most investigators have been quick to emphasize the link between percentage depletion and exploratory activity, the effect on the value of mineral holdings have typically been given short shrift. Consequently, it is important to indicate the conditions under which effect (1) and/or effect (2) will be significant.

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[18] The ability to charge off intangible exploration and development costs as expenses in the year they are incurred helps the producer to avoid the 50 per cent net income constraint. Most of these intangible costs are incurred before any oil is produced. These costs, therefore, may be used to offset other taxable income. Once the well is producing, since most of the costs have already been written off, the difference between net and gross income is quite small. If intangibles had to be capitalized and written off over the producing life of the well, the net income in any production period would be much smaller and might severely limit the tax deduction from the depletion allowance.

[19] For example, the preamble of the American Petroleum Institute's Statement of Policy on Percentage Depletion (November 1961) states that "this provision has been a key factor in maintaining high-risk exploration for new oil and gas reserves."

A. *Percentage Depletion, the Value of Undeveloped Oil Land, and the Incentive to Explore*

The effect of percentage depletion on reallocating resources and raising property values can be analyzed with the help of a simplified model. Assume that all income is taxed proportionately and that initially there is no depletion allowance. If two assets, A and B, have equivalent present costs and net income streams, then an individual will be indifferent as to purchasing either A or B. If the tax law is then changed so that a specific proportion of the future income stream of asset A is made tax exempt, then asset A will have a higher present value than before, while the present value of B remains unchanged. The individual will now be willing to incur greater costs to obtain A than before.

Two extreme situations may result. If, in the first case, asset A is in perfectly elastic supply, resources will be diverted towards the production of A, there will be an expansion in the output of the industry, and the product price will decline until the rate of return on A would equal the rate of return on B [15, p. 379]. If, in the second case, asset A is in perfectly inelastic supply then the willingness of individuals to pay more for asset A will, in a competitive environment, merely lead them to bid up the price of A until the expected rate of return on A is the same as can be earned on B. The original owner of A receives a windfall in the form of a higher economic rent for the use of his asset. The individual investor will receive the same rate of return on A or B; therefore, he will obtain the same rate of return as he would have received before the depletion allowance was instituted. Since the original owner receives the windfall while no benefits accrue to the individual operator, there is no incentive to move resources.

In reality, most capital assets fall somewhere between the two extreme cases. Given the depletion allowance, the more nearly perfect is the elasticity of supply of mineral lands, the greater will be the stimulus to reallocate resources, and the smaller will be the effect on the value of mineral holdings. On the other hand, the more inelastic the supply of mineral lands, then the greater will be the landowners' windfalls, while the additional exploratory activity will be minor. In the case of the domestic oil industry, therefore, the significance of percentage depletion will depend on the elasticity of supply of oil lands, i.e., on the supply conditions of those properties from which producers expect to be able to obtain crude oil.[20]

[20] Although many have argued that percentage depletion has encouraged exploration in the domestic oil industry, none has given a more lucid analysis of this position than Harberger, who argues that the depletion allowance misallocates resources in the oil industry. In his careful study, however, Harberger clearly recognizes that in certain

B. *Leasehold Costs and Royalties*

In the domestic oil industry, leasehold costs approximate 30 per cent of exploration costs [7, p. 155], while royalties represent 12½ per cent of total revenue. In the long run both of these costs are price-determined rather than price-determining (Cf. [3, p. 175]), and therefore the major costs in oil exploration and development are economic rents.

A brief description of the institutional arrangements for paying for the use of oil lands will be helpful in understanding the supply conditions of oil lands and in indicating why the rent payments take the dual form of lease bonuses and royalty payments. In the typical oil lease, the surface rights remain in the possession of the owner, who may use the land for agriculture or other purposes. The lessee is only granted the right to enter upon the land and explore, and to produce and sell oil if possible. In return for this privilege, the lessee pays the landowner a royalty, consisting of an agreed percentage of the oil produced or its gross value, plus an initial bonus, and an annual rental as long as the property is nonproductive. Once oil is discovered in profitable quantities, the lease remains in force as long as oil is produced.

Although the typical royalty is 12½ per cent, both the royalty percentage and the lease bonus are open to negotiation. Royalties as high as 50 per cent and bonuses of $1000 an acre have been offered on property where production is assured by surrounding development [13, p. 170]. Typically, the lease bonus depends on "how promising the prospect looks to the lessee, and on competition from other parties"

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mineral-producing industries, percentage depletion will encourage little or no additional exploration and that its main effect may be merely to increase the value of mineral holdings [5, p. 444]. Nevertheless, Harberger interprets the conditions in the domestic oil industry to be such that oil lands clearly are assets of the elastic-supply case [15, p. 379]. The fact that the annual volume of domestic oil exploration is large leads Harberger to conclude that the reason for all this exploratory activity is percentage depletion [5, p. 443].

My own acquaintance with the crude oil industry leads me to interpret the facts differently. In any discussion of the effects of percentage depletion, the emphasis must be on the encouragement of *additional* exploratory activities and not on the total volume of exploration. Since exploration has always been relatively large in the domestic industry, while the precursor to the present depletion allowance was not enacted until 1916, and since many foreign oil companies have done a good deal of foreign exploration over the years even though in many foreign countries there is no depletion allowance, it would appear that the large annual volume of domestic oil exploration cannot be used as prima facie evidence that percentage depletion has significantly encouraged exploration of oil lands.

Testifying several months after his study was published, Harberger comes closer to my own position when he hints at the inelasticity of supply of oil lands. For example, he states, in "... some real sense our reserves are the amount that is underground in this country and ... the faster we try to use them up, the worse shape we are going to be in in the future," i.e., the long-run supply of crude is perfectly inelastic [15, p. 379], and crude oil "is a wasting asset," i.e., oil is not reproducible [15, p. 400].

[7, p. 545], while, it is often claimed, the royalty rate appears to be "extraordinarily rigid" [8, p. 108, n. 5].

McKie and McDonald have indicated that with perfect competition and perfect foreknowledge, landowners could extract, in the form of rents, all the after-tax profits of the producing firms [8, p. 108]. In the absence of uncertainty, the lease bonus would equal the present value of the net income stream and there would be no need for royalty payments. Given uncertainty and large dispersions around the expected present value of an oil property, the lease bonus is only a partial measure of the expected mean value; the royalty payment allows the producer and the landowner to share the risks inherent in the wide dispersion about the mean value.

Lease bonuses and royalty percentages on producing properties have been fixed by a contract and reflect the producer's and landowner's estimates of the capitalized value of oil in the ground (taking into account the after-tax value of the depletion deduction) at the time of the signing of the contract.[21] These costs are essentially fixed for the life of the contract even if the depletion allowance is changed. Existing contracts on nonproducing properties, on the other hand, can, at the option of the lessee, always be renegotiated to reflect the expected capitalized value of oil in the ground at some future time.

The landowner of any undeveloped oil property exhibits a perfectly inelastic supply function for his mineral rights—there is no alternative use. Thus, the lease bonus and royalty payments are, as McKie and McDonald intimate, Ricardian rent payments. Since a reduction in percentage depletion is equivalent to the levying of an ad valorem excise tax, it follows that if the depletion allowance is reduced, (1) the after-tax capitalized value of all oil properties would decline proportionately; (2) producers would lower their bids for all nonproductive acreage; and (3) the tax could be passed back completely to those landowners who were not protected by existing contracts.

Thus, a reduction in the depletion allowance would not, in itself, change the expected future income stream, net of lease bonuses and royalty payments, on undeveloped oil properties; consequently, in the long run, there should be no reduction in the incentives to explore nonproductive oil lands.[22] The future relative distribution of income between producers and landowners would be altered. On presently undeveloped properties, the landowners' potential income share would be reduced.

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[21] As an illustration, assume perfect foreknowledge in a competitive economy having initially no depletion allowance and a 50 per cent corporate profits tax. If a 25 per cent depletion rate is then established, the after-tax value of this tax deduction would be equal to 12½ per cent of the expected income. Consequently, the producers would, under competitive conditions, bid up the price of nonproductive acreage by 12½ per cent. Is it coincidental that the typical royalty in the midcontinent area is ⅛th?

[22] Of course those producers who presently have leases on nonproductive acreage would

C. *Depletion and Production from Existing Facilities*

Although a change in the depletion allowance will not significantly affect decisions to explore based on long-run expectations of the net income stream of presently nonproductive acreage, there will be some short-run effects.

Landowners of presently producing properties are protected by contracts so that the additional tax cannot be pushed back on them. Industry spokesmen argue therefore, that any increase in taxes due to a reduction in the depletion allowance would be passed forward to the consumer. Basically the industry spokesmen are claiming that, given production techniques and factor prices, a reduction in the depletion allowance will cause the short-run supply curve to shift upwards. The ultimate incidence of this rise in the supply function will depend on the price elasticities of supply and demand. Industry representatives (in and out of Congress) believe that the supply schedule is highly elastic, while the demand for all petroleum products except gasoline is elastic. Owing to the lack of substitutes, the price elasticity of demand for gasoline is normally considered to be rather low. Thus it is argued that the gasoline consumer will bear the full brunt of the rise in the supply schedule; the pump price would rise by as much as 5 cents per gallon; and the industry would recoup the additional tax payments via higher product prices [14, p. 1004].

This analysis assumes that there would be no change in crude oil import quotas or state production restrictions as the domestic industry supply curve shifted upward. These suppositions are questionable on both political and economic grounds. Certainly any attempt to raise the pump price of gasoline by 5 cents a gallon would induce a public demand for a removal of import quotas.

Moreover, if the tax increase were not passed forward, many of the high-cost stripper wells would have to be abandoned. As a consequence, even under the present prorationing scheme, production from the more

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have to write off the original lease bonus as a loss if they wanted to renegotiate the contract for the property. If a producer decides to terminate his lease, then he will have to compete with all other producers for a new lease for the property. He will have to pay the new market price which, after a reduction in percentage depletion, will be, *ceteris paribus,* less than the original price (in terms of bonus plus royalties) by an amount equal to the discounted value of the reduction in the depletion allowance. Since the expected net income stream will not have changed, there will be no reduction in the incentive to explore for the industry even though some producers suffer losses because of previously paid lease bonuses.

efficient wells would increase. Thus, the short-run supply curve of the industry could shift outwards rather than upwards.

Of course, if a policy of compulsory field unitization were adopted, then removal of percentage depletion would assure that the resultant industry supply curve (S_4 in Figure 2) would be based on rational conservation principles. The resulting product prices cannot be determined a priori, but they are not likely to exceed current market prices.

To summarize, the major proposition emanating from this long-run analysis is that, given the inelastic supply conditions of nonproductive oil properties, percentage depletion tends mainly to increase the value of mineral holdings. As a consequence, the depletion allowance is primarily an ad valorem subsidy to mineral rights owners who have no alternative use for their resource.[23] Moreover, we have shown in Section I that, in the short run, percentage depletion gives many integrated refiners a vested interest in higher crude oil prices. Thus, it would appear that there is ample justification based on both short-run and long-run considerations for the reduction or elimination of percentage depletion from the tax structure. Only on the basis of equity considerations could a continuation of percentage depletion be defended.[24]

If the depletion provision were altered, it might be desirable to mitigate the possible adverse effects on production income on producing properties. This may be accomplished by either (a) maintaining existing depletion allowances on presently producing wells, or (b) reducing the depletion rate slowly on such wells.

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[23] In the absence of prorationing and percentage depletion, the short-run supply curve for the firm would be the sum of the marginal operating and marginal user cost curves elevated by $14\frac{2}{7}$ per cent to cover marginal royalty costs. Introduction of an effective depletion rate of $27\frac{1}{2}$ per cent with a corporate tax rate of 52 per cent would induce a downward shift in the firm's supply curve of 14.3 per cent, since the depletion allowance is an ad valorem subsidy with an after-tax value to corporations of 14.3 per cent of the supply price. (If the firm is unincorporated and the owners face a marginal personal income tax rate which exceeds 52 per cent, then the depletion allowance would be worth more than 14.3 per cent.) Hence, the marginal royalty cost (which shifts the supply schedule upwards) and the depletion allowance (which shifts the supply schedule downwards) tend to offset each other. The present marginal royalty payment closely reflects the present marginal value of the tax deduction due to depletion. As a result, most of the subsidy to the producer is passed on to the landowner.

[24] Despite the steady stream of propaganda on the need for the depletion tax concession in order to stimulate exploration, it is unquestionably recognized even by the proponents of this special doctrine that they are protecting their income position. Their intuition here has been keener than the reams of apologetic "scholarly" support for the allowance.

These proponents have often used equity arguments to justify percentage depletion. Those equity considerations most often emphasized are: (1) after-tax rewards should be greater in oil production than in other business operations because crude oil production is a more risky undertaking than other business ventures, and (2) oil is a wasting asset, i.e., the industry is selling its "capital assets on an installment basis" and therefore its net revenue should not be looked upon entirely as taxable income [14, p. 988-91].

It is not the intent of this paper to make any value judgment on these equity considerations. Harberger has, however, argued "It cannot have been the intent of Congress to make owners of mineral deposits richer at the expense of the rest of the community" [5, p. 444].

Alternative (a) would, in effect, hold an umbrella over producers who would otherwise suffer losses because of unexpected changes in the tax laws. Since percentage depletion has been under attack by Treasury officials and members of Congress for more than a decade, producers must have been well aware of the risks involved when they entered into lease agreements. With alternative (b), if the average life of an oil well is about ten years [14, p. 963], then a partial shelter for producers may be provided by scheduling reductions in the depletion rate over a ten-year period, with small reductions occurring in the early years and larger ones in later years.

III. *Conclusions*

Our analysis has shown that proper guidelines for the economic conservation of oil reserves can be formulated in terms of the marginal user cost inherent in all raw materials plus the marginal user cost of ultimate recovery. Unfortunately, the marginal user cost associated with the rule of capture prevents these other user costs from performing their allocative function, and as a result the free competitive market cannot properly husband oil reserves. Conservation policy must (1) eliminate the user cost associated with the rule of capture and (2) assure a free competitive market between crude oil buyers and sellers. Adoption of such a policy would promote industry operations more nearly consistent with the optimum allocation of resources over time than ensues from present legal and institutional practices.

Furthermore, our analysis has indicated that, in the long run, percentage depletion does not significantly encourage the allocation of additional real resources to exploring presently nonproductive oil lands. The depletion allowance is primarily an ad valorem subsidy to mineral rights owners. A reduction in the depletion allowance will not reduce the incentives to explore undeveloped oil lands. Hence, except perhaps on equity grounds, it is hard to sustain a defense of this favored tax treatment in our tax laws.

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SOCIALIST OPERATIONAL PRICE SYSTEMS

By ALEKSY WAKAR AND JANUSZ G. ZIELIŃSKI*

Depending on their relation to prices, two kinds of incentive systems can be distinguished: (a) incentives which concern the physical product and which are therefore independent of prices, e.g., piece-wage systems; (b) incentives which concern the value product and which are therefore partially dependent on prices. Bonuses for fulfilling the gross output or value-added plan and profit-bonuses belong to this second group. Our analysis will be confined to the second group of incentives.

I. *Gross Output Incentives and Prices*

Until very recently, the most widely used incentive for managers of enterprises in socialist countries was a bonus for fulfilling the plan of gross value of output (*valovaya produktsiya*). An appraisal in value terms requires that output be aggregated according to specified rules. In practice, aggregates are calculated in money terms, and prices are, as a rule, equal to the average total cost of the branch of industry plus a given percentage of profit.

The experience of socialist countries has shown beyond any doubt that bonuses for fulfilling (and overfulfilling) the plan of gross output have had decidedly negative effects on socialist production, the more so because of existing pricing practices. The current method of pricing (average total cost of the branch of industry plus 5 per cent profit) tends to approximate the value relations between different products. Product A which requires twice as much resources as product B is also twice as expensive. By pricing in this manner, the Central Planning Board attempts to limit the demand for product A in production and consumption and tries to encourage the demand for product B. It is easy to see, however, that so long as gross output bonuses are paid and current pricing practices are adhered to, managers will not follow the intentions of the Central Planning Board because it is to their advantage to choose expensive inputs and to substitute product A for product B [3, pp. 155-66]. We may label this a case of "Gresham's Law *à rebours*." Therefore, when a bonus system for fulfilling the gross output plan and a system of average cost-plus pricing are used together, they disorganize socialist production. In order to remedy this contradiction, either the price system has to be adjusted to the incentive system which is being used, or the incentive system must be made compatible with average cost-plus pricing.

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If for the sake of argument we disregard practical difficulties and the prevailing way of thinking in regard to price ratios, we may consider changes in the price system as theoretically feasible. We want to stress, however, that in the end we shall not propose that the gross output bonus system be "improved" by adjusting the system of pricing in an appropriate manner. We merely want to point out those changes in the price system which must be made if gross output bonuses are to be used effectively as an incentive device for plan fulfillment.

First, let us determine the buyer prices which would induce buyers (enterprises) to meet the objectives of the Central Planning Board.[1] If we want to prevent enterprises from overfulfilling the plan for, say, 100 per cent woolen cloth at the expense of 40 per cent woolen cloth, it would be advisable to equalize the prices of 100 per cent woolen yarn and of 40 per cent woolen yarn. The smaller the price difference between various grades of yarn, the less the danger that enterprises will violate their assortment plan. The conclusion that, in order to prevent enterprises from taking the easy way out, input prices must be similar or identical applies especially to raw materials, since their value is transferred into the product during one production cycle (process). But it also applies to machinery and equipment. The higher the depreciation rates and the share of depreciation allowances in total cost, the more pressing is the need for price equalization.

Second, let us look at the seller prices which would lead sellers to meet the objectives of the Central Planning Board. In order to render gross output incentives effective, only the prices of inputs have to be equalized. Different types of output (e.g., regular watches and waterproof plus shock-resistant watches, dacron suits and woolen suits, etc.)

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[1] The buyers are here, of course, the state enterprises and not the individual consumers. We are concerned in this paper with so-called "operational prices" only, which are used within the socialist sector for stimulating producers—sellers and buyers—to plan fulfillment. In the socialist economy there are also so-called "programming prices" used for construction of the plan and so-called "consumers' prices," which have to equalize supply and demand, and at the same time, to influence consumption patterns in the direction socially desirable (as seen by the Central Planning Board) [4, pp. 12-79].

usually require different production processes and different production expenditures. If enterprises are to be induced to fulfill their production plans under these conditions, prices for different kinds of output have to take these facts into account. If we were to apply the policy of equal prices to seller prices, we would put a premium on assortments with a small share of value added because they can be produced more easily, the bonus for plan fulfillment being the same. However, if seller prices were made proportional to average total cost, input prices being the same, we would put a similarly undesirable premium on assortments with a large share of value added because they would yield larger gross output bonuses. Price differentiation according to value added would make this premium even larger. To avoid both of these consequences, we have to find a price ratio that would discourage enterprises from concentrating on assortments which are easy to produce as well as on assortments which yield larger gross output bonuses. We may expect that such a seller price ratio would range between unity and a ratio of prices which is proportionate to the ratio of average total costs.

For illustration, we may use a numerical example of regular watches and waterproof plus shock-resistant watches. The temptation to utilize expensive inputs has been eliminated by equalizing input prices, and it is assumed that the material input is the same in quantitative terms. Thus production costs differ only because of the difference in value added:

	Regular Watch	Waterproof Shock-resistant Watch
Average transferred value	10	10
Average value added	10	20
Average total cost	20	30

If we were to price according to value added, our price ratio would have to be 1:2. If we were to price according to average total cost, our price ratio would have to be 2:3. To avoid favoring either the production of regular watches or the production of waterproof, shock-resistant watches, our price ratio would have to fall between 2:3 and 1:1. As an example, we may assume the ratio of 2.5:3, with seller prices of 30 for regular watches and of 36 for waterproof and shock-resistant watches. However, this is only one of many possibilities which fall within this range, and the question remains: which ratio of seller prices, precisely, induces fulfillment of the assortment plans for regular watches and waterproof, shock-resistant watches?

We are afraid that there is no general, theoretical answer to this question. A solution has to be found by means of trial and error. We may expect, however, that such a solution depends in large measure on the ratio of value added.

We have thus far found certain rules for price-setting within the framework of gross output incentives. Buyer (input) prices, as a rule, are to be equalized. Thus we eliminate the incentive to use expensive means of production as an easy method of fulfilling the gross output plan. Equalized buyer prices, however, does not solve the assortment problem. For its satisfactory resolution, seller prices have to be set so that they cover (industrial) average total cost plus "society's share" (accruing to the state, e.g., in the form of turnover tax) and so that seller price ratios range between unity and the ratio of average total costs, the precise ratios to be found by means of trial and error.

Within the framework of gross output incentives, these methods of pricing can be used effectively by the Central Planning Board as instruments of economic policy in affecting methods as well as directions of production, since they enable the Central Planning Board to strengthen administrative orders with material incentives. So far as methods of production are concerned, the Central Planning Board can induce any branch of industry to use desired inputs by *increasing* the prices of these inputs. In the case of machinery and equipment, demand also depends on depreciation rates and on rates of interest for fixed capital. By raising these rates, the Central Planning Board can further stimulate demand for machinery and equipment. So far as directions of production are concerned, the Central Planning Board can induce enterprises to exert special efforts in producing certain products or kinds of products by fixing the ratio of seller prices appropriately in their favor. Generally speaking, equal buyer prices are "neutral" in the sense that they induce indifference between inputs, and certain seller price ratios, to be found by trial and error, are "neutral" in the sense that they induce indifference between outputs. In the case of neutral price ratios, the Central Planning Board has to rely on administrative orders only. Any deviation from these neutral price ratios, however, provides an incentive in a particular direction, and price differentiation can thus be used by the Central Planning Board as a tool of economic policy.

Although we thus can discover prices which "neutralize" or "balance" the negative influence of gross output bonuses on the fulfillment of assortment plans for inputs as well as for outputs there are serious objections to the implementation of such a system of prices:

1. The required price structure violates so-called common sense. The prices of goods which "everywhere and always" were and are

expensive (e.g., 100 per cent woolen yarn) would have to be equal to the prices of relatively cheap goods (e.g., 40 per cent woolen yarn). The prices of goods bought and sold within the state sector would have to be fixed at ratios which would deviate far from those which we are accustomed to view as proper. The habit of thinking that price ratios have to, or ought to, correspond to the "value" ratios cannot be overcome easily. Adherence to this habit is not astonishing in view of the several thousand years of experience by which it has been formed [2, App. I by F. Engels]. Indeed, one may even wonder whether this habit can be overcome at all. For this reason, it may be better to look for an incentive system which would be compatible with the more "traditional" price system.

2. Within the framework of gross output incentives, we cannot induce the effective use of resources, even if prices are manipulated in the way suggested, because the system as such does not provide incentives for "economizing" in the sense of minimizing the use of manpower and of capital. This deficiency is attributable to the fact that the enterprises are required to maximize gross output. By fixing prices appropriately as described previously, we may be able to eliminate many of the negative allocative effects of the gross output incentive system, but we do not create incentives for improving the technical production coefficients. Within the framework of gross output incentives, we cannot expect "initiative from below" on the part of enterprises and/or branches of industry with a view to economizing the use of means of production. When prices are given, economizing has to be induced from above, for instance in the form of plan targets for cost reductions, input norms, etc.[3]

3. Within the framework of gross output incentives, we cannot avoid using two different price systems within the socialist sector, viz., a buyer price system and a seller price system. This requirement, however, holds true for other incentive systems as well, as we shall see in Sections II and III.

4. The practical difficulties of devising and operating a price system which is compatible with gross output incentives are evidently enormous. In view of the other objections to such a price system, it does not seem worth the effort necessary to implement it.

The aim of our analysis is not, however, to defend the system of gross output incentives, but to derive requirements for an effective price system corresponding to it. As a by-product of this discussion, the relativity of the law of supply and demand has become conspicuously apparent. Such obvious relationships as that between price and demand (the higher the price, the smaller the demand) hold true, in fact, only under certain institutional arrangements. Changes in the system of stimulating producers (e.g., a change to gross output incentives) affect the relation between price and demand (e.g., in the case of gross output incentives, an increase in the price of an input will increase the demand for that input). However obvious this fact may be, it is worth while to emphasize that a given relation depends upon a given institutional setting, because of the tremendous power of habit which is connected with that relation. This habit of thought is probably the main factor which explains why the Central Planning Board has made use of, and continues to make some use of, gross output incentives in combination with more or less "normal" price ratios and why it has practiced a price policy which has assumed more or less "traditional" price-demand relations.

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[3] In practice, gross output incentives are usually supplemented by incentives to economize inputs per unit of output, e.g., in the form of bonuses for diminishing unit cost. In this paper, however, we do not analyze the "mixed" incentive systems with bonuses based on many targets, as, e.g., achievements in raising the level of gross output, diminishing unit costs, improving the quality of products, introducing technical developments, etc.

II. *Profit Incentives and Prices*

At the present time only a few economists would defend an incentive system based on gross output. Rather, the profit-sharing system for workers and bonuses based on profit for management are advocated. The incentive system based on profit also consists of two parts: (1) bonuses based on profit achieved (or planned), and (2) a system of prices for means of production and final goods designed to determine the relative profitability of individual enterprises and branches of industry.

Can there be contradictions between profit incentives and the existing price system? Very likely yes. Only one (or a few) price systems can stimulate enterprises to behave in accordance with plans as to "what" and "how" to produce; and the existing system of operational prices does not meet the necessary requirements.

Within the framework of profit incentives, the system of operational prices bears some resemblance to the system of prices under "market socialism" of the Lange-Lerner type, except for these fundamental differences:

1. In the theoretical model of market socialism prices constitute an internally consistent system which, on the one hand, enables the CPB to achieve the optimal allocation of resources, and on the other, in conjunction with bonuses based on profit, stimulates enterprises to work towards plan fulfillment.

In existing socialist countries which are using the method of direct economic calculation (DEC),[3] the prices together with profit incentives have to stimulate the enterprises to plan fulfillment. However, as we have argued elsewhere [5] [6, pp. 83-91] [7], prices cannot be, and are not, used for economic verification of technical coefficients, but can and have to be used to enforce the universal application of methods of production that the CPB deems appropriate. Both in the theoretical market socialism model and in DEC, operational prices coupled with a given incentive system have an empirical character. They have to be adjusted according to the supply-demand situation and must not be based on any a priori rule of price determination (e.g., average total costs of branch of industry plus 5 per cent profit).

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[3] The method of economic calculation applied in existing socialist countries we call direct economic calculation (DEC) because in this type of calculation one computes and

2. In theoretical market socialism there is only one price facing both buyers and sellers. This is one of the basic requirements of this model. The one-price principle is essential for achieving the technical efficiency of production. Different prices for a given good can and do exist only in the sphere of consumption. With DEC, the situation is quite different, for the one-price principle is not one of its cornerstones. It seems to us that the principle of double prices—different for buyers and sellers— must be adhered to, if prices are to be used effectively as part of the incentive system.

Let us begin with the prices of means of production. The necessity for two different systems of operational prices, one for buyers and one for sellers, is the result of the external character of production methods in DEC.[4] Because of the external character of methods of

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coordinates directly the physical magnitudes; e.g., the supply of steel which is a function of existing capacities and other material and technical conditions, with demand for steel, which is also the result of certain technical calculations (input norms multiplied by volume of production) when desired final products are defined. Three basic elements are coordinated directly in this type of calculation: (1) existing material and human resources, (2) desired final products in accordance with the preferences of the CPB, and (3) technical coefficients of production. This type of calculation is known and described as material balances and/or input-output analysis.

In DEC we have prices, wages, etc. but they do not play an active, balancing role: in DEC the supply and demand of steel are not—*ceteris paribus*—a result of the price of steel; demand for labor is not a function of the wage level, and prices and wages do not fulfill the equilibrium conditions. In DEC there is—as a rule—a direct coordination (balancing) of physical quantities without active use (i.e., to determine supply and demand) of value categories such as prices, wages, the rate of interest.

This is obvious enough and generally recognized. Unfortunately the same cannot be said about conclusions following from it, or about the model of economic process which applies where the magnitudes expressed in monetary terms are not used as the basis for economic choice but rather as the way of representing the inputs and outputs when the aims and methods of production are given [5] [6, pp. 83-92] [7].

[4] One of the most fundamental features of DEC is the external character of methods of production. Within the framework of DEC there is no mechanism for economic verification of technical coefficients (methods of production). The "fundamental assumption" of input-output analysis (and the same, of course, applies to material balances) is that required input per unit of output is technologically determined and can be considered as a structural constant. When we treat the technical input coefficients as independently given parameters, we assume that they are independent of factor prices, and we eliminate from

production there is little reason to expect that one price can properly stimulate both sellers and buyers to follow the methods of production prescribed for them in the national plan. These planned methods of production are not internally consistent in the national economy as a whole and are not fully coordinated with the price structure. The latter evolves historically; among its many parents were the efforts to use prices to stimulate increased production in bottleneck areas.

In such a situation one should not be astonished by rather general contradictions between methods of production prescribed by the CPB and the methods of production implied (and stimulated) by relative prices. To resolve these contradictions one needs a double-price system which differentiates between producers and users, or a turnover tax calculated to achieve the same result.

The contradiction of the interests of sellers and buyers can apply to the general price level as well as to the individual prices of means of production.

In the sphere of consumers' goods there is also a need for different prices for sellers and buyers (the latter are now individual consumers). Two main reasons for this need are:

1. The CPB is, as a rule, active in the sphere of individual consumption. It has definite preferences regarding the desired structure of consumption because of social welfare and other considerations. To effectuate this policy, within the framework of free consumers' choice, the level and the structure of consumer goods' prices play an important role. As long as the total volume of consumer demand does not call for an amount of resources in excess of that allocated to industries producing consumers' goods, in a planned economy there are no serious reasons for the CPB—irrespective of the type of economic calculation actually applied—to be constrained in its consumer goods price policy by the level and structure of the costs of production of individual commodities. There are no persuasive reasons for "cost fetishism" in any type of economic calculation because the costs of production are more or less incidental, as incidental as the level and pace of technical progress in different branches of production, factor endowment in a given country, etc.[5]

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this model of general equilibrium the substitution principle of marginal productivity theory. This can be considered its fundamental weakness [1, pp. 408-9].

Since the methods of production used in DEC are external to the system, they are not verified within the system, so that the best methods of producing a given output cannot be chosen, but are taken from outside the system and constitute unverified data. They are so-called "traditional" methods of production, i.e., methods of production used in the past, or so-called "advanced" methods of production, usually taken from the practice of more advanced countries and used as data for plan-building by the country under consideration [5] [7].

[5] Of course, consumers' sovereignty is abolished here. But let us assume for the moment

2. If it is true that there are no valid reasons for cost fetishism under any method of economic calculation, it is especially true under DEC. As we have argued elsewhere [5] [7], in DEC the price ratios, as a rule, do not reflect marginal rates of transformation. Costs based on these prices, then, obviously must also deviate from marginal rates of transformation. In such a situation it would be a serious mistake to allow the cost structure (via retail prices) to influence the structure of consumption.[6]

We have said above that within the framework of DEC two systems of operational prices are needed for the effective use of prices as a part of a profit-incentive system. Let us discuss them in more detail and also introduce the concept of "margin of tolerance" which, in many cases, may render different prices for buyers and sellers unnecessary. The margin of tolerance can be defined as an area around the existing price within which price changes for buyers and/or sellers do not induce the enterprises to change existing methods of production and product-mix. As long as the margins of tolerance of buyers' prices (P_b) and sellers' prices (P_s) are overlapping, the need for a system of two distinct sets of operational prices does not exist.

The magnitude of the margin of tolerance in different product groups and in the same product group, in different enterprises of buyers and sellers, may vary. The very existence and actual magnitude of the margin of tolerance is a result of several factors:

1. Differentiated pressure of administrative orders. In DEC one of the main reasons for the existence of the margin of tolerance is the system of administrative orders—quantitative ("What to produce?") influencing P_s, and qualitative ("How to produce?") influencing P_b.

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that differences in price ratios between, say, Poland and the United States as shown in Table 1, are the result of *real* differences in marginal-cost ratios. Would it not be risky to say that one or the other structure of consumption, resulting, *ceteris paribus*, from the given price structure, is better than the other? If there are no objective criteria to determine what structure of consumption is better than another, the CPB has no reason to be much worried that, because of random elements in the consumer price structure, it is doing any harm. Moreover, in many cases it has definite opinions about the desired structure (and volume) of consumption, and price policy allows it to achieve its objectives without limiting the freedom of consumers' choice [6, pp. 134-75] [7, pp. 100-2].

Table 1

Retail Price Ratios (1961)

	U.S.A.	*Poland*
Ready-made suit—one lb. of ham	50:1	60:1
Can of sardines—one telephone call	2:1	40:1
Cup of coffee—tram ticket	.5:1	10:1
Drip-dry shirt—scientific book	1:1	18:1
Stretch socks—cinema ticket	1:1	4:1

[6] Elements which have to be considered to determine the level and structure of retail prices are discussed in [4, pp. 65-78] and [6, pp. 134-70, 179-90].

Under the system of administrative orders any deviation from planned tasks not resulting from factors beyond the enterprise's power, represents an illegal act. This undoubtedly inhibits deviations from the plan that may arise from the desire for material gains. The size of the margin of tolerance attributable to administrative orders depends in part on the actual pressure of administrative orders. As we know from experience, the actual pressure of administrative orders usually differs by branches of industry and by product groups. It is especially great in branches or products of so called "high priority."

2. Achieved level of social integration also has an influence on the magnitude of the margin of tolerance. Under *ceteris paribus* assumptions, the magnitude of the margin of tolerance is proportional to the achieved level of social integration. A high level of social integration means that the producers identify the planned goals with their own and therefore do their best to achieve them even if the actual price structure makes certain deviations profitable.

3. The absolute and relative magnitude of financial gains realizable by plan deviation obviously constitutes an important parameter determining the magnitude of the margin of tolerance. The pay-offs of plan deviation depend on many variables; among others, on the relative share of bonuses in the total income of management personnel. In the situation, not unknown in Poland a few years ago, in which bonuses run as high as 50-60 per cent of total income and are *de facto* indispensable for maintaining a "normal" standard of living, the natural tendency on the part of management to obtain bonuses (almost regardless of consequences) substantially diminishes the actual margin of tolerance. Under other circumstances, with different incentive intensities, the margin of tolerance would be substantially greater.

4. The existence of the "insensitivity margin." The magnitude of the margin of tolerance depends also on several factors which we can denominate as the "insensitivity margin." (a) First of all there is the relative share of a given input in total costs. If the share of a given input in total costs is in the range of one or a few per cent, the "sensitivity" of the enterprise (or the branch of industry) to its price change is, as a rule, small. If a certain input comprises, say, 1 per cent of total costs, even a 20 per cent increase in its price will lead to only a .2 per cent increase in total cost. It is very probable that a total cost increase of this magnitude will not cause any realignments in enterprise demand, and so the given price increase lies within the margin of tolerance. If, on the other hand, the very same input comprises 20 per cent of total costs, the same price increase can initiate the process of substitution. We can conclude, therefore, that the margin of tolerance for any commodity is differentiated according to individual enterprises,

buyers or sellers. That is, the *effective* margin of tolerance is a magnitude of price change which does not cause deviation from planned performance in *any* enterprise. It is determined by the smallest margin of tolerance, i.e., the margin of tolerance in the branch or enterprise with the narrowest insensitivity margin. This obviously complicates the task of constructing a system of uniform prices for buyers and sellers, but nevertheless it enables the CPB to construct a system of double prices (P_b and P_s) instead of individual accounting prices for every enterprise.

(b) The further element which accounts for the existence of the insensitivity margin and, via this margin, influences the magnitude of the margin of tolerance, is routine. Because of routine, price changes within certain limits do not have any effect on the demand or supply schedules of enterprises. This unwillingness to change has its subjective aspect—any change means, as a rule, effort and trouble for enterprise management; and its objective aspect—usually every change costs money (the cost of changing technology, input norms, machine operations, etc.).

5. Lack of substitutes. Another factor which affects the existence and magnitude of the margin of tolerance is the substitutability of the different means of production. The more specialized is a given means of production, the less sensitive the enterprise is to a change in its price and the greater the margin of tolerance. However, the ease of substituting input A varies with different enterprises. Input A may be easy to substitute in producing X, and very difficult in producing Z. Here again we meet the effective margin of tolerance described above.

Thus far we have been discussing substitutability *sensu stricto,* i.e., as determined by technical causes, and have come to the conclusion that the smaller the substitutability the greater the margin of tolerance and vice versa (of course under *ceteris paribus* assumptions). Substitutability can also be limited (the margin of tolerance increased) administratively, by rationing. The rationing of means of production, which means that the enterprise cannot buy, say, bricks and lumber in other proportions than those prescribed by the plan, or cannot buy more of any of these inputs than the planned allotment, has the same effect as technical insubstitutability and diminishes the enterprise's sensitivity to price changes, i.e., increases the margin of tolerance.

The possibility of substitution acts counter to the margin of tolerance, because it creates the objective basis for deviation from quantitative and qualitative planned tasks. The motive, however, for taking advantage of substitution possibilities is financial gain (in the form of profit bonuses) which can be achieved as the result of unauthorized changes in plan fulfillment. If the possibility of substitution is limited by technological and institutional (rationing) factors, the profitability of substitution, within these limits, depends on the price ratios. The margin of tolerance around the price of input A depends then, among other things, on the profitability of its substitution by or for input B, i.e., on the price ratio of substitutes. Hence the margin of tolerance around a given price depends on the price system.

The price system, however, does not determine the magnitude of the margin of tolerance, but only its distribution around a given price. Let us assume, for example, that the price ratio of lignite and coal is 1:2 and that, with their prices fixed at 10:20, the margin of tolerance is identical for both prices, symmetric and at the 10 per cent level; that is, in this case the margin of tolerance = price ± 10 per cent. The change in the price ratio to 9:22 will make the margin of tolerance asymmetric for both prices, and moreover, the margin of tolerance will exist now only in the case of a price increase for lignite (+20 per cent) and for a price decrease for coal (—20 per cent). In constructing the system of operational prices we have to take this into account.

The existence of the margin of tolerance creates the objective basis for fixing *one* price for both buyers and sellers, in spite of the external character of methods of production, without the danger of active, negative influence of prices on plan fulfillment. The possibility of fixing one price exists when the effective margins of tolerance around P_b and P_s are overlapping.

The effective margin of tolerance can be extended or contracted as a result of the CPB's economic policy. One reason why the CPB may be interested in extending the margin of tolerance is the desire to have one price for both buyers and sellers in the greatest possible number of cases. In changing the margin of tolerance the CPB can rely on: (1) the varying intensity of administrative orders, (2) differentiating the share of bonuses (based on profit) in absolute terms and in relation to basic salaries, and (3) rationing policy. With (a) strong administrative orders and (b) limited bonuses, it is possible, in a great number of cases, to fix one price both for buyers and sellers.

In the practice of socialist countries both of these conditions, (a) and (b), are generally fulfilled. In Poland, for example, the yearly profit bonus cannot exceed one month's wage or salary. This policy of limited bonuses is explained in terms of needs for market equilibrium for consumers' goods. This policy of limited bonuses can be defended also by a different set of arguments along the lines mentioned above. The policy of limited bonuses is in accord with the character of our economy based on direct economic calculation. It constitutes a valuable incentive for improving enterprise performance and yet is not strong enough to endanger the discipline necessary for strict plan fulfillment.

At the same time we have to realize that action by the CPB to increase the margin of tolerance has also its negative aspect because, in its ultimate effects, it means diminishing the force of incentives: directly diminishing, when we consciously limit the possible absolute and relative magnitude of financial gains which can be obtained by management; indirectly diminishing, when we rely on administrative orders and rationing of the means of production. Diminishing the force of material incentives, at a given level of social integration, means the actual deterioration of average performance.

The margin of tolerance is a feature of the price system not unique to socialist economies. It also exists in capitalist economies. However, as far as we know, it is usually not taken into account in texts on price theory. The existence of the margin of tolerance in capitalist economies is not surprising, because most of its sources are not specific characteristics of socialist economies, and with few exceptions (administrative orders and rationing) are present in market economies as well. We are also inclined to think—it is, however, only a guess which still needs substantial research for its verification—that the margin of tolerance is present especially in big enterprises. In big enterprises the sources of the margin of tolerance (specifically the insensitivity margin) are especially strong. (In a big enterprise the cost and time involved in adapting to price changes are particularly great and hence the profitability of reacting to every price change, however small, is apt to be less than in smaller business units.)

At this stage of our analysis we can formulate several conclusions:

1. There are many "equilibrium" price systems for a given plan.
2. Because of the margin of tolerance the CPB can in many cases fix one price for both sellers and buyers, in spite of the external character of the methods of production under direct economic calculation.
3. The existence of the margin of tolerance facilitates the stabilization of operational prices without negative effects on plan fulfillment. As long as changes in economic conditions (and thus in planned tasks) call for a change in prices within their margins of tolerance, keeping prices unchanged by definition does not cause any reaction by enterprises and does not weaken incentives to any substantial degree. Keeping prices stable, therefore, has no adverse effects on plan fulfillment.

When we discussed the price system required for an incentive system based on gross output, we felt a little like Alice in Wonderland. It was the economic world as we know it *à rebours:* expensive products were priced the same as cheap goods, price increases had the effect of increasing effective demand, etc. Within the framework of profit incentives such an extraordinary price system is not necessary for effective plan fulfillment. Because the goal of the enterprise is now profit rather than gross output, it is possible to use prices based on the notion of production costs and to have a more or less traditional price structure. Scarce goods, which have to be economized, can again command high prices and vice versa. Of course, we are only saying here that now prices can be related to production costs, not that there is any rigid formula by which they should be determined. If profit bonuses are to be used as an effective incentive for fulfilling planned tasks, the prices must have an empirical character, to be fixed and changed according to varying economic conditions and plan objectives.

This empirical approach to prices is in direct contradiction to the general and deeply rooted tendency of "price fetishism." The experience of capitalist economies has accustomed us to think that prices must be based on value, price of production, or costs. This is the root of the belief that prices have to be "proper," that is to say, they must have an objective basis in costs, however calculated. It would be in direct contradition to this belief to fix a high price for low-cost goods or vice versa. The empirical approach is limited by fetishism.

III. *Value-Added Incentives and Prices*

The incentive system based on gross output stimulates enterprises to use expensive instead of cheap methods of production and to produce material-intensive rather than labor-intensive goods. The change from the incentive system based on gross output to one based on value added eliminates, to a great extent, the negative influence of "normal" prices (i.e. prices based on average cost of production plus 5 per cent profit) upon plan fulfillment. At the same time, however, it creates certain new problems dangerous for the effective functioning of the national economy.

By value added is meant here the difference between gross output and the value of all material means of production, interest, rent, and taxes. This definition of value added is used for the purpose of economic policy and differs from the definition of value added *sensu stricto* as used in Marxian economic theory. In the latter sense the value added is equal to $v + s$ (variable capital plus surplus value) and obviously includes wages, interest, rent, taxes and similar forms of redistribution of value added.

Within the framework of the incentive system, based on value added as defined above, the enterprises will use means of production according to their price ratios fixed by the CPB. Because the values of these inputs do not directly influence the value added, there is no reason for the enterprises to prefer expensive inputs in choosing production methods. There is also no reason for them to try to overfulfill the plan of gross output by eliminating the cheap goods from their assortment plan.

How will enterprises behave under the incentive system based on value added?

A. *Methods of Production*

With given prices the enterprises will try to minimize the "transferred value" (i.e., value of constant capital) and other items which are subtracted from gross output, because this, *ex definitione,* will help to maximize their value added. The CPB can stimulate enterprises to economize any given input by a relative rise of its price or vice versa.

Whether the enterprises will economize the capital stock depends on the type of cost calculation used. Under the cost calculation which does not include interest charges and rent, there will be a tendency on the part of enterprises to have as large a capital stock as possible. The latter does not cost them much (only depreciation charges which, thus far, are very small in most socialist countries) and it facilitates the maximization of value added. This tendency, however, can be avoided by charging a proper interest rate and rent, and by a sound depreciation policy. Under this type of cost calculation it will be profitable for enterprises, trying to maximize value added, to use the capital stock and natural resources only to a certain level. The CPB can raise or lower this level by appropriate changes in interest and rent charges.

Using an incentive system based on value added, we can expect the initiative for economizing the material means of production to come from within the enterprise. Whether this initiative will also lead to an attempt to economize capital stock and natural resources will depend on the type of cost calculation used.

This rosy picture of incentives based on value added also, however, has its negative side: the impact on labor force utilization and thus on the utilization of material means of production. Within the framework of incentives based on value added the labor force has a zero or even negative price from the point of view of its influence on the level of value added. Let us assume that the production plan of the enterprise is 10 units of X at 10 zlotys per unit in operational prices, and that planned costs are:

Materials and depreciation	80
Wages and salaries (value added)	20
Total	100

with 10 per cent bonus for achieving the planned volume of value added.

If the enterprise achieves the production target, using a labor input 10 per cent higher than planned (e.g., due to overtime work) the level of value added will not be influenced. Bonuses will therefore not be diminished. In this situation, from the point of view of the enterprise, the marginal cost of labor is zero.

If the enterprise achieves the production target, using a labor input 10 per cent higher than planned (e.g., due to overtime work), but at the same time cutting nonlabor inputs by 5 units, it will overfulfill the value-added plan. In this case, the labor force has a negative price from the point of view of bonus maximization. The same will hold, only not so strongly, if the use of more labor input than planned is due to additional employment (and not overtime) since then the bonuses will have to be shared by a greater number of employees. This will also be true, even more strongly, under a system of progressive bonuses based on value added.

The result is that within the framework of incentives based on value added there will be a tendency on the part of enterprises to substitute labor for material means of production. There will also be a lack of financial incentives to economize labor.

Two serious dangers are connected with this: (1) the danger of spending more for the national wage fund than planned, resulting in a disequilibrium in the consumers' goods market, leading to inflationary pressure; and (2) the danger that deviations from planned methods of production will disturb the planned real-balance equilibrium, i.e., relationships envisaged in the national input-output table.

Can these dangers be prevented by an appropriate price policy? Obviously not. In the situation where the price of labor from the enterprise's point of view is zero or negative, there is no possible change in the price ratios of material means of production to labor which can remedy the two dangers mentioned above. The remedy lies in administrative means. Price policy per se is ineffective. But used in conjunction with administrative means it can again become an effective tool in stimulating producers to fulfill planned tasks. To achieve this, we have to eliminate the zero or negative price of the labor at the enterprise level. It can be done by strict control and rationing of the wage fund.

Under strict rationing of the wage fund, when the enterprise cannot increase its value added by increasing labor inputs, labor ceases to have a zero or negative price in enterprise calculation. When this is the case, price policy for material means of production is again an effective tool for stimulating producers to achieve the planned methods of production and product-mix. Enterprises treat labor inputs as given (a function of the planned wage fund) and react to changes in price ratios in the same way as under profit incentives, i.e., they try to equate the marginal value productivity of each factor with its price. The CPB can again use the prices of factors of production as a part of an incentive system.

We have to realize, however, that value-added incentives create a certain conflict of interest between enterprises and the Central Planning Board. At the stage of plan building, when the wage fund is not yet fixed, labor, from the enterprise-calculation point of view, has a zero or negative price. As a result, enterprises will try to get as high a wage fund as possible in order to create hidden reserves. These can be used to overfulfill the planned value added by cutting material costs. This pressure from enterprises, due to the incentive system used, will, undoubtedly, be reflected in methods of production approved by the planners. Then, at the stage of plan fulfillment, the actual performance will tend to deviate from planned tasks because enterprises will have tried to maximize the value added by substituting labor for material means of production within the limits of the approved wage fund. The consequences of this conflict of interests cannot be resolved satisfactorily within the framework of an incentive system based on value added.

B. *What to Produce*

Let us now discuss the consequences of value added incentives on what will be produced. The magnitude of value added in 100 zlotys of gross output differs in various branches of production (e.g., it is relatively great in mining and relatively small in the electric power industry) and—what is especially important—it differs also for various products within a given branch of industry or enterprise. For example, the relative share of value added in gross output is greater in the case of handpainted porcelain than plain or regular porcelain. This fact creates certain problems for economic policy:

1. The difference in the share of value added in 100 zlotys of gross output in various branches of industry makes it necessary to apply bonuses differentiated by branches or, in the case of equal bonuses, the use of a properly differentiated turnover tax. These are necessary if we want to avoid unjustified differences in the level of bonuses received by different branches of industry.
2. The different share of value added in various goods makes necessary the use of a turnover tax differentiated by products. Otherwise there will be a tendency to deviate from the planned product-mix.

Hence the properly differentiated turnover tax makes possible the use of price policy as an effective tool for stimulating enterprises to produce in accordance with the tasks prescribed for them in the national plan.

Incentives based on value added do not invalidate the argument put forward in the previous section that, with direct economic calculation, there is a need for a system of two operational prices—one for buyers and one for sellers—due to the external character of the methods of production. Since our discussion of the margin of tolerance existing around every operational price also applies to the incentive system based on value added, we do not have to repeat our previous arguments here.

IV. *Conclusions*

1. The operational price and incentive systems always have to be analyzed together and not in isolation. The latter, however, is usually practiced by the planning authorities. For every incentive system there is an appropriate price system, and changes in one will always have to be accompanied by compensating changes in the other. Otherwise, there will be a contradiction between prices and incentives used, a contradiction from the point of view of their influence on plan fulfillment.
2. With direct economic calculation there is no optimal price or incentive system per se. We can find, however, an optimal combination: that combination which best serves plan fulfillment and which precludes price or incentive stimuli not in harmony with planned tasks.
3. The "price consequences" of any incentive system or of contemplated changes in it (i.e. changes in rules and/or intensity of incentives) always have to be carefully analyzed to avoid their possible negative effect on plan fulfillment. And vice versa, any price changes have to be analyzed from the point of view of their impact on the functioning of the given incentive system.
4. The choice of any given incentive system determines the system of operational prices which can be effectively used in conjunction with it and vice versa. Thus, as we have shown in Section I, the effects of average-cost-plus pricing operate in contradiction to the stimuli of gross output incentives. This mutual interdependence of the incentive and operational price systems limits the CPB's freedom of action in making both price and incentive policy.

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COMMUNICATIONS

Quantitative Research in American Economic History

Publication of the revised and expanded *Historical Statistics*[1] in 1960 was a notable achievement in bringing together the available quantitative information on the historical aspects of the U.S. economy. A comparison of the new volume with the first *Historical Statistics*,[2] published in 1949, amply testifies to the impressive accomplishment of the Bureau of the Census, the Committee on Historical Statistics of the Social Science Research Council, and the various consultants in this specialized field who were responsible for the new volume.

The introduction to the 1960 volume stresses that the original *Historical Statistics* was intended as a "working document" to pave the way for the "more comprehensive and definitive volume." In good part the data for the earlier volume were the result of research efforts over time by a variety of government agencies and by the National Bureau of Economic Research. The contribution of professional economic historians—with some notable exceptions such as the Committee on Price History—was not impressive.

The new *Historical Statistics* reflects a fundamental change in this respect. While the consultants responsible for individual sections were predominantly of governmental or National Bureau origin, there was more than a scattering of professional economic historians. A number of the most important series added to the new volume stems from the 1957 Conference on Income and Wealth held jointly with the Economic History Association.[3] Some of the papers at this conference were devoted primarily to a criticism of existing statistics. Others, such as the commodity output figures from 1839-99, the regional income estimates from 1840-1950, and the balance-of-payments figures from 1790-1900, were net additions to our knowledge.

This conference heralded a growing interest in quantitative research in U.S. economic history. It has been followed by annual conferences at Purdue University which have been working sessions devoted to research in progress. The growing attendance at these sessions has reflected the increasing volume of quantitative research. In 1963 still another Conference on Income and Wealth in the nineteenth century will be held.

A revolution is taking place in economic history in the United States. It is being initiated by a new generation of economic historians who are both skeptical of traditional interpretations of U.S. economic history and convinced that a new economic history must be firmly grounded in sound statistical data.

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[1] U.S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1957*. Washington 1960.

[2] U.S. Bureau of the Census, *Historical Statistics of the United States, 1789-1945*. Washington 1949.

[3] *Trends in the American Economy in the Nineteenth Century*, Nat. Bur. Econ. Research Stud. in Income and Wealth, Vol. 24. Princeton 1960.

Even a cursory examination of accepted "truths" of U.S. economic history suggests that many of them are inconsistent with elementary economic analysis and have never been subjected to—and would not survive—testing with statistical data. A number of major reappraisals have already been undertaken. The unprofitability of slavery in the antebellum South,[4] the indispensable role of the railroads in nineteenth-century American development,[5] and the importance of the Civil War in accelerating U.S. industrialization,[6] are three illustrations of this critical scrutiny, and they are just the beginning of the reassessment of our economic past.

If we are learning what U.S. economic history is not, it is a more difficult task to reach positive conclusions. A reconstruction requires far more comprehensive knowledge of our past than we currently possess. This knowledge, whenever possible, must be made available in the form of the quantities that have constituted the external manifestation of economic change in the United States. If the new *Historical Statistics* gives evidence of our relative richness with respect to quantitative data, it is still true that we are far from well endowed with the quantitative information necessary to outline the contours and patterns of American economic change from the past. We have only scratched the surface. Even a cursory examination of the wealth of material available in the form of early price data, official documents of many kinds, local historical materials, and state statistics, indicates that possibilities for a much firmer reconstruction of the quantitative estimates of our past are available to us. While any single time series may be subject to a wide range of error, the gradual accretion of independent series lends greater confidence in our ability to arrive at careful judgments about the characteristics of our economic development in previous years.

A more difficult problem faces us in the development of the theoretical hypotheses necessary for shaping the direction of quantitative research. A decade or more of concentrated research on economic growth has yielded meager results for the quantitative economic historian. The tools of the economist provide initial hypotheses to explore a wide range of questions posed by the economic historian, but for those concerned with the great question of the economic rise and fall of nations the fare is still thin. Perhaps the major result so far has been to disabuse the economic historian of any simple ready-made explanation of U.S. economic growth, and to encourage more intensive investigation of the varying sources of changing productivity in the economy. In this area the tentative exploratory hypotheses of economists offer useful starting points for further work. Recent research on technical change,[7] investment in human capital,[8] and economies of scale,[9] all offer the economic historian starting-points from which to develop further hypotheses and undertake quantitative research. On a somewhat breathtaking scale, the study of E. F. Dennison[10] is an imaginative effort to provide a more comprehensive analysis of the contribution of different sources of increasing efficiency to the growth of the U.S. economy.

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[4] A. H. Conrad and John R. Meyer, "The Economics of Slavery in the Ante-Bellum South," *Jour. Pol. Econ.*, April 1958, *66*, 95-130.

[5] Robert W. Fogel, "A Quantitative Approach to the Study of Railroads in American Economic Growth, a Report of Some Preliminary Findings," *Jour. Econ. Hist.*, June 1962, *22* (2), 163-97.

[6] Robert E. Gallman, "Commodity Output 1839-1899," *Trends in the American Economy in the Nineteenth Century*, pp. 13-72; Thomas Cochran, "Did the Civil War Retard Industrialization," *Mississippi Valley Historical Review*, 1961, *48*, 197-210; Douglass C. North, *The Economic Growth of the United States, 1790-1860*, Englewood Cliffs 1961.

Economic history has been, and will continue to be, a field in which historians and economists combine their efforts to contribute to our knowledge of the economic past. Moreover, this new emphasis in no way vitiates the contributions of the nonquantitative historian. It does promise to provide both the historian and economist with important new support. For the political and social historian, the basic revisions of our economic history should suggest some fundamental reinterpretations in political and social history.

The economist can expect that the quantitative research and analysis of the economic historian will provide him both with improved data to test hypotheses and with a cumulating number of analytical insights into the process of past economic change which are the essential prerequisite to evolving sound theoretical propositions about long-run economic development.

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[7] A suggestive study for the economic historian is W. E. G. Salter, *Productivity and Technical Change*, Cambridge 1960.

[8] Investment in Human Beings," *Jour. Pol. Econ.* Suppl., Oct. 1962, *70* (5), Pt. 2.

[9] George Stigler, "Economic Problems in Measuring Changes in Productivity," *Output, Input, and Productivity Measurement*, Stud. in Income and Wealth, Vol. 25, Princeton 1961, pp. 47-78.

[10] *The Sources of Economic Growth in the United States and the Alternatives Before Us*, Suppl. Paper No. 13, Committee for Economic Development, New York 1962.

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Trade Restraints in the Western Community

A recent report of the U.S. Congress Subcommittee on Foreign Economic Policy, entitled *Trade Restraints in the Western Community* [7] makes an assessment of obstructions to trade and especially of tariff levels. This document has been used by the Congress, has attracted wide attention in the world, and may have a great deal of influence on policy. Yet it is grossly misleading in several respects.

I. *Quantitative Restrictions*

The report after assessing, in a fashion, the height of tariffs on various groups of commodities, says blandly that the United States belongs to a few countries with the lowest tariff levels in the world on agricultural products. It is true that the report does mention that in the case of agricultural goods tariffs are not the most important instrument of protection, because many countries keep fairly low tariffs on such products but grant a very high degree of protection to domestic producers by subsidies and quota restrictions, or import licensing (e.g. the United States, France, and Germany). This qualification is, however, put only casually, *en passant,* and worse, Section B of the report dealing specifically with quantitative restrictions eschews any discussion of such restrictions on agricultural products. Why? Tucked away in the introduction instead of in Section B where it might carry some weight, is the weak excuse that the complexity of the restrictions makes it impracticable to discuss them. The reader could then be excused if he concluded that the U.S. policy in the field of agricultural imports is quite liberal. In this context it would be quite revealing to mention that such an important butter exporter as Australia enjoys an annual quota of butter exports to the United States amounting to 45 tons!

II. *Measurement of Tariff Levels*

The authors of the report undertook to calculate what cannot be calculated, that is, the height of tariffs in various countries. They took an unstated number of goods, ". . . thought to represent no less than 75 per cent of the international trade of any country among those for whom comparisons are made . . ." (p. 7), grouped them into categories and weighted the particular tariff on each item by the imports of that item in the country examined! There are various factors that could have been used as weights here, e.g., total U.S. exports, or total world exports, or total consumption of each commodity in the country examined [1] [2] [3] [6]. All of these would have serious weaknesses, but the weighting system adopted in the report is the worst possible one.

It should be obvious that under this system, where a particular tariff is very high the volume of imports of the article will be very low and therefore the very high tariff rate will be given a very low weight in the total weighting system, which will thus strongly tend to hide the high tariff rate. Worse than that, where the tariff rate is prohibitive, i.e., there will be no imports of the commodity, the weight used will be zero, which gives an absurd result—a very high tariff is equivalent to free trade!

There is no indication in the report that its authors were aware of the pitfalls. They argue: "These average tariff figures are reasonably representative of the relative level of restrictions placed on imports by various nations . . ." (p. 6). Perhaps, however, they thought that some measure of the obstacles to trade is better than no measure at all. If so, they were singularly wrong. There are many situations where inaccurate information is worse than none. Indeed, normally this is so; and where numbers are involved their inherent prestige makes the position even more dangerous.

The report gives what purports to be average weighted tariff rates for a number of countries. In respect to industrial goods it gives, e.g., for the United States an average rate of 11 per cent and for Australia 12 per cent, which would place the two countries in the medium tariff group. It is interesting to give here the findings of the League of Nations 1927 *Report* [6]. That report took 110 articles (". . . being typical exports from 14 countries . . .") and gave the weighted average tariff rates. The United States emerged as the most protectionist country in the world (except for Spain) with an average rate of 35-40 per cent. Australia also ranked very high in that assessment with the fifth place in the world and an average rate of 25-30 per cent. Have those two countries so drastically reduced their tariff rates in absolute terms and also relatively to others, or are the weighted average rates quite wrong?

To demonstrate the errors, any country could be taken. Let us take Australia. The Congressional report states, [7, p. 8] that, e.g., the average Australian tariff on motor road vehicles, railroad vehicles, and other transport equipment is 7 per cent (!) at the GATT, i.e., the MFN rate. Table 1 gives, however, the actual tariff rates for some items in the vehicle group, as given in an official Australian source [5].

TABLE 1—SELECTED TARIFF RATES, VEHICLES, AUSTRALIA[a]

Tariff item	Article	MFN tariff rate per cent
360(D) (2)	Complete motor cars	35
360(A) (1)	Complete trucks	27½
360(A) (2)	Complete trucks of different specification	12
360(c)	Railway vehicles	47½
359(1)	Springs for trolley buses	57½
359(2)	Shock absorbers	57½
359(3)	Air-brake equipment	52½
359(G) (2)	Certain parts for axle assemblies	7½
359(G) (3)	Certain parts for axle assemblies	62½
359(1)	Parts for railway vehicles: Bodies	47½
359(2)	Parts for railway vehicles: Chassis	37½

[a] Certain vehicle parts, subject to the discretion of the Department of Customs and provided that they are not available from the United Kingdom, are duty free, e.g., tariff item 359(D) (2). *Source:* [5].

It should be clear from the table that the "average weighted tariff rate" can only mislead. It is not only that the average weighted tariff will always underrate the degree of protection, but in addition the degree of distortion will be unequal for various groups of goods, and unequal for various countries. That degree of distortion cannot in the very essence of the problem be assessed or calculated in any way. All we can say is that countries with highly protectionist policies, which impose very high tariffs on such a group of goods as, e.g., finished goods, and very low tariffs on a few of the materials and components (which they temporarily do not intend to make), will have the degree of their protectionism grossly underrated compared to countries which have a fairly low but uniform tariff collected on almost all goods. And yet, the latter type of tariff system has essentially the nature of a revenue tariff, has almost no protective effect (as this term is normally understood), and in fact, since such a uniform tariff increases the costs of all imports uniformly, it works in this respect in almost exactly the same fashion as devaluation would (though, of course, it has no favorable effects on exports).

III. *General*

Even if the height of tariffs could be measured, there would be no particular advantage in having the measurement. There is a large number of other very important protectionist measures to which no such yardstick could be applied, such as, e.g., quantitative restrictions, the Buy-American Act, or even the buy-local-goods propaganda, and numerous administrative chicaneries. Finally, various countries have a domestic sales tax (or its equivalent) collected at various rates and levels in the process of production or distribution. Many countries collect their sales tax on the "landed cost" of the commodity, which includes, *inter alia,* the customs tariff. The portion of the sales tax which is based on the c.i.f. foreign price of the commodity, plus, perhaps, the portion which is based on the imputed profit of the importer, have of course a counterpart in the sales tax on the domestic good, and therefore there is no element of discrimination; but the portion of sales tax which is collected on the customs tariff itself has no domestic equivalent, and may properly be considered as a roundabout method of increasing the customs tariff. Thus the true level of protection on complete motor cars in, e.g., Australia is at present at the MFN rate 35 per cent plus 22½ per cent of 35 per cent (the current rate of sales tax is 22½ per cent), which brings the total to 42.9 per cent.

Above all, if we could do the impossible, and put a figure on all of the protectionist measures taken together and expressed in terms of a tariff, we would still be ignorant of the true degree of protection that any particular industry enjoys in various countries. It should be clear that, e.g., protection equivalent to a 10 per cent tariff granted to the U.S. car industry would give a powerful protectionist effect to that industry; and yet the same 10 per cent granted by, e.g., Spain, Japan or India would give virtually no protective effect to their car industries. In the case of each particular good and country there will be different degrees of elasticity of demand for the imported good and different domestic elasticities of supply of it. Where the domestic elasticity of supply is low, any particular level of tariff will give less protective effect than when the elasticity of supply is higher. Where the elasticity of demand for the imported good is fairly high a given moderate tariff may produce quite a considerable *obstructive* effect against exports to that country. The elasticity of demand for a particular import will depend in each country on the particular tastes of the population, general conditions, and availability of close substitutes. The elasticity of the domestic supply of the imported good will depend on the size of the country and the variety of its resources. Where the country itself is small and its resources not varied, the obstructive effect of a given tariff may be quite small. If, however, the country belongs to a preferential tariff block, such as the British preferential system, or the European Economic Community, then the smallness of the country would not matter in respect to the effect of preferential tariff margins. The preferential tariff area would have such a vast variety of resources and skills that quite often a minute preference margin—a small tariff against the outsider would produce a very great obstructive effect and distortion of international trade.

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The Geometry of Capital and Interest: a Suggested Simplification

In recent years, the study of capital and interest has come to be viewed as a branch of monetary theory. No doubt interest is, in some degree, a monetary phenomenon. (If it is not, central bank policy is an awful hoax.) Unfortunately, young economists who first make the acquaintance of capital and interest through their work in monetary theory often seem to believe that interest is mainly a monetary phenomenon. They are, of course, badly informed.

Since Böhm-Bawerk's day the well-informed economist has known that interest is rooted mainly in the productivity of investment and the economy's preference for present over future consumption. The effective exposition of this truth, however, has never been easy, and for the last thirty years pedagogical progress has been distressingly slow. Monetary theorists now have a large bag of elegant tricks that demonstrate how changes in the money supply and liquidity preference can affect the interest rate. Advocates of the so-called "real" theory of interest must fall back on a set of teaching aids with a distinctly musty odor—Robinson Crusoe and his fish-catching activities, growing trees, aging wine, the more roundabout method of production, etc. Therefore, there may be merit in describing a pedagogical device that allows the essential properties of capital and interest to be simply, albeit rigorously, conveyed.

Two of the more effective tools available are Irving Fisher's indifference curves [2, pp. 387-90] and Frank Knight's Crusonia plant—a species of vegetation that supplies all human wants and, though unattended, grows at a constant (geometric) rate "except as new tissue is cut away for consumption" [3, p. 30]. In essence, the pedagogical device described here applies, with modifications, Fisher's geometry to Knight's Crusonia plant.[1]

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[1] The Crusonia plant has the great virtue of eliminating three complications in capital theory that are fundamentally unimportant but are, nonetheless, time-consuming in the classroom and a source of confusion to the young. It enables us to ignore the "period of production"—the gestation period of capital assets. (For the view that the period of production is an important idea, see Robert Dorfman's rigorous restatement of Böhm-Bawerk's theory [1].) This period is perforce zero since the Crusonia plant grows con-

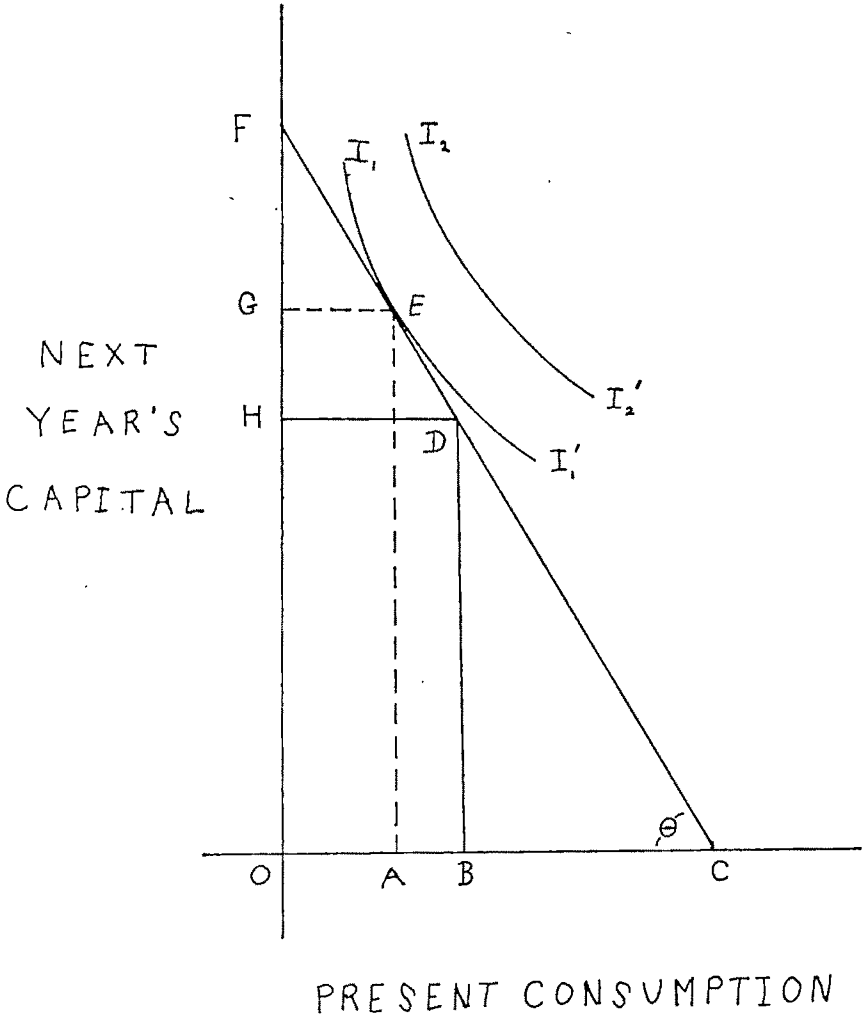

FIGURE 1

Consider Figure 1. Measure present consumption on the horizontal axis *OC*. We posit that all consumption for the coming year is done "now." This assumption allows us to treat consumption as having the dimensions of a stock rather than of a flow and, hence, to represent consumption and the stock of capital on the same axis. In Figure 1, *OC* denotes the maximum amount of consumption that is possible now. *OC* also denotes the present stock of capital

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tinuously. It allows us to avoid all measurement problems. The stock of capital, consumption, and investment per unit of time, can be expressed as quantities of Crusonia. And finally, the Crusonia plant permits an exposition of capital theory in the first instance without any attention to the mechanics of capitalization and discount.

(Crusonia) on the assumption that no consumption for this year has yet occurred, and that the whole of the present capital stock can, if it is desired, be consumed this year.

Now measure the stock of capital that will be in existence (and available for consumption) one year from now on the vertical axis OF. If no consumption takes place now, the stock of capital will reach OF by the end of the year. Clearly the size of this future capital stock is a function of (a) the marginal productivity of investment, i.e., of not consuming, and (b) the amount consumed now. Every possible combination of present consumption and next year's capital stock is represented by a point on the line CF.

On OF mark off the distance OH equal to OC. The marginal productivity of investment (*mpi*) is given by the slope of the line CF (minus one). Since this slope is a constant, there are no diminishing returns to investment. The rate of growth of the capital stock is independent of its size, CF is perforce a straight line, and *mpi* equals $\tan \theta - 1$. We perceive from Figure 1 that OB is the amount of consumption compatible with a capital stock that reaches a maximum of OC (or OH) each year. Immediately after consumption OB, the present capital stock is BC. During the year it grows back to OC (or OH). Should consumption exceed OB, the capital stock will be smaller one year from now. Should some part of OB be invested, it will be larger.

In order to reckon investment—and hence capital growth—we must specify the economy's preference for present consumption over future capital. This we do in Figure 1 by a set of Fisher-type indifference curves, I_1I_1', I_2I_2'. Present consumption is again measured along the horizontal axis and the stock of capital one year from now along the vertical axis.[2]

From Figure 1 we see that the economy attains its highest indifference curve (I_1I_1') by consuming OA now and having a capital stock of OG in one year's time. We may also read off the following information. For a period of one year:

$OC = OH =$ initial capital stock.
$OG =$ final capital stock.
$OB =$ income available for consumption.
$OA =$ actual consumption.
$AB =$ income invested.
$HG =$ growth in capital stock ("net investment").
$\frac{HG}{OH} =$ capital stock growth rate.

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[2] In Fisher's original geometry the vertical axis measures future income and the horizontal axis present income [2, p. 387]. In that context, however, Fisher is not using "income" to denote a flow of goods (or services) in perpetuity. The value of the capital stock in his diagram can only be obtained by discounting the future income by the interest rate and adding the result to present income. Thus it is not immediately clear from Fisher's geometry whether a failure to consume all of present income (as defined by Fisher) serves to increase the capital stock or merely retard its decline. This uncertainty can be resolved by using the data provided by Fisher in his diagram; but the computation is tedious.

In Figure 1 the marginal productivity of investment during the year is given by any of the following:

$$\tan\theta - 1, \frac{OF}{OC} - 1, \frac{HG}{AB} - 1, \frac{HF}{OB} - 1, \frac{HF}{OH}\cdot$$

Figure 1 depicts the "normal" case of an economy that is increasing its capital stock. To derive a zero growth rate we need only revise our diagram to make the indifference curve, I_1I_1' tangent to the line CF at point D. To show capital consumption we would make the point of tangency lie between D and C.

The geometry of Figure 1 has an additional merit as a teaching aid. It can be used to show that there is no genuine conflict between writers who treat capital as a stock of "real" things that yield incomes in perpetuity and those who define it as a sum of future incomes discounted by the rate of interest. (Given perfect arbitrage, the rate of interest must, of course, equal the marginal productivity of investment.) For example, let the perpetual annual income be HF; let HF be paid in a lump sum once a year; and let the first payment be due one year from today. The present value of a perpetuity disbursed in this manner is equal to the annual income divided by the interest rate. The interest rate is $\frac{HF}{OH}\cdot$ Therefore, $\frac{HF}{\frac{HF}{OH}} = OH.$

But from Figure 1 it is immediately apparent that, when consumption takes place on the last day of the year rather than on the first (as we previously assumed), HF is the annual harvest or income of the capital stock OC or OH.

Our geometry is easily modified to accommodate the case where the marginal productivity of investment declines as the capital stock increases. This is done in Figure 2. Let us revert to the assumption that all consumption for the year takes place now. Once again OC denotes the present stock of capital, OB the quantity of present consumption compatible with a constant stock of capital OC (or OH), and the arc FC every possible combination of present consumption and future capital.

However, when diminishing returns to investment are introduced into our model, a complication appears. There is now a different *mpi* for every different capital stock. And, since in the absence of consumption, the capital stock grows continuously, there is a different *mpi* for every moment during the year. (Recall that *mpi* is what the instantaneous growth rate of the capital stock would be if consumption were zero.) Thus one is reduced to speaking of the average value of *mpi* for the year. The greater the fraction of income invested now, the smaller the average value of *mpi*.

Figure 2 indicates that the economy has achieved long-run equilibrium. It attains its highest indifference curve II' by consuming the whole of its annual income OB. Average *mpi* is equal to the slope of FC and II' at point D (minus one). Note that when diminishing returns to investment are posited, average

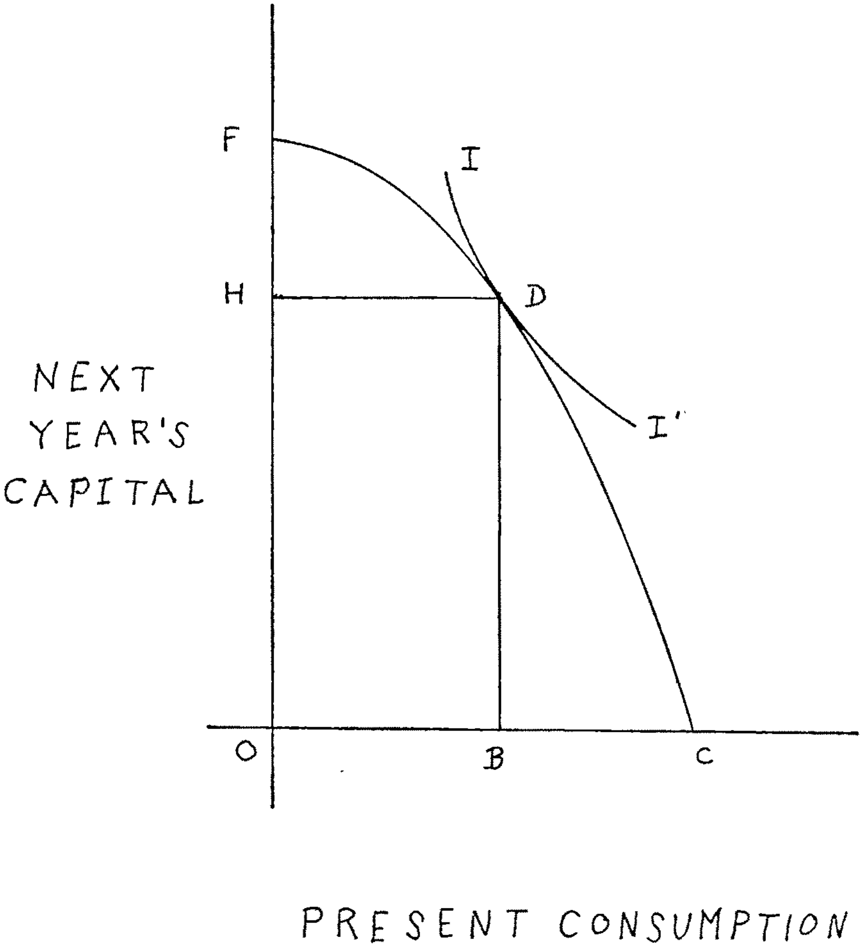

FIGURE 2

mpi is necessarily less than the average rate of return on investment during the year.

In Figure 2 the quantity of consumption invested now is *BC*, and the capital stock one year hence will be *OH* (or *BD*). The average rate of return on investment *BC* is therefore $OH/BC - 1$. This last rate has no special significance in capital theory, except that it is sometimes mistaken for the annual market rate of interest. But the interest rate is tied by arbitrage to average *mpi*—to the slope of *FC* at point *D* (minus one) in the present example. Indeed, the equality of average *mpi* and the interest rate is implied by the proposition that the economy has achieved its highest indifference curve.

What is true is that, as the long-run equilibrium of a stable capital stock is approached, the interest rate and average *mpi* tend toward the average rate of return on *income* invested. For now the quantity of income invested becomes an ever smaller percentage of total investment, i.e., the capital stock. Thus in Figure 2, if some minute portion of income *OB* were saved, the average rate of return on income invested would be virtually the same as average *mpi*. In effect, the small fraction of *OB* saved would be the incremental unit of investment.

The essential properties of capital, income, consumption, investment, capital growth, time preference, and capital productivity can be set forth in many ways. The method described above has, I believe, the merit of allowing the delineation to be made clearly, compactly, and with minimum use of cumbersome notation.

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The Criterion for an Adverse Balance of Payments

I should like to register a protest with regard to the criteria now being used by the Department of Commerce with respect to whether changes in the balance of payments are adverse.

The following table consists of Lines 30 through 48 extracted from the Second Quarter 1962 data for the Balance of Payments of the United States, as presented in the September 1962 *Survey of Current Business* (pp. 14-15; in millions of dollars):

30	U.S. capital, net [increase in U.S. assets (−)]	−1,101
31	Private, net	−686
32	Direct investments, net	−449
33	New issues of foreign securities	−319
34	Redemptions	21
35	Transactions in outstanding foreign securities	−17
36	Other long-term, net	−80
37	Short-term, net	158
38	Government, net	−415
39	Long-term capital	−511
40	Repayments	212
41	Foreign currency holdings and short-term claims, net [increase (−)]	−116
42	Foreign capital, net [increase in U.S. liabilities (+)]	609
43	Direct investments in the United States	49

44	Other long-term investments	13
45	Other capital excluding liquid funds	32
46	Increase in foreign holdings of liquid dollar assets	515
47	Gold and convertible currencies, purchases (−) or sales (+) by monetary authorities	−207
48	Reduction in gold and convertible currency holdings (line 47) and increase in U.S. liquid liabilities (line 46)	308

It is line 48 that the Department of Commerce customarily uses as the gauge of improvement or deterioration in the balance of payments. And this is the figure that has generally been cited by President Kennedy and United States monetary authorities during the last two or three years as evidence of the degree of adversity in the changes occurring in the United States payments position.

For example, the explanatory article in the September 1962 *Survey of Current Business* begins: "The overall balance in our foreign transactions, measured by changes in gold and convertible currency holdings of our monetary authorities and in liquid liabilities, in the second quarter of this year, was adverse by about $300 million." This is the $308 million of line 48. (Seasonal adjustments might convert this figure to $218 million.)

The New York Times of October 9, 1962, cites Undersecretary of the Treasury, Robert V. Roosa, as hoping for an improvement in the balance-of-payments picture by the end of 1963 despite this adverse showing. The *Times* article goes on to say: "The only official returns available for 1962 cover the first two quarters . . . For the April 1-June 30 quarter the deficit was about $218,000,000 . . ." This would seem to be the "'seasonally adjusted" figure mentioned above.

This criterion essentially lacks symmetry. Gold loss is netted with gold gain, but the same cannot be said in connection with the handling of short-term funds. Line 46, which is included in the total of line 48, includes the changes in foreign holdings of liquid dollar assets. But the more or less equivalent figures for changes in United States holdings of short-term assets abroad (lines 37 and 41) are omitted from the totals of line 48.

The principal argument for this procedure is, I presume, that the foreigner might really create difficulties for us by withdrawing his dollar balances on short notice. Presumably the United States wants to be able to meet such a drain without calling upon short-term balances held abroad by United States citizens.

It might be argued that this is the conservative approach to the matter. But is it really that? Of course, it should be kept in mind that foreigners may withdraw these balances; but to handle the increase in foreign deposits here as though it had adverse effects which are homogeneous in kind with net gold outflow is surely misrepresentation of the economic significance of dollar balances owned by foreigners.

Financial centers are naturally debtors at short term. The center that can attract balances of this sort does so because foreigners have confidence in it and in its financial reliability. Short-term balances are simply not kept in financially weak centers. The fact that such balances in the United States are high and rising is evidence of the financial strength of the United States, not of the financial weakness of the United States. The increase in these foreign short-term balances in the United States simply should not be added to the net loss of gold figure as evidence of the degree of worsening in the balance-of-payments position of the United States.

That short-term indebtedness is characteristic of financial centers and is evidence of their financial strength is indicated by centuries of international finance history. England was a debtor at short term consistently during the nineteenth century—and still is for that matter. The United States has generally been a net debtor at short term in decent decades. So far as the United States is concerned the shift from net creditor at short term to net debtor at short term is the chief evidence we have of the transition of the United States to the role of major international financial center.

In the second quarter of 1962 the increase in foreign holdings of liquid dollar assets ($515 million) overshadowed the alleged adverse balance of $308 million. Actually it might about as well be claimed that developments in the second quarter were favorable as that they were unfavorable. Gold was gained and foreigners expressed confidence in the dollar by increasing their short-term holdings in this country.

The Department of Commerce merely compounds its felony by leaving out of account the change in United States holdings of short-term balances abroad—balances which could be used to cushion the shock in the event foreigners should withdraw their dollar balances on short notice. It is true that in the second quarter these holdings fell by [$158 million (line 37) minus $116 million (line 41)] $42 million. But in general during recent years, while monetary authorities have been complaining about balance-of-payments-deficits, this figure has been rising at a substantial rate. In any event, whether it is rising or falling, conditions of symmetry would seem to suggest the desirability of taking it into account.

Incidentally a pure "gold holdings" figure might be better as a measure of what is happening to the balance of payments than the present lop-sided hybrid. But recent changes in the format of the balance of payments statement mean that we cannot get such a figure from the statement. Instead we get a figure in lines 47 and 48 which is adulterated by the presence of convertible currencies held by monetary authorities.

The hysteria that, a year ago, inconvenienced the families of a great number of service men and which resulted in the cutting of the tourist duty-free import allowance back from $500 to $100 might well have been avoided if the understanding of the meaning of an adverse balance of payments had been a little better.

Of course, in the next decade or two the United States may well have serious balance-of-payments troubles. Let us hope that these troubles, if and when they occur, are analyzed with the help of better definition of what is meant by an adverse balance of payments.

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Errata

In the article by Marvin Frankel, "The Production Function in Allocation and Growth: A Synthesis" (*American Economic Review,* December 1962, *52,* 995-1022), many (but not all) subscribers' copies contained a number of typographical errors which resulted from the breaking-off of parts of certain characters as the printing of the issue proceeded. Thus the dots over several variables, intended to mark them as time derivatives, were omitted from a number of expressions. The omissions occur in (4) (4a) (5b) (5f) (5g) (5h) (5i) (5k) (9a) (9b) (9c) (13) (14) (14a) (14b) (14c) (15). Except for (5b) their effect—and a means of identifying them—is to make the numerator and denominator of certain terms in the expressions identical. Thus (4) should read:

$$\frac{\dot{P}}{P} = .015 + .35\,\frac{\dot{K}}{K} + .65\,\frac{\dot{L}}{L}. \tag{4}$$

In (5b) the third term should be $\dot{K}$.

In the review by Gerhard Colm in the December issue, p. 1166, the word "eastern" in the third line should be "western."

BOOK REVIEWS

General Economics: Methodology

Money, Growth, and Methodology and Other Essays in Economics. In Honor of Johan Åkerman, March 31, 1961. Edited by HUGO HEGELAND. Lund, Sweden: CWK Gleerup, 1961. Pp. xii, 509.

It is always difficult to review a *Festschrift* which lacks a unified theme. The present one is emphatically no exception. Very appropriately, in view of the wide range of problems to which the great scholar who is here honored has made important contributions, the area covered is exceedingly large. Forty-eight distinguished contributors have written essays which are grouped by the editor under five headings: theory of growth; money, interest, and inflation; methodology, ideology, and economic concepts; trade, competition, and programming; economic policy and economic development. All a reviewer can do is to mention a few names and to refer specifically to a few gems in order to convey an idea of the riches which are in danger of being overlooked because they appear in such a volume and not in one of the economic journals.[1]

About half of the essays are in English; the rest are in French, Swedish, and German—the Swedish contributions being summarized in English (and two of them in German).

The first paper on growth by Gottfried Bombach (9 pages in German) offers some rather simple but sensible and critical observations on global forecasts of future growth. Erich Schneider's (5 pages) and Brinley Thomas' papers are interesting among other reasons because they refer to Åkerman's work. Brinley Thomas' contribution, "The Rhythm of Growth in the Atlantic Community" (9 pages), compares the three-cycle schema of Schumpeter, Åkerman's scheme and the so-called "Kuznets cycle" which was also identified in A. F. Burns' *Production Trends in the U.S. Since 1870.* I am inclined to agree with Thomas that the Kuznets cycle is more of a cyclical reality than Schumpeter's Kondratieff. Tinbergen's essay, "Development Theory—An Econometrist's View" (9 pages), sketches a broad program of econometric research encompassing the measuring by means of indexes of "environmental variables" such as "climate," "institutions," and "the state of technology."

In Part II, "Money, Interest and Inflation," let me briefly comment on three papers. Gösta Mickwitz in his essay, "Inflation as a Stimulus to Growth in a Three-quarter Industrialized Society" (in Swedish, 10 pages), argues that the chances that development can be speeded by inflation are better in semi-industrialized countries than in either a fully developed or wholly underdeveloped country. What he really proves, it seems to me, is the not so startling proposition that countries already on the road to development can grow faster than those farther ahead or farther behind—with or without inflation. It would be easy, however, to show that *potential* growth is higher if inflation is avoided. This does not mean that *actually* growth will be faster in the absence of inflation, because it is always possible to avoid an evil—inflation—by methods which make things still worse.

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[1] It would be a good idea to publish a *Festschrift* as a special number of a periodical rather than as a separate volume. A fractional offset for the lack of wide circulation may be found in the circumstance that the knowledge that what one writes will be entombed in a *Festschrift* may loosen the tongue of some contributors and induce them to write more provocatively than they would dare to write for a more accessible medium.

B. Ohlin discusses the ancient problem of the quantity theory of money (9 pages) versus an income theory *à la Keynes ou Suedoise.* He "indicates from the beginning that his attitude is a critical one towards the [quantity] theory as it is commonly presented by its orthodox adherents." The reason is that during the business cycle the quantity theory does not work well because the velocity of money changes. Towards the end of his paper the author relents in his opposition and admits that "the insistence of the quantity theorists that the functioning of the monetary mechanism plays an important role during the different phases of the business cycle seems to me to be eminently justified." This concession is made to the "modern" quantity theory, especially that of Milton Friedman and of his disciples. The reviewer would raise the question who the proponents of a mechanical, orthodox quantity theory are. He cannot think of any major economist between Hume and Irving Fisher (both included) who would qualify for that role.

Sven Rydenfelt in his essay "On the Theory of Wage Inflation" (in English, 11 pages) asserts that in Europe even labor governments such as the Swedish or British and their economic advisers accept the wage-push theory of inflation. The reviewer agrees with this statement and would support it by referring to the well-known report, "The Problem of Rising Prices" (OEEC, Paris 1961). This report not only sees in wage pressure exerted by trade unions the main cause of inflation (in recent years) but states flatly (p. 70) that there "cannot be a profit-spiral" corresponding to the "wage-spiral" (in Washington economic jargon that there is no such thing as "mark-up inflation"). The report was signed not only by W. Fellner and F. Lutz, but also by Bent Hansen and Richard Kahn.

The author of the essay under consideration is impressed by the criticism which the wage-push theory has received at the hands of American labor economists and liberal economists of the Chicago group. He compares the role of union representatives in wage bargains with that of rain-makers in primitive Africa. "They offer the audience a drama in the form of aggressive pronouncements, menaces of strike and prolonged nightly sessions. And when, at last, the wage increases come—they take the credit" (p. 134), the obvious implication being that the same wage increase would have come anyway. To the reviewer this seems to be a gross understatement of union power. The author speaks of "menace of strike." Maybe it remains a threat in Sweden, but in the United States and the United Kingdom the threat is often carried out, and in Latin America wage-jumps by 50 per cent or more across the board, enforced by powerful unions, are the order of the day. It is, of course, true that wage pressure cannot push up the price level unless the quantity of money is allowed to increase. But the crucial question is at what level of unemployment would the wage push cease if the monetary authorities stood firm. This question the author does not discuss.

In Part III, "Methodology, Ideology and Economic Concepts," I should like to draw the reader's attention to A. Gerschenkron's sparkling and very learned causerie on "Ideology as a Methodological and Historical Problem" (15 pages). He first takes up the old problem of the role of value judgments in science in general and the social sciences in particular. The spirited defense of the principle of *Wertfreiheit* is unfortunately nowadays more needed than ever, despite Cairnes, Max Weber, Pigou,[2] etc.

After having rejected the "imaginary" difference between natural and social sciences consisting of the alleged impossibility in the social sphere to abstract from the scientist's valuations, Gerschenkron takes up what he regards as specific difficulties of the social sciences, namely, the existence of "mutual influences which connect the social scientist and his object." He refers especially to the "possible effect of—published—prediction upon the predicted event," a difficulty which has often been referred to in connection with business cycle forecasting.

It seems to me that this difficulty is not confined to the social sciences. That medicine may encounter it seems clear. A member of the Vienna circle of positivists, Otto Hahn, once jokingly observed that zoologists, strictly speaking, do not know how ants behave but only how they behave when they are observed. He adds: "There is serious truth in this joke: Every event is somehow disturbed by the fact that it is observed. That this is not a case of irrelevant hairsplitting, but has basic importance, is clearly shown by the most recent development of [atomic and subatomic] physics."[3]

Part IV opens with a paper by Maurice Allais on "A Paradox in the Theory of International Trade." With his customary elegance and delightful, though unnecessary, originality he proves that the "common notion" that an import duty will always reduce the volume of imports may be wrong in a general equilibrium framework. If the foreign demand for a given country's exports is inelastic, an import (or export) duty imposed by that country will cause an increase in imports at better terms of trade. There is no reference to Edgeworth, Marshall, or anybody else.

Another real gem in this volume is P. A. Samuelson's 16-page nonmathematical essay on "A New Theorem of Nonsubstitution." The paper states theorems in the area of capital theory, but gives no rigorous proofs which would require elaborate mathematical analysis. Starting with the case of timeless production, "in which all effects of time are non-existent" (i.e., the interest rate is zero) and postulating "a single primary factor—labor," it can be shown that "all prices are equal to the sum of direct and congealed labor." This is easy to see in the hierarchical "Austrian" production structure, but is also true in what Samuelson calls a Marx-Leontief circular production structure where it is impossible to distinguish "early" and "late" stages of production. According to Samuelson, this was shown rigorously the first time by Samuelson and Georgescu Rogen independently in 1949. It is easy to expand the model for the case of more than one primary factor (still abstracting from time).

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[2] Since Pigou's excellent formulation, which is all the more impressive because it avoids economic and philosophical jargon, is not well known, let me quote it: "If the 'art' of social reform is to be effective, the basis of it must be laid in a 'science.' The contribution towards the work of practice that economists aspire to make is to provide for it this foundation. Their effort, though it may well be roused to action by the emotions, itself necessarily lies within the sphere of the intellect. Resentment at the evils investigated must be controlled, lest it militate against scientific exactitute in our study of their causes. Pity however sincere and grief however real are here intruders to be driven ruthlessly away. Stirred by their appeal we have entered the temple of science. Against them its doors are closed, and they must wait without for our return." *Unemployment.* New York: Home University Library, 1913, pp. 10-11.

[3] *Logik, Mathematik und Naturerkennen.* Vienna: 1933, pp. 21-22.

Introducing time, i.e., assuming that production takes time, that inputs precede outputs and time costs money, Samuelson lays down the principle that no use should be made of the "childish device" of assuming à la Clark and others a single homogeneous factor "capital" or "free capital" "until he [the author] has shown he can rigorously formulate his theories completely without them." In reality economic theory has always been full of and to this day abounds with, such "devices," i.e., aggregates whose redundancy has not yet been rigorously demonstrated. I am sure Samuelson will agree that economics would have made little progress if nobody had had the courage to use such concepts before their redundancy had been proved. As far as the special case here under consideration—a homogeneous mass of "abstract" capital—is concerned, many capital theorists have felt uneasy. It is perhaps the central theme of Hayek's *Pure Theory of Capital* to show how to dispense with that abstraction.

Samuelson's generalized "substitution" or more accurately "nonsubstitution theorem" then says: Although there are numerous capital goods "it is nonetheless completely possible to reduce everything [what is this precisely?] rigorously down to a real wage and single interest model," (p. 413); or more concretely (p. 415):

> In a model where one primary factor (labor) is non-producible, where there are myriad capital goods of all types producible by known constant-returns-to-scale technologies, let us suppose there is a change in the money wage rate. In the short run when the stocks of the various capital goods are fixed, there will, of course, be a substitution against labor and the quasi-rents to non-labor factors will fall, their falling being a necessary condition for the transient rise in real [wage] rates. In all likelihood the interest rate or pattern of interest rates will in the short run move downward. If, however, one performs the experiment under long-run conditions, stipulating that all the adjustments of various types of capital goods take place until in the new equilibrium the old interest rate reasserts itself, then the increase in the real wage will have been transient and will have disappeared. If the money wage rate is permanently higher, then all other price relationships will be permanently higher in the same percentage degree.

This implies among other things that (with given constant-returns-to-scale technologies) there will be no substitution of capital for labor when money wages rise unless there is a change in the rate of interest.

It seems to me that all this amounts to a vindication of Böhm-Bawerk against J. B. Clark, of Hayek against Knight, and of Kaldor against Hayek in their dispute over the Ricardo effect (except that Hayek can be defended by assuming that he had the short-run conditions in mind which Samuelson mentions in the quotation above) and of Leontief's criticism of Paul H. Douglas (*Quarterly Journal of Economics, 49,* 147). It is greatly to be hoped that Samuelson's suggestive sketch, which is written in telegram style, will be fully worked out by the author himself or by someone else.

The essays which have been specifically mentioned in this review constitute but a small fraction of the volume's content. There are others by R. Frisch ("Mixed Linear and Quadratic Programming by the Multiplex Method"), Alvin Hansen ("The Changing Business Cycle"), Léon Dupriez ("De la fonction d'utilité de la monnaie"), Oskar Morgenstern ("A New Look at Economic Time Series Analysis"), to mention only a few.

It is a pity that there is no alphabetical index and that the volume does not contain a bibliography, let alone a biographical sketch or an appreciation of the work of the great scholar who is honored.

GOTTFRIED HABERLER

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Toward a Reasonable Society: The Values of Industrial Civilization. By C. E. AYRES. Austin: University of Texas Press, 1961. Pp. 301. $4.75.

Professor Ayres sets out to examine the "forces and processes that have brought about in Western civilization a vast scientific-technological revolution" (p. 14). His interest is not, however, simply the analytical one of elucidating the commingled elements which have led to that result. His real interest lies in the value-system which is inherent in the development, and in the manner in which this value-system does, or may, contribute to the emergence of a "good" society. This involves him in the necessity of breaking down a conventional distinction between objective, *wertfrei* scientific knowledge on the one hand and socially sanctioned values on the other. He argues, on the contrary, that there are values which are valid at all times and in all societies and that these are derived precisely from the matter-of-fact technological knowledge of any given society as against the values which are rooted in tribal belief and make believe and are different for each society.

"Doing-and-knowing, science-and-technology, is the real life process of mankind. This is the process from which modern industrial civilization has resulted, and it is the process in terms of which men have always judged things good and bad, and actions right and wrong" (p. 15). This challenging, if somewhat mystifying, statement is followed by an optimistic conclusion: "Genuine values derive from the life process of mankind, a process to which every man is committed by virtue of being a man. This is the process to which we owe industrial society, and one can therefore say with certainty and precision that the industrial way of life is good—and not only good: the best that man has ever known" (p. 15). The argument of the book is devoted to justifying this conclusion.

Much of the argument proceeds in the context of cultural anthropology.

In this context, Ayres finds it necessary to demolish what seem to him to be the misinterpretations which have been placed upon the results of anthropological research—in particular, the view that "values—all values—are the irrational emanations of the social practices which happen to prevail among the people whose values they are" (p. 22). The moral agnosticism involved in the idea that value judgments are simply the expression of social conventions seems to him not only "essentially false," but "of very great danger to the whole of Western civilization" (p. 24), giving rise to the familiar cynicism that "each generation rewrites history on the basis of its own prejudices" (p. 24).

In passing, he cites the economic doctrine of *laissez faire* as an example, exposes the origins of his view that the marginal economic analysis is "meaningless," and commits himself to the Veblenian idea that "the economy is not regulated by the price system. It is regulated by the institutional structure of Western society, of which the market is at most only a manifestation" (p. 28). Apart from this interjection, and the statement that "the present book represents an act of rebellion: rebellion against the grand tradition of classical political economy and modern economics" (p. 27), the book is devoid of any contact with the disciplines of economic analysis—except for one or two similar interjections late in the book. His precise complaint against the classical tradition is expressed in the following words: "The trouble with the classical doctrine of economic individualism is not that it refuted mercantilism but that in doing so it created a barrier against all subsequent economic planning and therefore against organized intelligence itself" (p. 45). Why his misgivings about Adam Smith and his interpretation of anthropological data are to be regarded as antipathetic to the analytical apparatus of positive modern economics is a point upon which Ayres does not bother to enlighten the reader.

Within its own context, the argument is impressive and in a degree persuasive. That is to say, he shows that matter-of-fact knowledge provides a basis upon which to make rational connections of cause-and-effect, that such knowledge is cumulative, that the beneficial results of such knowledge are regarded as "good" in all societies, and that this sort of "goodness" is different in kind from the "good" sanctioned solely by tribal superstition and convention. It cuts through what he calls "the deep-seated tendencies of modern thought to polarize knowledge and values" instead of "distinguishing clearly between values which are true and rational and those which are irrational and false" (p. 85).

Ayres's own polarization between the rational good which is technologically based and the irrational good which is based on myths and magic takes a good deal of demonstrating and requires him to follow a devious course through the mysterious psychological and cultural traits of mankind which cannot be summarized here. But the argument always circles back to the same point: "The values which the magic charm exemplifies are culture-limited. But the values which the digging stick exemplifies are the same for all people and all ages" (p. 85). ". . . the symbolic process [by which all human behavior is organized] endows mankind with two sets of powers; the power of organizing facts and the power of creating fictions" (p. 103). ". . . there is only one sort of knowledge, the knowledge man has acquired in the course of his technological activities. . . . Apart from these activities, symbols lead only to illusion" (p. 107). Such a thesis obviously requires the exploration into the theory of knowledge and of moral behavior which Ayres makes; but the present reviewer feels no competency to make a critical assessment of his views. I am left, however, with a sense of uneasiness about the process of extreme polarization by which all "real" values are traced to a rational apprehension of cause-and-effect rooted in the cumulative accretion of technological knowledge.[1] Perhaps the critical question is: Can societies exist without myths?

Following his explorations in the anthropological terrain, Ayres turns to the fundamental values which accumulated technological knowledge has contributed to modern industrial civilization. These are listed as Freedom, Equality, Security, Abundance, and Excellence. The argument that the advance of scientific and technological knowledge magnifies these "goods" takes up the last third of the book. There is not space to summarize the argument, but I will sample it with a few quotations:

On Freedom: "The great achievement of the Western peoples has been the progressive filling of the power vacuum that follows the removal of the tyrant not with another tyrant but with another way of life" (p. 184). ". . . the most important freedom for every individual and for any society is freedom from ignorance" (p. 183).

On Equality: "Throughout modern times the industrial revolution has been eroding all three of these foundations of the feudal system: its myths, its social structure, and its way of life" (p. 189). ". . . a middle class . . . was originally a minority group having no distinguishing characteristics save negative ones: nonaristocratic and unbonded to a feudal lord. In this sense ours is now a classless society" (p. 189). ". . . in bringing literacy to the people the printing press at one stroke nullified the advantage which priestly scribes had always enjoyed over the subject population" (p. 197). ". . . the pretensions of inequality . . . originate in the traditional social structures . . . sustained by systems of tribal superstition" (p. 205).

On Security: ". . . the imagined security for which men have invoked Higher Powers derives its germinal meaning from the actual security men have achieved by use of tools and through efficient organization" (p. 209). "What has changed with the evolution of industrial society is the vast complexity of the physical conditions of existence . . . a gradual shift of the focus of attention from ceremonial objectives of power and glory to technological processes of sound economic growth" (p. 218). ". . . the wage system . . . provides the machinery by which the whole community has been able to share the advantages of industrial development" (p. 223). ". . . the wage system itself fails to keep pace with burgeoning production. Thus social security is a device for sustaining aggregate consumption, just as property is a device for sustaining industrial enterprise" (p. 226).

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[1] The polarization is not unlike in kind Veblen's polarization of "industrial" and "pecuniary" employments, though much more extensive in its coverage.

On Excellence: "Has industrial society sacrificed quality for quantity?" ". . . in order to feel a vague uneasiness with regard to the vulgar hurly-burly of the twentieth century. All that is necessary is a correspondingly vague nostalgia for some past era, or even a fixation on some particular feature of some earlier culture" (p. 247). ". . . a methodological error . . . of comparing what is common in one situation with what is best in another" (pp. 247-48). ". . . amenability to diffusion is an essential determinant of true excellence. . . . The supposition that only the elite can appreciate true excellence is not merely snobbery; it is ceremonialism, pure and simple" (p. 254). ". . . is conformity inherently bad? . . . Is it bad that hundreds of millions of people should hear the same language spoken?"

On Abundance: Here I will comment rather than merely quote. Noting that "abundance . . . is one of the most conspicuous facts of modern life" (p. 229), Ayres goes on to note an ambivalent attitude toward it—"the paradox of plenty." "Western society both welcomes abundance and resists it" (p. 229). "When industrial efficiency produces an abundance that threatens to upset the established order of society, then tribal values require that it be resisted" (p. 230). Some of the passages quoted earlier might well have appeared in a brochure of the National Association of Manufacturers; but in this loaded sentence Ayres reveals—or perhaps one should say, conceals—his profound misgivings about the market system. He is back in the Veblenian world of "sabotage," or "conscientious withdrawal of industrial efficiency." The very striking omission in this book is that Ayres nowhere describes, or even suggests, the nature of changes in the institutional structure which would abolish the restraints upon full realization of the productive potential of the industrial system. Still, in his view, abundance is winning out, supporting a limited optimism.

In the same context he takes occasion to air his well-known distaste for "price theory," or the "theory of resource allocation." ". . . many economists find the popular notion that ours is now an economy of abundance extremely irritating . . . they insist that the whole function of the economy is to economize in the allocation of scarce resources among alternative uses not all of which can possibly be pursued at the same time" (p. 236). He also alleges that economists have a "favorite paradox" derived from Keynes to the effect that "the sovereign cure for too much investment is more investment" (p. 238). Here lies Ayres's fatal blindspot. He identifies market theory with apologetics, a form of mystique used to defend the existing social order. I would be the last to deny that some economists get their theory and their theology somewhat intermingled. But Ayres simply refuses to see the necessity for a theory of "economizing" as a means to understanding the characteristics of the economic order.

This observation leads me to end this review on a note of bewilderment. Modern industrialization has proceeded within the context of a definite structure of socio-economic institutions. Some *agency* has been responsible for the introduction of the technological improvements to which Ayres attributes so great an advance in the real values of society. But this agency, or the institutional complex within which it operates, is never mentioned. Technology by some mysterious cumulative process develops and works its wonders in the improvement of man. In an "institutionalist" this failure to mention modern economic institutions appears to be an odd oversight. Presumably they too have inculcated some values. Are these all magic and superstition?

Ayres places a high value on democracy and attempts to subsume it under his thesis of the derivation of true values from technology. ". . . in democracy considerations of operational efficiency are paramount" (p. 284). ". . . the democratic process is a process of learning the truth and operating accordingly, and the unanimity toward which the process aims is that of the universality of science and technology" (p. 285). This sort of teleology and "animism" ought, one would suppose, to be absent from Ayres's manner of thought. In any case, is one free to dissociate democracy in the political sphere from that high degree of freedom in the economic sphere which appears to be supported with "virtual unanimity" in at least some portions of Western civilization? Or is this that spurious form of unanimity "produced by emotional conditioning and . . . therefore limited to the particular community to whose beliefs and practices the members of the community have been emotionally conditioned" (p. 284), where democracy has not yet learned what its teleological functions are? Ayres will, I fear, have to write another book on contemporary political and economic institutions to rescue me from my present bewilderment.

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Money, Trade and Economic Growth—Survey Lectures in Economic Theory. By Harry G. Johnson. Cambridge: Harvard University Press, 1962. Pp. 199. $4.50.

This little book, chiefly of articles reprinted from journals not widely read in the United States, should have something for all. Four papers on "Trade and Growth," in the formal, deductive style of which Johnson has shown himself a master, treat of the balance of payments, commercial policy, customs unions and effects of economic development. Two chapters on "Money" offer a lucid summary of Keynesian macroeconomic theory and a critical review of it. The final section on "Economic Growth" includes "Planning and the Market in Economic Development" and two fine Galbraith-type critiques of the economic theory and social policy of our "opulent society."

No dogmatic defender of the rule of "free" market forces, Johnson is aware that international economic issues frequently involve "choices between sub-optimal situations" or "the theory of second-best" (p. 43). He recognizes, nevertheless, "that the existence of a balance-of-payments problem assumes the presence of a monetary authority which intervenes in the foreign exchange market to peg the rate of exchange. . . ." (p. 17). And he is sobering to those who may assume blithely that controls on trade will always be less painful than exchange rates adjusted to the underlying demand and supply of foreign currencies.

Johnson is partial in his analysis to the "absorption approach," associated with Sydney Alexander, in the extended context of the work of James Meade. (This does not inhibit his examination of the logical implications of various output and demand price-elasticities at a less than fully aggregative level.) Johnson would thus view a balance-of-payments "surplus"or "deficit" as a discrepancy between domestic output and domestic expenditure and would examine measures to restore "balance" in terms of "expenditure-switching" and "expenditure-reducing" and effects on domestic output.

Applying this approach to the problem of exchange instability, Johnson appears to this reviewer to understate that problem in declaring, ". . . exchange instability implies that a reduction in the price of the country's output, relative to the price of the output of the rest of the world, will reduce the quantity of the country's output demanded" (p. 24). For the difficulty is that, in the case of a payments deficit, an exchange devaluation may increase the quantity of the country's output demanded and hence reduce the deficit *measured in foreign goods* but still increase the deficit measured in terms of the domestic output that must be produced, or against which the country must borrow, in order to meet that deficit.

Johnson's discussion of "the economic theory of customs union" is interesting and timely. This theory, as he points out, is "complicated," and results are not always what might be anticipated by the simple notion that any extension of a free-trade area is good. For not only may parties outside the union be injured but, under certain conditions, a participating country as a whole (not merely certain pressure groups within a nation) might be better off without the union. In general, "a country is less likely to gain from entering a customs union, the more different it is from its partners, the more the partners produce close substitutes for goods produced in foreign countries, and the greater the difference in real costs between foreign and partner supplies of such products; also the more elastic is the foreign supply of such products; and the more the country concerned is a producer for the world market rather than for the market of one or more of the partner countries" (p. 58).

"Monetary Theory and Keynesian Economics," presented originally in 1956 as a lecture in Pakistan, is an excellent, literary and diagrammatic exposition of the Keynesian system. "The General Theory after Twenty-Five Years," given to the American Economic Association at the December 1960 meetings, is eloquent and well-written but has somehow a harsher ring. With reference to the *General Theory* one learns now of its "untidy construction" (p. 127), that "insistence on . . . the necessary identity of savings and investment . . . . clutters up dynamic analysis" (p. 133), of "the false impression, of which Keynes himself was the chief victim, that holding securities is the only relevant alternative to holding money" (p. 135), of "the neglect of the influence of wealth on consumption" (p. 140), "that Keynesian underemployment equilibrium depends on wage rigidity unless the wealth-effect peters out before full employment is reached—an empirical possibility which only a few die-hards have been prepared to defend" (p. 142), "that Keynes drastically overgeneralized a particularly bad depression" and "stagnationists do still exist in the modern world; but . . . they are certainly not underconsumptionists" (p. 143), and that somehow "a theory in which money is important . . . turned into the theory that money is unimportant," resulting in a "bias that the majority of Keynesians have . . . against allowing money, and consequently monetary policy, an important role in determining the level of activity of the economy" (p. 145).

A full response to all of these points, and others, is hardly in place here. But surely one should insist on the identity of saving and investment while considering the implications, whether for dynamic analysis or comparative statics, of differences between saving and *investment demand.* The option of holding goods in addition to money and securities was discussed in some detail in the *General Theory* in the lengthy, if tortuous, Chapter 17 on "The Essential Properties of Interest on Money" (pp. 222-44), and its role in the Keynesian system was early brought to the fore in work of Lange, Hicks and Somers. The influence of wealth on consumption was explicitly considered in the *General Theory* under the heading of "windfall changes in capital-values not allowed for in calculating net income" (pp. 92-93, 95-96); that those making postwar forecasts of consumption forgot this is not properly accountable to either the theory or its author.

Johnson himself does not defend the "Pigou effect," the ultimate weapon in the assault on the concept of persistent unemployment with flexible wages, as *empirically* plausible. His language is therefore strange in that the "die-hards" might argue that only at a conceptual level is there any issue. At this conceptual level the ultimate dependence of underemployment equilibrium on wage rigidity (that is, the potency of the Pigou effect in the presence of flexible wages) requires at least three critical and *theoretically* arbitrary assumptions: (1) that the private sector is a net creditor—there must be a government debt or money (Treasury money) not backed by private debt; (2) that the government, out of money illusion (!) or a conscious countercyclical policy (!), allows its real debt to the private sector to vary appropriately; and, (3) that, particularly in view of the increased attention given to life spans and permanent income in Modigliani-Friedman-type consumption functions, the motive of accumulation in an older, Marx-Weber sense may not set an upper bound, conceivably short of full employment requirements, to the stimulus to consumption from increases in wealth.

The basis for viewing monetary policy as a poor countercyclical tool does go back to the *General Theory,* where it is pointed out that "the long-term market-rate of interest will depend, not only on the current policy of the monetary authority, but also on market expectations concerning its future policy" and, particularly, "a conviction that its policy will not greatly change in the very near future" (pp. 202-03). How can one attain the low long-term rates Keynes (and others) have considered desirable from the point of view of optimal investment and distribution policy, or lower these rates sufficiently in a depression, if the market learns from repeated experience that the monetary authority will "tighten" money in every boom and balance-of-payments "crisis"? As for overgeneralizing the depression and underconsumption of the 'thirties, I hate to think that we include in our *general* model of the economy a permanent military budget that makes up directly ten per cent or more of aggregate demand. If this is a special case, maintaining full employment demand by increased consumption, whether private or public, remains a correctly emphasized goal.

Johnson's concluding papers on the "opulent society" (one of which is hitherto unpublished) are particularly interesting and worthwhile. He emphasizes the extent to which we all live off invested capital, whether of stocks of consumer goods or of human skills. In regard to the latter, he points up the ignorance, uncertainty and inadequate resources at the disposal of the individual family unit which is responsible for so much of our investment in education. These are contrasted with the huge resources and market control available to the giant corporation. Yet, ironically, since our free society prohibits slavery, the corporation has every incentive to invest in physical capital but little incentive to invest in human capital. (One may wonder anew at tax credits for business purchase of equipment while the United States Congress fails to pass measures in aid of education.) I recommend Johnson's observations on these subjects highly. In fact, I recommend the whole book.

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Research Methods in Economics and Business. By ROBERT FERBER AND P. J. VERDOORN. New York: The Macmillan Company, 1962. Pp. xiv, 573. $9.00.

This book is designed to provide a systematic approach to research in applied economics with special emphasis on marketing problems of the firm and analysis of national economic aggregates. The authors identify three categories of prospective use for the volume: as a text for a course in research methods, as a reference manual for graduate students engaged in dissertation research, and as a reference manual for economic and market researchers in industry. For all three categories the book undertakes to furnish both help in organizing problems for research and an elementary introduction to a variety of research techniques. It includes examples of the application of specific research techniques to business and economic problems, and exercises designed to build research skills.

After some introductory discussion of the value and uses of a systematic approach to research, Part II (146 pages) outlines a number of techniques of quantitative analysis of economic relationships (frequency distributions, variance, regression, time series, cross sections, and mathematical techniques for decision making). In Part III (234 pages), the text reviews problems and methods in collecting and analyzing cross-section business and economic data (surveys, questionnaires, sampling, panels, scaling, attitude studies) and techniques for analyzing time-series data, concluding with the construction of general and specific economic models. Part IV (108 pages) is devoted to a discussion of the application of some of the techniques covered earlier to two practical problem situations: forecasting and the design of a complete marketing audit for a firm. Each part concludes with a listing of references for further study.

This book is both useful and exasperating. On the useful side, it opens a number of important subjects for the fledgling researcher and gives him an elementary exposure to problems, techniques, and applications. On the exasperating side, it is unbalanced in its treatment of simple and difficult subjects. The discussion enters in considerable detail into the conduct of field surveys. On the other hand, mathematical decision-making techniques and the building and use of economic models are dealt with in relatively brusque fashion that plunges quickly into complex considerations. These issues are treated at a pace and level that could readily baffle beginning students. The result is that many instructors would, and should, hesitate to assign the book as a continuing basic reference. A comparable difficulty would be encountered by independent researchers who might turn to the book for elementary self-education. Against this, many parts of the text offer substantial educational values. In addition, the supplemental reading suggestions are well selected and informatively characterized. On balance, therefore, the volume can be assessed as a useful addition to the list of research handbooks, with the warning that it should be employed with caution because of inconsistencies in content level.

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Economics: Principles, Problems, and Perspectives. By HOWARD S. DYE, JOHN R. MOORE, AND J. FRED HOLLY. Boston: Allyn and Bacon, Inc., 1962. Pp. xii, 627. $7.75.

"Let us begin in a thoroughly common-sense manner," wrote Schumpeter. "What distinguishes the 'scientific' economist from all the other people who think, talk, and write about economic topics is a command of techniques that we class under three heads: history, statistics, and 'theory'. . . . Of these fundamental fields, *economic history*—which issues into and includes present-day facts—is by far the most important."

Dye, Moore, and Holly are obviously in agreement with Harvard's stimulating teacher. The distinctive feature of their new text clearly is its strong historical and dynamic-institutional emphasis, aimed at providing perspective for studying the ever-changing economy of the twentieth century. Concentrating "upon economic growth, stability, and income," the authors use history "to develop both the factual and philosophical background" of these "most important problem areas."

A sense of history, of the dynamic quality of U.S. capitalism and the inevitability of institutional change, runs throughout the book; yet the other techniques identified by Schumpeter as necessary for competent economic thinking are by no means neglected. The text is adequate in its treatment of statistical data and statistical methods and does a superior, though concise job in the traditional areas of price theory and national income analysis.

Economics is divided into six parts: General Introduction and Historical Survey (74 pages); Income, Employment, and Stability (95 pages); Policies to Achieve Growth and Stability, including money and banking and government finance (110 pages); Theory of Prices and Production (90 pages); Problem Sectors in the American Economy, including income distribution, the business sector, agriculture, and labor (140 pages); and Growth and Stability in the World Economy (91 pages). A summary and list of study questions follow each of the 26 chapters. Too meaty for a three-hour, one-semester course, it would serve well with supplementary readings, for the typical two-semester, six-hour course.

Dye, Moore, and Holly present the theory of national income determination as clearly and completely as is done in other leading principles texts, including derivation of a statistical consumption function, shifting consumption functions, aggregate demand and the saving-investment approaches, and multiplier and accelerator analysis. Moreover, they consider more carefully than most the price and output effects of changes in aggregate demand at various levels of employment. Their treatment of money and banking under the heading of "Policies to Achieve Growth and Stability" will help the student to appreciate more deeply the theory of a monetary economy rather than regarding money in more isolated, pre-Keynesian, "neutral-veil" terms.

Chapter 25, "Problems and Policies of Economic Development," is an excellent brief treatment of the subject for both underdeveloped and more affluent societies, with effective use made of major data sources and recent growth literature.

With an acknowledged emphasis on macroeconomic problems, what becomes of traditional micro theory? Here again they do virtually everything that other authors do, and then go an extra step or two. An example is the treatment of elasticity of demand and supply, which includes not merely price elasticity, but also income, cross, and "promotional" elasticity. The theory of the firm under pure competition makes use of total revenue and total cost curves, then proceeds to the more traditional average and marginal analysis. Profit maximization is lucidly explained for competitive and monopoly models, and concise treatment of monopolistic competition and oligopoly is included. The standard diagrams and charts are all there, with the exception (alas!) of the kinked demand curve. Value questions are not overlooked, as so often happens when economists analyze "value theory." Dye, Moore, and Holly raise such questions: "Is pure competition ideal?" and "If we could obtain income distribution as measured by competitive marginal productivity analysis, does this mean that income would be fairly or correctly distributed?"

Chapter 5, "Measuring Economic Activity," provides the reader with a quick insight into some fundamentals of national income accounting, measures of the changing value of money, adjustment of GNP for population growth. Statistical data and methods are treated both on a sectoral and aggregate basis.

This brings us back to the really distinctive feature and outstanding virtue of the text: its historical-institutional orientation. "Economic development is a cumulative process. The development of a new institution leads to the introduction of still other institutions until, ultimately, the overall institutional framework is changed. And in the process society's value structure changes—new ideas and goals replace the old. Also, new resources are discovered, new uses are found for old resources, and new techniques for resource utilization are developed" (p. 38).

Three chapters are specifically historical, dealing with "Economic Development: From the Middle Ages to the Industrial Revolution," "From the Industrial Revolution to Modern Times," and "Essentials of American Growth." The relationship between ideas and economic development is illustrated in brief treatments of the doctrine of just price, the Protestant ethic, Adam Smith's invisible hand and policy of *laissez faire*. Even the villainous Karl Marx and John Hobson make their way into this elementary study of the U.S. economy. To determine how distinctive this historical emphasis was, I checked 40 introductory college texts. I found that only a single book presents more than a bare one-chapter outline of the evolution of our economic system, and 35 do virtually nothing with the history and development of an industrial market economy. If we could assume that supplementary readings and lectures filled this gap in historical analysis in the typical principles text, there would be less cause for concern. There seems, however, to be little basis for such an assumption.

This book is really excellent; but it is not the end-all in principles texts. Students will find the exposition clear but not particularly entertaining. Certain areas, such as the microeconomic aspects of public finance, are given inadequate treatment. The authors do not develop adequately the notion of a public economy and political consensus as a complementary or alternative valuing mechanism to the market. They seem to exaggerate the degree of consensus among economists regarding tax equity (p. 249 f.), and might be misunderstood in their statement on recent trends in federal compared with state and local expenditures (p. 246).

For those who go along with Schumpeter's criteria for economic competency, *Economics* by Dye, Moore, and Holly will surely be welcomed as a useful, challenging, and thoroughly competent teaching aid. To a greater extent than perhaps any other recent introductory textbook, it presents clearly and forcefully the basic tools necessary to "think, talk, and write about economic topics" in a competent manner.

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Economics—Principles and Issues. By DELBERT A. SNIDER. Homewood, Ill.: Dorsey Press, 1962. Pp. xvi, 654. $7.

This compact (654 pp.) text is intended for the introductory course. In terms of topics included and analysis employed, it may fairly be characterized as quite conventional. Some notion of the relative emphasis upon and ordering of topics may be had by considering the parts into which the volume has been divided.

Part I. Background and Organization of Economic Life (7 chapters, 138 pp.): In addition to introductory scope-and-method topics, this part discusses the monetary system (including the Federal Reserve System and money creation).

Part II. Economic Growth (4 chapters, 68 pp.): This part introduces basic national income concepts and deals with growth problems in both underdeveloped and developed countries.

Part III. Economic Stability (5 chapters, 92 pp.): This part includes the theory of national income determination, analysis of the causes of instability, and monetary and fiscal policies to achieve stability.

Part IV. The Allocation of Resources (15 chapters, 276 pp.): This part includes the theory of consumers' behavior, theory of the firm, competitive price determination, nonperfectly-competitive market analysis, and the role of government in resource allocation. For the book as a whole, the relative emphasis is upon allocation, microanalysis. Although, by implication at least, the author presents much of this material as if it constituted adequate descriptive (predictive) science (a source of annoyance for this reviewer), the results which emerge are largely in terms of the general concept of economic "efficiency," i.e., in the rather abstract terms of welfare economics implications of equality versus nonequality of marginal costs and prices (a pleasing feature for this reviewer). The final three chapters in this part are concerned with foreign trade and international monetary relations.

Part V. The Distribution of Income (3 chapters, 66 pp.): This part extends the microanalysis to pricing of inputs and the determination of functional and personal shares. It also discusses distribution issues and policies.

With respect to content and organization, the author observes in his preface: "Comparison of the Soviet Union's economic system with that of the United States is drawn throughout the book, not relegated to a separate chapter." Reading this to imply that such comparisons would be a substantively important feature of the text, this reviewer was cheered—having long felt that this would make a good format for the introductory text. So read, however, the statement quoted proved rather misleading. Although various features of the Soviet economy are accorded a few pages and sentences at various places in the text (pages 35-46, 60, 312, 332-35, 622), in general very little information on this topic is imparted. The treatment of this topic is more "in passing" than it is an effort to present any systematic picture of the organization of the Soviet economy or the economics of central planning.

In the author's view (see preface), while the principles course should deal with the development of fundamental principles (rather than mere recitation of historical and descriptive material), it should not be "excessively technical." He notes his effort "to make the book rigorous without being pedantic." In aiming at these goals, the result is a book at a very modest level of technical complexity—and a book which contains relatively little historical and descriptive material. In this reviewer's opinion, the book would be improved by the inclusion of considerably more basic data (e.g., relating to macroeconomic performance, status of underdeveloped countries, balance-of-payments accounts, and so on).

The author's style is, generally, commendably clear and concise. There is rather lavish use of graphical exposition for largely illustrative purposes (see

Part IV). However, graphical exposition tends to be avoided in some contexts (e.g., equilibrium of national income) in which, it is to be hoped, it might have done some theoretical "work," i.e., permit the student to deduce results not intuitively obvious to him or results he could not deduce as readily by alternative means.

In the textbook field, one man's meat is frequently another's poison, and evaluative review is extraordinarily difficult. For one thing, along with the text, the services of the instructor and, usually, additional readings are inputs to the course. How the text should be contrived depends in part upon how one conceives the functions of the other inputs. Nevertheless (and without elaborating these points) a few general comments are in order.

Taking the basic book format as given, a general difficulty this reviewer felt with this text is suggested by the author's statement (p. vi): "Where theoretical niceties and refinements are not required, I have refrained from indulging in them—and have even restrained the temptation to mention in footnotes they exist and would be discussed if only the students were a little brighter." One consequence of this posture as it is reflected in the book is that it tends unnecessarily "to talk down" to the average student and, this reviewer would fear, fails to provide the intellectual horizons necessary to really interest the better student. A good case can be made that the introductory text should suggest (perhaps in footnotes, appendices) the nature of the analysis which lies ahead of that elaborated for present purposes—and that it should "indulge" in a few "niceties and refinements" where these are necessary to accomplish this purpose. This is, of course, controversial ground, and clearly a balance must be struck. The reviewer can only say that, for his personal taste, the lower level of technical complexity in this book has been purchased at too high a cost in terms of conceptual richness.

At a more general level of evaluation, a major question with respect to this text is concerned simply with the fact that, in terms of its basic format, it is conventional. Although generally a most dubious position, perhaps in the textbook field it is at least partially legitimate to criticize an author's book on the grounds that he didn't write some other book. However, there is in the profession currently considerable dissatisfaction with the typical design of the introductory course. In this reviewer's opinion, what the introductory course may well need is not additional texts along conventional lines, but rather efforts to experiment with rather different approaches.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Studies in an Inflationary Economy. By F. W. PAISH. New York: St. Martin's Press; London: Macmillan, 1962. Pp. ix, 336. $10.00.

Most of this book consists of a selection of F. W. Paish's essays which were either published or delivered as public lectures during the 1950's. The major stress is on the analysis and problems of inflation. Paish ascribes the inflationary pressures in the United Kingdom between 1953 and 1958 to a general excess of demand, particularly in the form of fixed investment. He explicitly rejects the notion that wage-push was a significant factor in the inflation.

In one of the most interesting parts of this book, the author sets forth his most recent views about the sources of Britain's postwar difficulties, together with his policy recommendations. In his earlier papers, he had identified inflation as a condition in which incomes rise faster than output. In his final chapter, which he wrote especially for this book in July 1961, he has shifted the emphasis away from output as such because "the significance of a given rise in output cannot be appreciated unless we know whether it was accompanied by a rise or fall in the margin of unused resources." The key to this latest analysis is the assumption of a stable relationship between the rate of rise in money incomes and the proportion of productive capacity currently employed. Given this assumption, the author concludes that long-run stability depends on stabilizing the margin of unused capacity at the critical level at which money incomes rise at an annual rate equal to that of the growth of productive capacity. Paish calculates that, for the postwar period in England, the required margin was not less than 5 per cent and perhaps as much as 7 per cent. This corresponds to an unemployment rate between 2 and 2½ per cent.

Paish presents a good deal of evidence to support his contention that the basic cause of Britain's recurrent postwar difficulties can be traced to the fact that, except for eighteen months from the end of 1957 to the middle of 1959, and possibly also during the last three quarters of 1952, the minimum margin of excess capacity was exceeded. This analysis is the basis for his proposal that public policy should seek to stabilize the margin of unused capacity at a level at which money incomes rise at the same rate as capacity. He expects that the adoption of this technique ". . . would enable the Government considerably to reduce the magnitude of fluctuations in the growth of output and the level of employment, as well as to prevent or reduce rises in prices and balance-of-payment difficulties."

How well this expectation would be borne out in practice would depend to a great extent on how stable the critical relationship involving the unused margin actually proved to be. In addition, however, the results would depend on the way the guide was used. At the present time, policy-makers must often make their decisions in the face of confusing and even conflicting indications from the existing list of policy guides. If the proposed guide were used as a supplement to the existing list, its signals would sometimes clarify the picture given by the other guides, but at other times its signals might aggravate the uncertainty of the policy-makers. In either case, its impact would be limited by the extent of the dependence on the other guides. The uncertainty in the minds of policy-makers could be removed and the proposed guide could have a much greater effect if it were used as an exclusive guide for policy. By itself, however, the proposed guide would not be reliable, for the same reason that stability of prices is not a reliable guide. As Paish has shown, a period of stable prices will be temporary if the margin of unused capacity falls below the critical level. In a similar way, however, a period of stability in the unused margin might also be temporary if structural distortions should develop within the economy. Since the unused margin is an aggregative index, it might not be sensitive to those distortions until the strains they produced had become acute. By the time the index had signaled the need for a change in policy, the corrective counteraction would be overdue.

Although the analysis of the nature, causes, and control of inflation is the dominant theme of the book, the author has also examined many other topics, ranging from a critical analysis of the concept of liquidity as it appears in the report of the Radcliffe Committee to his reflections on the functions of industry and on the role of the economist as scientist. In between he has also discussed the international economic position of the United Kingdom (especially emphasizing Britain's decline as a capital-exporter) and various topics in business finance, including the London new-issue market, company profits and their distribution since the war, and the economic functions of the stock exchange. The chapter on the functions of the stock market is particularly worth mentioning as an example of Paish's ability to bring a fresh and penetrating viewpoint to an old topic.

This is a stimulating and provocative book in which theoretical insight, statistical interpretation, and historical perspective have been combined to illuminate alternatives for public policy. It should find an eager audience among readers with an interest in both the theoretical and applied aspects of the topics which are covered.

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Ökonomische Theorie der Sozialpolitik. By Elisabeth Liefman-Keil. Berlin: Springer, 1961. Pp. xv, 424. DM 49.80.

The book is intended to present a theory of social policy (*Sozialpolitik*) which, in its German form, means essentially income policy. The German tradition goes back to the famous Verein für Sozialpolitik and was perhaps best expressed by G. Schmöller when, in his inaugural address as the first president of this organization in 1871, he called upon all governments to mobilize their resources for the purpose of "correcting the income distribution." The author feels that income distribution is largely determined by the ownership of productive wealth and that therefore corrective policy has to concentrate on wages policy and on social transfer payments, especially social security. She disclaims adherence to the belief of John Stuart Mill that income distribution is determined institutionally rather than in the market, but agrees that social policy has to concentrate on institutions which "determine" distribution, e.g., institutions of the labor market (p. 4).

In the first part of the book aims, instruments, and strategy of the policy are discussed, e.g., justice, equalization of income margins (aims); price policy referring to wages on the one hand, and goods on the other, fiscal policy, law-making procedures (instruments); and organized group action (strategy). The last is presumed to have a bias toward egalitarianism and middle-of-the-roadism in a democracy (for which interpretation writings of

De Tocqueville, J. A. Schumpeter, H. Hotelling and A. Downs are adduced). The second part deals with income redistribution by way of social transfer payments of various types. The influence of redistribution on the size of the national product, that is, the aggregate income, is emphasized by the sequence of discussion. Redistribution is discussed first, and aggregate income as well as labor and wages policy are discussed second. The final part is devoted to international problems, e.g., the influence of protectionism and of international organizations. Many parts include brief "theoretical appendices."

The foregoing implies perhaps the criticism that the writer has done too little by doing too much. Thus it is with reluctance that the reviewer finds fault with an omission. The author remarks that social transfer payments have increasingly become a substitute for individual ownership (p. 408). This might well be the case. But she omits a very pertinent facet of German social policy which, the reviewer believes, is of great, and increasing, importance. In recent years a large number of academically and politically influential economists in Germany have addressed themselves to the very problem of erosion of ownership, both functionally and by quantity, and propose to counteract the trend by widening the distribution of productive wealth. (To mention only a few: Minister of Economic Affairs L. Erhard in *Property for All;* O. von Nell-Breunig in *Theses on Wage Earners' Ownership;* Zweig and Feldhege in *Property for Everybody.*) Considerable legislation has been enacted during the 1950's promoting this redistribution. Neither the writers nor the laws are mentioned by Miss Liefmann-Keil. This is even more surprising as her own assumption, above mentioned, that ownership largely determines income, would seem to lead toward redistribution of ownership as a primary remedy of maladjustments, and as the statistical facts show that the legislation pertinent to this problem has not been entirely unsuccessful.

The reviewer is not certain that the word "theory" means exactly the same in German as in English usage. The German use has a certain bias toward emphasis on taxonomy, and Miss Liefmann-Keil's work is not the only good and worth-while book on theory with this taxonomic flavor. This does not necessarily reduce its value; the book offers, in a concise and well organized way, minutely subdivided by many pertinent viewpoints, an encyclopedic overview of recent thinking and policy measures, mainly with regard to Germany, referring to planning for income, income distribution, and income redistribution. Anybody interested either in German economic policy or in income policy in general will read it with profit and pleasure.

FREDERICK G. REUSS

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What Keynes Means. By ANATOL MURAD. New York: Bookman Associates, 1962. Pp. 223. $4.50.

Professor Murad, formerly of Rutgers and presently of Puerto Rico, in this new book attempts a comprehensive appraisal of the three facets of the Keynesian system—namely, "science, policy, and ideology"—on the basis of his lecture series given as a Fulbright visiting professor at the University of Münster in 1958. Those particular chapters of his book on "The Prophet of the New Economics," "The Keynesian 'Revolution,'" "Economic Policies Suggested by Keynes," and "Evaluation and Criticism of the General Theory" will be found of general interest while the rest consist of pedagogically useful and constructively critical explanations of the familiar stuff of the *General Theory*. Inasmuch, however, as the author seeks to unravel the "ambiguities and contradictions" of Keynes' theories of demand and interest as his main contribution, I concentrate on these particular criticisms of Murad's.

With regard first to the Keynesian theory of demand, Murad points out Keynes' opacity concerning "expected" demand and "realized" demand, and then offers to distinguish the former as "the *subjective* aggregate demand" from the latter as "the *objective* aggregate demand." But then every theorist knows, though not every student may know, that the relevant Keynesian demand function is an *ex ante* theoretical one instead of an *ex post* statistical one. Is not the author being a bit too punctilious and fastidious here, as well as elsewhere in his book?

Next, Murad objects to the customary statement that "$C + I$, or total expenditure, will equal income only in equilibrium." He argues that "income, Y, must by definition always be equal to $C + I$, regardless of whether or not income and employment are at the equilibrium level," and then regards this definitional equality of income and expenditure as "a reassertion of 'Say's law' which Keynes was out to disprove" (p. 62). This terminological difficulty which Murad attributes to the *General Theory* can easily be surmounted by making, as every instructor in income theory should, an explicit distinction between a genuine equation of the form $Y_{t+1} = (1 - s)Y_t + I$ and a mere identity of the form $Y \equiv C + I$. Put in the former form, an equilibrium solution emerges from the working of the dynamic adjustment mechanism of the form $Y_{t+1} - Y_t = f(sY_t - I)$, provided that the stability condition $dS/dY > dI/dY$ is satisfied. By contrast, the second of the above forms is incapable of explaining the behavior of the whole economy. In retrospect, Keynes did for the whole economy what Leon Walras did for the commodity market, to wit: replacement of Say's law by an equilibrium condition to be established, albeit through "deliberate action" (Keynes) and "market forces" (Walras). All this is pointed out here by way of implementing Murad's own specification of "repudiation of Say's law" as a distinguishing and smashing feature of "the Keynesian Revolution" (p. 168).

Perhaps it is a reflection of Murad's principal concern with monetary theory that he should pay so much attention to the Keynesian theory of interest which, in his view, makes only "a negative contribution showing the older theory wrong" (p. 138). First, Murad complains about the supposed "tautology" involved in Keynes' proposition that interest is the "reward for parting with liquidity." Murad imputes a tautology to that proposition by idiosyncratically asserting that "the interest income which they [people] derive from holding debts is not the 'reward for parting with liquidity,' but the 'reward' for holding interest-yielding assets in place of other assets" (p. 142). In an uncertain and risky world, which Keynes contemplated, there is a presumption in favor of holding liquid assets from the precautionary and speculative motives, thus giving effect to the "store of value" function of money stressed by Keynes. If these motives are sufficiently strong, as in a cycle-sensitive economy, then it is but natural for people to prefer liquidity to nonliquidity as a matter of personal security and amenity. Under these circumstances people would have to be given some inducement before they would part with liquidity and shift to nonliquidity, and that inducement takes the form of a positive rate of interest. Otherwise nonliquid assets like bonds could be perfectly interest-free. So long as the real world remains uncertain and risky in respect of the future, just so long will the savers have the alternatives of holding their savings in the form of liquid assets and holding them in the form of nonliquid assets, the choice between those alternatives depending on the relative advantages to be derived from them. Thus in the presence of conflicting motives and alternative opportunities the aforementioned Keynesian proposition does not at all involve any tautology, for a positive rate of interest is at least one important factor affecting people's responses to those motives and opportunities.

Failure to recognize these alternatives confronting wealth-holders leads to Murad's misleading complaint that the Keynesian liquidity function should be "a supply-of-media-of-payment schedule" instead of a demand for money schedule (p. 142). Moreover, this complaint is inconsistent with his earlier admission that the supply of money is determined by the central bank "on grounds of public policy" (p. 135). This inconsistency is in turn coupled with a misunderstanding of the supply-of-money function in the *General Theory,* for Murad ignores the central-bank-induced shifts of the vertical, interest-inelastic supply of money function $M = \overline{M}$ when he complains about that function not being "a horizontal line" instead (p. 143). Such a misunderstanding can arise from failing to distinguish between the steepness of the $\overline{M}$ curve expressed by $(d\overline{M}/dr)/(\overline{M}/r)$ and its *position* expressed by $\overline{M}_1$, $\overline{M}_2$, or $\overline{M}_3$. Thus, contrary to Murad's apprehension, the Keynesian supply-of-money function, with its zero interest-elasticity, is compatible with the practical requirements that the central bank should supply an "elastic" currency via the member banks.

One other flaw in the Keynesian theory of interest found by Murad is Keynes' supposed failure to recognize interest both as "a property income" and as "a payment for a service" (p. 146). On this supposition Murad naturally finds it "puzzling" that Keynes should advocate the "euthanasia of the rentier" (p. 146). However, one has only to recall Keynes' distinction between "the pure rate of interest" due to the "scarcity-value" of money capital and "the gross yield of assets including the return in respect of risk" (*General Theory,* p. 221) in order to realize his awareness of the income *and* cost aspects of interest. It is this latter cost aspect (or "a payment for a service," in Murad's terminology) which makes the "capital-charges" entering the cost of production "oppressive" to the entrepreneurs (especially interest-sensitive ones) by relentlessly necessitating the higher and higher marginal efficiency of capital as the only inducement to invest. And "capital-charges" as such are made unduly high by the inclusion of that "pure rate of interest" which Keynes sought to abolish while at the same time realistically setting an institutional minimum to the desired fall in "the cost of bringing borrowers and lenders together" (*General Theory*, p. 219).

My extended comments on Murad's criticism of the Keynesian theory of interest must not end without calling the reader's attention to his keen appreciation of the historical significance of that theory, namely, that it has served to effect the transition from the classical emphasis on change in the interest rate as the *modus operandi* of the economy (on the assumption of full employment with constant income) to the modern emphasis on income change as the relevant *modus operandi* (on the new assumption of less than full employment with variable income).

Murad's interpretations of Keynes both as a scientist and as a policy-recommender on the one hand seem to be more provocative than, though just as fair as, those of Dillard in his *The Economics of John Maynard Keynes* (1948), while on the other hand his evaluations of Keynes as an ideologist and as a history-maker appear to be more forthright than, though just as searching as, those made recently by H. L. McCracken in his *Keynesian Economics in the Stream of Economic Thought* (1961).

The volume under review is accompanied by a helpful résumé of Keynes' principal works and exciting career. This reviewer, for one, would recommend the Murad book not only to the serious students of income-employment theory but also to the practical men of affairs seeking light rather than heat in the economics field.

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National Income and the Price Level: A Study in Macrotheory. By MARTIN J. BAILEY. New York: McGraw-Hill Book Co., 1962. Pp. ix, 304. $6.95.

This is a book which when it concentrates on a proven cast of characters turns in a highly creditable portrayal of modern income theory, but when it wanders backstage or tries out additional bit-players, runs the risk of losing its audience.

Bailey's exposition is organized around Hicks' famous synthesis of the cash-balance and product markets (here called the "monetary" and "expenditure" sectors respectively). Those accustomed to savings-investment analysis being stated in *ex ante* and *ex post* terms will be surprised to find these concepts studiously avoided. Apart from this, the author's exposition is first-rate, making expert use of both equations and diagrams. Its very clarity, however, suggests how limited the Keynes-Hicks model is for analyzing the financial-flow aspects of economic activity. Given the conventional framework, Bailey still is remiss in failing to expand the discussion of financial claims beyond money itself. Presumably the author regards recent monetary controversies as largely definitional in character (see p. 192).

The introduction of the price level brings complications to the two-sector model. Because of the assumed constancy of the supply curve of cash balances in nominal terms, there are as many possible supply curves of real cash balances and thus as many monetary-sector curves as there are possible price levels. Simultaneous equilibrium in the monetary and expenditure sectors can thus take place at various combinations of interest rates and national income levels.

This indeterminancy is overcome by the introduction of a third sector—the production and employment sector. A full-employment level of real income can be specified given equilibrium in the labor market. At any other price level than the one effecting over-all equilibrium, disequilibrium in the expenditure sector will act to restore the price level to a unique value.

The possibility that consistent equilibrium solutions may not be reached is one of the concerns of the third chapter ("Advanced Analysis of National Income Determination"). Inflexible real wages (or inflexible money wages not offset by monetary policy), horizontality in the monetary-sector curve above the intersection of the expenditure-sector curve with full-employment income, or finally an expenditure curve falling to the left of the vertical full-employment line—all of these conditions may be responsible for inconsistency solutions.

Real cash balances play an important role in this chapter. Expected changes in such balances help explain inflation. As an additional variable in the consumption function, real cash balances make the expenditure sector responsive to changes in the monetary sector, whereas previously the direction of impact was exclusively the other way. Introducing real cash balances as an additional variable in the production and employment sector means that full-employment levels are now also affected by monetary change.

The multiplier effects of government expenditures lose their customary simplicity at Bailey's hands. A variety of multipliers can be specified, depending on the assumptions made about consumer response to government consumption and investment expenditures and government deficits. The number is further increased if we make alternative assumptions about tax receipts (a continuously balanced budget, fixed total tax receipts, tax receipts fluctuating with income).

The plausibility of the model and the likelihood of consistent solutions (without government intervention) are both made doubtful by some of the findings of Chapters 7 and 8, ("The Effects of the Rate of Interest and Real Cash Balances on Consumption," and "Measurement and Prediction: An Introduction"). Bailey is dubious about the efficacy of real cash balances even on theoretical grounds, and empirical studies have failed to establish a determinate relation between interest rates and investment. His exclusive concern in the empirical chapter with single equations to the neglect of full-scale econometric models is not consistent with his own elaboration of an interdependent model.

The policy conclusions (Chapter 6, "Policies for Economic Stabilization") follow from the model and from principles of optimum resource allocation. Monetary policy should be combined with tax policy so as to preserve full-employment equilibrium levels. The optimal amount of government expenditures should be determined by comparing private and public marginal productivities with market interest rates. He is critical of past Federal Reserve policy and is pessimistic about the possibilities for future effective monetary and fiscal policy. Nowhere in the chapter is there an appreciation for the complexities of policy-making—for the problems posed by conflicting goals, by rising prices in the midst of unemployment, and by an institutional framework which frowns on direct controls.

The rest of the book represents the author's attempt to break new ground and has a distinctly peripheral flavor: the feasibility of disaggregating the model into one of general equilibrium, the availability of a substantial backlog of investment opportunities at low interest rates (Chapter 4, "Some Problems in the Analysis of Equilibrium"); the consistency of imperfect competition with general equilibrium solutions (Chapter 5, "General Equilibrium with Imperfect Competition"); forecasting techniques based on exponentially declining weighted averages of past values (Chapter 9, "Expectations and Adjustment to Change: A Key Link in Cumulative Movements of the Economy"). In the first-mentioned chapter, why the author should have anticipated a disaggregation problem to begin with, is not clear. The conclusions in the hard-to-follow imperfect competition chapter have the same negative character. Imperfect competition is found to have no more of an aggregate output effect than that ordinarily associated with negatively sloping demand curves.

An appendix on the "Concept of Income" will probably have more interest for the average reader. Unfortunately, in recommending that a wide variety of imputed transactions be counted in the national income, the author seems to be courting a ***reductio ad absurdum*** in the income concept.

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Der Kostenpreis bei öffentlichen Aufträgen. By HELMUT DIEDERICH. Heidelberg: Quelle & Meyer, 1961. Pp. 236. DM 25.00.

This book is one of a series of five publications of the Research Institute of Economic Policy at the University of Main dealing with the problem of public contracts. Diederich presents a systematic economic analysis of cost pricing. Although cost pricing has been in principle applied to public contracts it can be used advantageously in the control of cartels and similar marketing combinations. It is also helpful in price regulation for transportation and public utilities.

Diederich analyzes the shortcomings of cost pricing and emphasizes that these shortcomings force governments to limit the use of this pricing standard. The author also presents suggestions as to how the cost-price principle can be improved as a substitute for market price to forestall excessive or unsound profit making in public contracts.

Chapter 1 deals with the deficiencies of cost pricing in fields of its application and also includes a critical evaluation of the government regulations. Diederich points out that despite the strict control of prices in Germany during the Second World War the average contractor who worked for the German Armed Forces earned a 4-5 per cent profit in excess of the officially approved returns on invested capital. Our experience is not much better despite the activities of price-control agencies during and after the last year. It is also evident that cost pricing per se does not stimulate industrial managers to use the production factors in an optimum way and that it can erode or eliminate entirely performance incentives.

Chapter 2 on correct cost pricing is analytically the most interesting part of the book. The author evaluates the impact of perfect and imperfect competition on cost prices and monopolistic and oligopolistic tendencies in the case of public bids. He also recognizes that there is always monopoly on the part of the purchaser (the government).

Only reasonable costs (average costs and not marginal costs) should be utilized, for a contractor with a poor technical organization and many bottlenecks in his production line should not expect a "just price" covering his inefficiency. Reasonable costs should be based on the costs of an average plant since there may be striking differences in costs due to the plant location, size of production facilities, supply of raw materials, working conditions, utilization of available capacity, and diversification of products. If fixed cost prices cannot be agreed upon, provisional (guide) prices are used in Germany, supplemented in some cases by a price escalator clause (cost-reimbursement type).

Although Germany published several price regulations prior and during the Second World War (see Louis Domeratzky: *Price Control in Germany—Policy and Technique;* Washington, 1941), pricing policy in Germany is now based on regulations published by the Federal Minister for Economy in 1953 (for defense contracts) and in 1955 (for public construction work).

Diederich compares the U.S. and British price regulations with those prevailing in Germany. He focuses his comments on the Department of Defense, *Armed Services Procurement Regulations* (1960 edition) and the Department of the Army, *Army Procurement Procedure* (1957 edition, with changes No. 1-23 of June 23, 1960). U.S. defense orders are, as in Germany, divided into two groups: procurement by formal advertising (public bidding), and procurement by negotiation (usually cost-plus contracts). U.S. directives covering the use of fixed prices are more explicit than those used in Germany. In exceptional cases fixed-price incentive contracts are used which provide that the contractor must share his profit accruing from his efforts (e.g., better management efficiency) with the government. In the cost-plus-incentive-fee contract the level of the profit addition is dependent on the ratio of actually incurred costs to the given standard costs.

The British system has generally been based on market prices and fixed cost prices while cost-plus contracts were limited to repair and research work. The "target cost" contracts in England are similar to the U.S. fixed-price incentive contracts. In the so-called "maximum price contract" the contractor is paid the actual costs ascertained by post-costing, plus a rate of profit stipulated by the government, limited by the maximum price.

In his third chapter Diederich describes and evaluates those costs that should be included in a "just price" including compensation for services of the proprietor (in partnerships and sole proprietorships), premium the contractor is entitled to charge for the risk and uncertainty of profit, and interest on capital invested by the entrepreneur. These items are controversial in

U.S. cost accounting practice. In Germany, it is permissible to use the reproduction value of fixed assets as the basis for computation of annual depreciation but this is not allowed in the United States where initial costs must be taken as basis.

Diederich's book viewed as a whole is a thorough study of war and postwar pricing policy in Germany compared with that in the United States and Great Britain. However, he fails to mention U.S. price regulations during the Second World War and the Korean War, and recent publications in this country on small business pooling for defense production. Since Diederich analyzes U.S. and British public contract pricing, it would also have been helpful if he had mentioned Soviet pricing policy based on the labor theory of value, though presently some Soviet authors (cf. recent publications of I. S. Malyshev and V. A. Sobol) recommend that government contractors should base pricing on average production costs plus a certain amount of "surplus value" (corresponding to our profit).

In a comparatively extensive bibliography Diederich lists only one book published in the United States subsequent to the Second World War, namely, G. W. Lupton: *Government Contracts Simplified* (Washington, 1954), though there are many other interesting studies by, for example, J. P. Miller (1945), Doris P. Rothwell (1946), D. Reck (1951), J. V. van Sickle (1952), and P. M. Trueger (1953). Apart from two descriptive books, Henry A. Cohen's *Public Construction Contracts and the Law* (New York, 1961) and Institute on Government Contracts: *Lectures Presented in 1961,* no book on public contracts has been published in the United States recently, although some $122 billion are spent annually by federal and local governments.

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Income, Employment, and Economic Growth. By WALLACE C. PETERSON. New York: W. W. Norton & Company, 1962. Pp. xii, 550. $6.95.

This is an intermediate text in aggregate economic analysis and concerned with the three basic problems of output (employment), prices, and economic growth.

Part I (Chapters 2-3), "The National Income," attempts to explain the fundamental concepts in national income accounting. Some instructors may find it objectionable when indirect business taxes are equated with capital consumption allowances in the statement: "Like indirect business taxes, these allowances [capital consumption] are a non-income charge against the output totals" [p. 62]. And it is also unfortunate that an important distinction between national income at factor cost and national income at market price is lost when the author makes the statement that the net national product concept is not widely used because ". . . existing measurement techniques do not permit an accurate measurement of real capital consumption" [p. 64].

More important, perhaps, is the failure to give such social accounting techniques as "input-output" and "flow of funds" more than a passing reference.

Part II (Chapters 4-13), "The Theory of Income and Employment," is, in the main, devoted to the standard concepts of modern (or Keynesian) equilibrium income analysis. An introductory chapter sketches out the main elements of a "classical" income model, but the failure to put all the various identities and functions of both the "classical" and "Keynesian" models into complete systems makes it difficult to show the fundamental difference.

Some instructors may find it refreshing to see the usual economic relationships treated as *ex ante* rather than as *ex post.* Others, however, may find that the attempt to force the traditional *ex post* algebra and diagrams into an *ex ante* framework creates complications that are not worth the effort. One example is the author's definition of the 45° diagonal in the typical income-spending diagram as an *ex ante* supply function rather than as simply a reference line which equates income and spending in the *ex post* sense.

The implication that inventory disinvestment is the immediate result of investment *ex ante* exceeding savings *ex ante,* and the author's assumption that the marginal propensity to tax may be so high that the balanced-budget multiplier may become negative, are two other examples of minor items about which some may have reservations. One chapter on the theory of money and interest (centered on liquidity preference) concludes the first 380 pages of this 550-page book.

On the whole it seems to this reviewer that these first 380 pages seldom take the student beyond that level of analysis found in the better elementary texts; the employment of almost 70 per cent of this intermediate text for what is essentially a review of elementary economics will probably detract from its usefulness in those schools where the emphasis in the elementary course is analytical rather than descriptive.

The last two chapters of Part II are concerned with the analysis of aggregate price levels (demand-pull, cost-push, etc.) and the distribution of national income among functional shares (labor, capital, etc.). It is at this point that the student is taken beyond the elementary level and introduced to some current economic debate and the reasoning found in the work of such writers as Ricardo, Marx, Kaldor and Weintraub. The treatment of these ideas is brief but succeeds in blocking out the main lines of analysis and should stimulate the good student to further reading.

Part III (Chapters 14-16), "The Theory of Economic Growth," is built around the Harrod-Domar models, and the last chapter dealing with "economic growth, structural change and government policy," is especially well done. The author keeps strictly within the confines of the previous economic analysis and, eschewing those sociological and psychological factors that are often brought into discussions of growth analysis when the economic model is found wanting, illustrates some of the dilemmas of economic growth for the underdeveloped world.

It should be pointed out that, throughout the book, most of the ideas are presented with a minimum of mathematics and, when mathematical relations are used, care is taken to caution the student about the limiting assumptions.

One cannot finish a review of a textbook without feeling somewhat apologetic for both the criticism and the praise. The standard the reviewer brings to his task is as subjective as that of the author. Any textbook that earns a completely laudatory review may owe as much to the accidental juxtaposition of author and reviewer as to any innate qualities of the book. Fortunately, most of those teaching manage to make do with a variety of texts that they find objectionable in varying degrees. Some find a better solution by writing their own texts, while the majority will undoubtedly continue to use the texts of others which they will supplement to fit their own views as to the best way to organize the instruction.

In spite of this particular reviewer's reservations, any student who reads Peterson's book should learn much about the basic analytical concepts found in modern economics. And, if a student is fortunate enough to have a competent instructor, even the "faults" may assist the learning process. All of which is a long way around to say: *"chacun à son goût"* or *"de gustibus non est disputandum."*

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Einführung in die Wirtschaftstheorie. Pt. 4, Vol. 1, Ausgewählte Kapitel der Geschichte der Wirtschaftstheorie. By ERICH SCHNEIDER. Tubingen: J. C. B. Mohr (Paul Siebeck), 1962. Pp. viii, 423. DM 28; paper DM 23.

This book is a selection of readings in the history of price theory interlaced with a careful exposition of the chosen theorems. Covered are the circular flow theories of Quesnay, Marx and Böhm-Bawerk (46 pages), classical price theory and its modifiers (110 pages), the various versions of marginal theory (150 pages), the German historical school (48 pages), as well as the influence of Marshall in England (16 pages), and the theories explaining the market structures between competition and monopoly (32 pages). Three appendices are attached that reprint Quesnay's Tableau Economique, Walras' memorial of Gossen, and the exchange of letters between Jevons and Walras in 1874.

Being written by a German theorist, the book gives a somewhat greater emphasis to continental writers. As the successors of the classical theory one finds not only Cournot's theory of competitive price, but also Hermann's six price determinants, and especially Mangoldt's theory of international demand as well as of joint demand and joint costs. (The essay on Mangoldt was first published in the *Festschrift* in honor of Ragnar Frisch.) In order to take account of the various beginnings of marginal theory, J. H. von Thünen is presented as the originator, Dupuit and Gossen as "the real founders," while Menger, Jevons and Walras are treated extensively because they independently completed the theory. Pareto is not much praised for having dispensed with marginal utility and for having relieved the theory of its hedonism, but is seen mainly as the successor of Walras, since Pareto gave a more general expression to the general equilibrium theory. Marshall's contribution is said to lie primarily in his introduction of time in the partial equilibrium analysis of supply. Duopoly theory begins by contrasting the contributions of Cournot and Launhardt, then summarizes the ideas of Edgworth and Bowley, and culminates with Frisch's theory of market behavior.

This distribution of emphasis is allied with the author's contention that students of economics should be well versed in contemporary theory before they study the history of economic doctrines. Accordingly, the author presupposes that the reader has worked through his three previous books of this *Einführung,* which deal with the theories of the circular flow, of relative prices, and of employment, respectively. The organization of the volume under review, seen as Part IV of a comprehensive work, had thus to be adjusted to the earlier statement of theories, which partially explains the abbreviated treatment of Pareto and the central position of Frisch's theory in the last chapter. The same didactic motive may also have moved the author to avoid any discussion of methodological issues. For instance, the controversy on the measurability of utility is merely mentioned (the main references are relegated to a footnote), and the arguments of the two schools of thought remain unstated. Present controversies growing out of historical doctrines are thus not used as means of instruction; the student is confronted with an authoritative statement of earlier doctrines as seen by present theorists.

Far from distorting the historical record, employing contemporary theory as criteria for selecting doctrines for review is said to lead to a superior form of understanding past theories. The history of doctrines is seen as a process of continuous intellectual development. The pattern of theoretical *Entwicklungsstufen* is stated most clearly in the marginal theory, in the case of which the individual contributors are regarded as originators, founders, culminators and successors. If the asserted linkage between them had really been that of historically documented successive developmental stages, why then had the same principles to be discovered several times in the same century? Continuity of the same theorem was absent. Even when knowledge of new theories was accessible, Marshall did not accept the general equilibrium theory but presented his partial method as a superior alternative that dominated English thinking for about 50 years. Didactically selected problems thus do not produce a tenable history of doctrines.

The German historical school did not supply us with a functional price theory. Its inclusion in the present volume seems to have been based on the notion that even so-called blind alleys constitute *notwendige Durchgangsstufen zur richtigen Erkenntnis.* Rather than show this necessity, the author merely reprints the programmatic theoretical intentions of some of these writers and noticed that these promises were not kept. His subsequent review of the *Methodenstreit* leads him to the conclusion that Menger won the battle for the deductive method of theorizing. As evidence for this, the student is given a long quote from Menger's pamphlet that insulted Schmoller by charging him with ignorance, dishonesty, and ruthless opportunism. One can only feel dismay in rereading this pamphlet which is a classical example of academic status politics, since the attacker claimed the greater share of professional honor by trying to prove that economic history as a branch of economics can only be recognized if it is reduced to a handmaiden of deductive theory.

As a matter of fact, it was Max Weber who distinguished between statements of value and of fact, who separated the historical from the typological method of investigation, and who recognized the ideal-typical nature of pure theory when he gave a detailed critique of the historical school.[1] In employing a modified version of this typological method. Walter Eucken succeeded in developing the morphological theory of markets. It is in terms of a typological, rather than functional, price analysis that the contribution of the historical school to economic knowledge can be assessed.

In all, the main attempt of the author to select certain doctrines and employ them for supplementary instruction in functional price theory certainly has been successful. Hardly anyone, however, will be able to say the same for the proposed new method of writing doctrinal history.

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[1] "Roscher und Knies und die logischen Probleme der historischen Nationalökonomie," *Gesammelte Aufsatze zur Wissenschaftslehre,* Tübingen 1922, pp. 1-145.

Economic Theory in Restrospect. By MARK BLAUG. Homewood, Ill.: Richard D. Irwin, Inc., 1962. Pp. xvi, 633. $7.95.

Professor Blaug's text-monograph, something of an updated "survey of contemporary economics" and its history, is erudite in substance, rigorous in formulation, and generally well written. At the least, it will serve the splendid purpose of making the specialist in dead men less disreputable in the eyes of his theory-teaching colleagues.

The author's purpose is "to teach contemporary theory," but theory "cannot be fully understood except as a legacy handed down from the past" (p. ix). Maybe so. But some who strive to master modern theory may feel that struggling with Ricardo's "invariable measure of value" and Marx's "transformation of values into prices" is taking rather the long way around. Must we be given (dubious) reasons for studying the history of theory? Is it not sufficient that, for some of us, it is just good, clean fun?

Unencumbered (or unadorned) by biography, sociology of knowledge, economic history, or noneconomic intellectual history, the book is delimited in scope. But even this lengthy and well-packed volume cannot adequately survey all of the topics of modern theory it undertakes to discuss. At many points the discussion is manageable only by mature students. However, if the student already knows the theory, the historical review is more summary than introduction and guide: the theory is better used by Blaug to study the history than is the history to learn the theory. If teaching contemporary theory is the big purpose, it is discouraging, after some 45 detailed pages on Ricardo, to reach the conclusion that, "What has really survived is [only] the Law of Comparative Cost and the method of comparative static analysis" (p. 128).

The book is delimited also in being generally confined to major analytical topics and to leading figures. There is little or no mention of historical, institutional socialist (other than Marx), utopian, and romantic-nationalist writers. Even some of the more conspicuous giants are given short shrift, Menger perhaps most notably. The survey begins with mercantilism; 25 pages later, we come to Smith (to whom is devoted a chapter of only 25 pages, compared to one of 45 pages for J. S. Mill); by page 270, we reach the marginalists—well over half the book being assigned to less than the last century. And in the process of rigorously restating and evaluating old theories (with a penchant for mathematical formulation), Blaug sometimes takes occasion to incorporate modern analysis; e.g., supplementing a brief review of physiocracy is a reference to input-output, and along with his treatment of Malthus is a discussion of Harrodian growth.

A more subtle and important delimitation is the emphasis on the "words" of theory at the expense of the "tune," on elegance (not without an occasional touch of pretentiousness) over wisdom, on abstract virtuosity in lieu of worldly comprehension. In the context of appraising Smith, Blaug gives precedence to "grasp of the essential logic of economic relationships" over "brilliance in handling purely analytical concepts"—"for acute insight into the nature of the economic process, it would be difficult to find Smith's equal" (p. 57). But this is at the end of 18 pages (virtually the entire chapter) of small print devoted overwhelmingly to "purely analytical concepts."

"Reader's guides" are presented of Smith, Ricardo, Mill, Marx, Marshall, Wicksteed, and Wicksell, ranging from 14 to 19 pages (except for a briefer guide to Wicksteed). These are a sort of annotated table of contents, with commentary bulking larger than full-blown exposition. Most of the authors selected for this special treatment are sufficiently obvious choices, but perhaps Malthus (whose assessment by Blaug is scarcely overly generous), Say, Cournot, Jevons, and Menger have equally impressive claims.

There are no footnote citations (even for direct quotations), but at the end of each chapter is a highly valuable annotated bibliography aggregating some 38 pages.

Despite preceding comments on what the book is not, as well as what it is, let it be emphasized that this is a work which was decidedly worth doing. (Fellner's *Modern Economic Analysis* may be the closest substitute, but it is shorter, generally pitched at a simpler level, and not as comprehensive in historical coverage.) True, the mathematical consideration of what happens to the rent share in Ricardo's distribution model may better expose the author's own finesse than a vital segment of modern theory; and such an undiluted dose of pure theory may convey the impression—is it wholly wrong?—that, as the "economic boxes" have become more and more precious, the worth of their content has not risen proportionately. At any rate, it is good to have a book which systematically gives the hard substance of historically developing theory rather than the usual poetry.

But it is too bad that the given substantive content was not spread over an additional 50 pages, so that non-Yale undergraduates, too, would find it manageable.

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Main Currents in Modern Economics. By BEN B. SELIGMAN. New York: The Free Press of Glencoe, 1962. Pp. x, 871. $11.75.

Both Schumpeter's unfinished *opus* and, more recently, Samuelson's presidential address, have emphasized the distinction between the economic "thought" and the economic "analysis" approaches to doctrinal history. One wonders whether proponents of the two views will initiate a useful dialogue, culminating in another kind of "neoclassical synthesis." Or perhaps as T. W. Hutchison *(A Review of Economic Doctrines: 1870-1929)* said of the 19th century methodological discussions, "history and analysis in the main [will go] their separate ways, each more or less tacitly acknowledging, or at least tolerating, the role of the other."

Seligman, who begins his study at the same point in time as that of Hutchison, makes his choice of method abundantly clear. He has never, he says, been "overly sympathetic with efforts to make economics a matter of pure analytical technique." And, while he accepts "technical competence as a requirement for effective economic reasoning," he compares it with the necessity to walk before running (p. viii). In his avowedly institutionalist approach, economic policy stands at least at 100 per cent parity with economic theory.

The organization of the work follows from the assumption that "the history of economics today is tripartite in character" (p. ix). First, "The Revolt against Formalism" examines the earlier historical reaction, emphasizing German and British contributions. This is followed by an analysis of the socialist (primarily Marxist) attack upon orthodoxy. Finally the American post-Veblenians are given a thorough discussion in a subsection on Institutionalism.

"The Reaffirmation of Tradition" is the theme of Part II, the first section of which traces marginalism from Jevons through the "Libertarianism" of Von Mises. Treated separately is the evolution of general equilibrium systems from Walras to Leontief, while the last subsection deals with the development of ideas in England from Wicksteed to such distinguished contemporaries as Robertson, Hawtrey, Robbins, and Shackle.

Part III, "The Thrust toward Technique" opens with an unusually detailed discussion of the Swedish school. This is followed by an interesting study of American developments which contrasts the "Young Radicals": Ely, Patten, etc., with more "traditionally" oriented thinkers. The work is completed with a rather more heterogeneous section which ranges from "Realism" (Schumpeter) to the "Ultimate in Technique" (game theory and linear programming).

In view of the ambitious scope of the work, it would be hopeless to attempt any detailed summary of its content. It is enough to say that the extensive treatment of the minor Marxists, the American Institutionalists after Veblen, and the Swedish school would alone constitute a most useful contribution to the critical literature. The subsection on the Americans may also become a standard reference. Useful throughout are Seligman's thorough studies of the less well known policy and reform statements of some otherwise well-known economists. One does occasionally feel, however, that theoretical issues deserve more systematic, if not more rigorous, treatment.

But perhaps it is more useful to ask whether the work advances the hypothetical dialogue between historians of thought and historians of analysis. The query inserted in the chapter heading: "Leon Walras: the Greatest Economist?" and the conclusion that problems stated by Seligman, ". . . which raise virtually all the major economic questions of the day, can find little place in such a theory" (p. 386), suggest a desire to contest the position argued by Schumpeter in his *History of Economic Analysis*. One is forced to conclude, however, that the gap between "thought" and "analysis" is left as wide as ever. Consider two brief examples of a recurring theme: ". . . perhaps more psychology and history and sociology are necessary if we are to grasp the true nature of the human animal" (p. 385); and again, "The political climate, the basic drives and aims of a society are too complex to be entirely compressed into mathematical equations and marginal curves" (p. 729). If one turns from these reiterated statements with the feeling that all this has been said before, this is also true of Seligman's prescription: "The ultimate understanding of a changing economy may well require the modern theorist to resort to detailed institutional analysis, much as he may not want to do so" (p. 729).

Even as one would hesitate wholeheartedly to disagree with either the diagnosis or the prescription, so one might want to challenge the imputation of disinterest or even hostility to the "modern theorist." One would have liked, for example, to hear Seligman's comments on Shubik's call for a "mathematical institutional economics." And while a rather reluctant admiration is revealed for Boulding's efforts to build general systems theory, one wonders whether the author would be willing fully to accept Boulding's dictum that "science is a process in which unimportant questions which can be answered are substituted for the important questions which have no answers."

But perhaps the contemporary institutionalist, like his historicist predecessors, may make his best contribution as a gadfly rather than as a synthesizer. In any event, Seligman's book can occupy a useful place on any economist's bookshelf.

ROBERT CAMPBELL

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Elementary Mathematics of Price Theory. By CLARK LEE ALLEN. Belmont: Wadsworth Publishing Company, 1962. Pp. xv, 155. $3.95.

The text is divided into three parts. Part A is an elementary presentation of the properties of functions and graphs. The treatment is much like that of the ninth-grade algebra course. However, the problems are chosen to illustrate applications to elementary price theory. Problem 3 on p. 15 is representative: "Henry Schultz estimates the supply curve for imported sugar in the U.S. in 1903-1913 as $S = 1.1P - 0.1$. Find the price if the quantity is (a) 1; (b) 0.8; (c) 0.5." It should be noted that it is usual at this level for the pedagogue to stress units of measurement. Is P measured in dollars per pound, and is S measured in pounds?

In Part B, elementary topics in the calculus are developed. Included are simple and partial differentiation, integration, homogeneous functions, and assorted price-theoretic applications. The Euler theorem (one of several of that name!) that is known from its importance to the theory of the distribution of national income is stated and artfully applied to various production functions. It is too bad that the theorem is not proved.

Part C, entitled "Geometry," is independent of Part B. Using the less powerful vehicle of two-dimensional geometry, the relationships between average quantities and marginal quantities are established anew. The approach of this part is good for two reasons. Because so much of the literature is argued in diagrammatic form, the student ignorant of the geometric constructs is at a disadvantage in seeking an economics education. Second, by having mastered these concepts in a fresh medium, the student is likely to have greater confidence in the results. Also included in this part is a careful presentation of the elasticity concept and an excellent geometric discussion of the nature of linear programming.

The book has a bibliography, an index, and four appendices. Appendix A is a selected review of mathematical fundamentals. Appendix B is on nomographic solutions—the technique of engineering graphics now exploited in the statistical control of inventories. Appendix C is to remind the student of general equilibrium problems. Answers to the odd-numbered exercises are to be found in Appendix D.

It is obvious that great care has been taken to ensure that this product will be of use for a wide class of potential consumers. But like most such products, this text serves any particular user quite imperfectly. This is a book of techniques not to be used for teaching elementary price theory. It is recognized that today's professional student of economics should be exposed, at least, to the calculus. To introduce the mathematical concept of limit in a price theory course fails to recognize obvious advantages of a pedagogic division of labor. While it is difficult to imagine that this book could serve well as a text, many students will desire this handy paperback copy in their libraries for reference use.

KARL SHELL

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Economic History; Economic Development; National Economies

Growth of Industrial Production in the Soviet Union. By G. WARREN NUTTER with ISRAEL BORENSTEIN and ADAM KAUFMAN. Princeton: Princeton University Press for National Bureau of Economic Research, 1962. Pp. xxvii, 706. $15.00.

This is an important book, whether judged by the issue it treats, the work that has gone into it, or by its public impact. It is also a big book, containing over 600 pages. It contains three distinct communiqués. Over half the book is a report to the specialist—a carefully annotated catalogue of the *q*'s, *p*'s, and *v*'s taken as the raw material for Nutter's measurement of Soviet growth, and an explanation in almost worksheet detail of their manipulation to construct a variety of index numbers. This part of the book is essentially complete in itself. Secondly, there are eight chapters which discuss and interpret Soviet growth as revealed in these calculations. Finally a ten-page summary tries to set out the general findings of the study in its entirety. It is primarily at this third level of generality that the book has had its public impact. Critique of such a book might legitimately cover both its technical approach to the basic data, and the generalizations and interpretations distilled from these underlying calculations.

The central issue in the study is the rate of Soviet industrial growth. I think that any specialist, having worked through the technical half of the book, cannot but admire the workmanship displayed in the construction of the indexes. The painstaking assembly of data, experimentation with alternative weighting systems, the allowances for such complications as territorial changes and armaments production, would leave even the most ruthless thesis adviser at a loss to suggest "important questions that ought also to be discussed." There are comparisons with Tsarist growth, several kinds of comparisons with U.S. growth, comparisons with labor input to give measures of productivity trends, and many other instructive applications of the indexes.

The measure of Soviet industrial growth that Nutter apparently considers to have the most general validity (as evidenced by his carrying it through to the summary) is an index for a sample of slightly over 100 output series comprising about half of all industrial output. The weighting system is complicated, but its main features are a moving weight base and, as far as practicable, use of value-added rather than gross-value weights. This index shows Soviet output growing by 6.2 times between 1913 and 1955, with virtually all the growth taking place after 1928. (The 1928 index is 2 per cent above 1913.) Against the background of other indexes that have been published by scholars outside the Soviet Union this is a modal conclusion rather than an extremal one. The fundamental quarrel over this index is whether we get a better insight into the capabilities of the Soviet growth machine by taking the 27th root of 6.2 and concluding that Soviet industrial growth has been at a rate of 6.9 per cent per year or taking the 42nd root and concluding that the rate has been 4.4 per cent. But that issue is forced in this book only when we move from the index to interpretation, and disagreement with Nutter's conclusion on this point does not reflect on the index itself.

A second important concern of the book is the ratio of Soviet to U.S. industrial output. Nutter asserts that despite the differential in the U.S. and Soviet growth rates the ratio of Soviet to U.S. indusrial output has crept up only from 14 per cent in 1913 to 23 per cent in 1955. In the summary he adds that this 23 per cent might as well be too high as too low. This finding must be evaluated deep in the appendices, rather than at the level of interpretation, and examination of his calculations suggests that 23 per cent is far too low. Drawing on his basic set of Soviet output data he determines Soviet-U.S. quantity relatives for a sample of commodities in 1955. These are aggregated using first U.S. value-added weights, and then ruble value-added weights, to arrive at two index numbers of relative output in the part of industry covered by his sample. The output relative for the sample is about 40 per cent. (His dual approach discloses a slight index number effect, but that is not the central problem.) He also derives the average ruble-dollar ratio for value added implicit in this price and quantity information. Since the index of relative output applies only to a sample of 45 industries, he adjusts it to refer to all industry by asking what share of total value added in each country is accounted for by the industries in his sample. (His method can alternatively be thought of as converting value added in all Soviet industry to dollars by means of the ruble-dollar ratio found for the sample.) Since the sample turns out to account for only about a quarter of U.S. value added but about half of Soviet value added, the 40 per cent ratio for the sample is adjusted downward to 23 per cent for all industry.

The weak element in this calculation is the data for ruble value added. Such information is not explicitly available in any Soviet source and has had to be produced by devious estimation. The value-added prices estimated for individual commodities seem generally to be much too large—indeed some of them are simply gross value. On the other hand his estimate for value added in all industry seems too low. It is derived from some Soviet statements referring to the industry sector in Soviet national income accounts. A rather larger estimate can be derived starting with data on the gross output *(valovaia produktsiia)* of industry. The latter approach is probably preferable since much more is known about the definitions and practices underlying the concept of gross output of industry than about national income accounts. These two biases make his estimate of the share of all industrial value added accounted for by his sample too large, and his ratio for Soviet-U.S. industrial output too small.

Many of the other calculations and conclusions of the book call for the same kind of critique as these two issues but if the book is to be evaluated in general terms, one can only say that it is a great accomplishment that deserves thorough study both to appreciate its contributions and to check some of its data assumptions.

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On the Theory of Social Change: How Economic Growth Begins. By EVERETT E. HAGEN. Homewood, Ill.: Dorsey Press, 1962. Pp. 557. $7.50.

This is an important book. It is the most creative and provocative contribution that has yet appeared in the field of economic development and it must be savored by all who work in this area.

"The Nature of the Problem" sets the tone, arguing that "the study of societies must be a theory of personality, for the analysis of societies is the analysis of human behavior." Hagen is thus concerned with those factors of personality that transform a stagnant traditional society into one that experiences continuing technological progress. Those who lead the escape from tradition are identified as members of a group which, having once had status in the traditional society, lost ground, and regained it only later, after several generations, as new avenues for advancement opened up and as new personality types were moulded.

Hagen then deals with personality and the stability of traditional societies. His description of these cultures is one of the best in the literature and all who work in underdeveloped countries should feel themselves on familiar terrain. These societies tend to be "custom-bound, hierarchical, ascriptive, and unproductive." The phenomena of the world are seen by members of these societies as arbitrary and not amenable to analysis. They feel uncomfortable raising questions about these phenomena, and instead they find security in performing their ascribed roles, which are usually humble ones but nevertheless respected. Awkward problems one does not have to solve. They can be kicked all the way up to God, corporeal or incarnate. Decisions can be resolved by passing the buck or, if one is further up the hierarchy, one's status is confirmed by the right to give orders.

Rarely does the creative individual emerge in such an environment. A creative individual is one who can use his subconscious productively. He assumes that the world will respond to his initiative and does not feel threatened by unresolved situations. These attitudes, together with a burning desire to perform, are not induced in the authoritarian framework. One of Hagen's greatest contributions is to lead us through the synthetic world of psychology in such a way that it becomes clear exactly how the authoritarian personality is formed in an authoritarian society and how personality and society mutually reinforce each other to induce a stable social equilibrium. In performing this task he has not only interpreted for us the obscure doctrines of our social psychologist colleagues but has also bridged gaps in their thinking where it has been necessary to build a model of personality methodologically suited to the needs of the economist.

We can only hint here at the mechanism of personality formation. It begins early in life of course. The creative individual must somehow receive encouragement, demonstration, advice, guidance, restraint, and a perception of being valued. Anxiety to achieve is important and this may arise if performance, but not unreasonable achievement, is expected of him. The individual destined to be bound by uncreative traditionalism, on the other hand, is never regarded as having independent capabilities of his own, and he is protected from the threatening world by being absorbed neatly into an orderly hierarchy with its limited, prescribed roles and responsibilities.

A powerful force is needed to disturb the equilibrium of a traditional society. The process begins as some group within the society finds that it no longer is accorded the respect by others that it had come to expect. Hagen argues that the anxiety which this loss of status respect causes in adults will alter the home environment in predictable ways which, accumulating over several generations, may lead members of this group to embrace roles of creative leadership.

The personality of mature individuals cannot be fundamentally changed, but their reactions to altered status change their overt behavior in the home and thus affect the more malleable youth. Initially the changed personality becomes retreatist but eventually the creative personality may arise. Several roads to creativity are possible. A son, for instance, may be blessed with a nurturant mother who, humiliated at retreatist weakness in the father, tries to help the son to strength. The son, perceiving his father's loss of status respect may rage at those he feels responsible. If, however, he rejects the values of the aggressor group he will squeeze himself out of society. On the other hand he may find possibilities arising within the society which permit him to acquire some of the status of his father's detractors without actually crossing into areas forbidden to him. Thus, while the traditional elite may eschew technical skills, the manipulation of technology may open to some individuals channels by which they can acquire wealth, and hence some degree of acceptance by the elite. This description does not do justice to the subtle and perspective analysis offered by Hagen but is offered merely to suggest his line of reasoning.

The latter half of the book attempts to test, by means of case studies, the hypothesis that entrepreneurs arise in the early stages of growth from groups that had formerly lost status. Included are studies of England, Japan, Colombia, Indonesia. Stagnation through traditionalism is illustrated by the examples of Burma and the Sioux living on the bleak Dakota reservations.

It is not possible to read a book such as this without raising questions. The least satisfactory section is that in which the author tries to bolster his own case by attacking economic theories of growth. The arguments are thin and unworthy of the remainder of the book. For example, Hagen initially denies that inadequate markets are obstacles to industrial development since "where larger plants have not appeared in traditional societies the most important reasons typically are that the community finds the change in behavior patterns somewhat repugnant. . . ." Later in Chapter 12, it is admitted that market size might be important after all. But apart from that, this facile assertion completely evades the basic problems of lack of technical knowledge, managerial experience, and so on that necessitate small-scale beginnings for indigenous enterprises.

Hagen's economic analysis prompts a gnawing doubt. This reviewer has no competence to appraise the author's use of history or social psychology. If these applications are as inadequate as the book's economics then many economists are going to be misled. In addition, the theory should be tested against the evidence of societies which do not seem, at first glance, to fit. Authoritarian Germany, for instance, or New Zealand, where the groups that have lost status may be difficult to identify. And should we discuss Soviet development in terms of the same type of creative leader?

The general impression that one receives from Hagen's work is a good one. His presentation is clear and powerful. His insights are superb and one is carried away by the conviction that here, at last, is the key to the question that has been worrying us: How does economic growth begin in a free society? Hagen is surely a creative personality.

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Teoria della politica economica e pianificazione regionale (Theory of Economic Policy and Regional Planning). By FRANCESCO PARILLO. Milan: A. Giuffre, 1962. Pp. 150. L. 1.200.

In this publication, which originated at the Institute of Economics of the University of Messina, Parillo builds upon his previous studies concerning a theory of economic development (see: F. Parillo, *Contributo alla teoria della politica economica*, Torino, UTTET, 1959) and attempts to complete this research by extending it into the field of economic planning. In his opinion, contemporary society shows a close connection between planning, economic policy, and economic development. Planning appears as a congenial instrument of economic policy and economic development as the concrete and dominant object of economic planning.

The first part of the book deals with some fundamental problems of economic development and presents three principles of rationality to be used in the elaboration of an economic development plan. These principles are: (1) compatibility of objectives with available means, (2) interdependence and totality of the effects, and (3) solidarity of economic freedom and the other freedoms.

In the second part of the study the author applies these criteria to regional planning. In the case of the first one—compatibility of objectives with available means—he deals with such matters as: factors responsible for unbalanced growth in a dual-structure economy (having both advanced and backward areas); specification of the objectives of a regional development plan; general consequences in terms of industrial areas; and the analysis of benefits and costs.

In connection with the second criterion the discussion is focused on the use of input-output analysis, linear programming and methods of forecasting national aggregates (future regional income, prospective changes in demand schedules, and future capital requirements).

In the case of the third criterion—solidarity of economic freedom and the other freedoms—Parillo raises the issue: How is individual liberty to be reconciled with modern social requirements? Here the author proposes to determine the limits of governmental intervention beyond which democratic institutions are endangered or brought to a point where they would function only very imperfectly.

The principles suggested for the elaboration of an effective development plan are the same as those which the author used in the construction of his theory of economic policy. This is in line with Parillo's idea of inserting economic planning as an instrument into his theory of economic policy.

The third part of the book consists of an examination of the operative instruments of regional planning from the standpoint of: (1) flexibility and suitability for the attainment of goals, (2) ties of interdependence, and (3) the socio-economic costs of their use. Here Parillo becomes concerned with such problems as the policy with regard to public works directed at the creation of fixed social capital; the policy as to incentives; and direct intervention on the part of government to improve the human element.

The author stresses the need for incorporating the regional factor into economic science on a broader basis. "With the insertion of space into economic science," he assures the reader," an almost completely unexplored avenue is opened. This avenue promises to make the theory of development move ahead rapidly" (p. 49). This reviewer fully agrees with this view.

Strong emphasis is also placed on the need for a proper coordination of national and subnational regional planning. "The [national] plan arises from a need for coordination and must, in its formulation and execution, remain subject to this irrenounceable condition. It would be unhistorical and unrealistic, if the different regional or territorial plans would turn into isolated programs, not connected with each other, and, above all, not connected with the center and not incorporated into the framework of national planning and programming" (p. 12).

Activities in subnational regional planning have played an increasingly important role in Italy in the last three decades. They have had repercussions also on Italian economic analysis by stimulating the exploration of the role of the regional factor in the theory of economic development. The present study is a good example of this. The publication contains numerous references to recent writings on the topic in English, French, and Italian. The inclusion of a systematic list of references at the end would, no doubt, have added to its value for those interested in current Italian contributions to the evolution of a theory of economic development.

E. O. Kraemer

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La programmazione lineare per un modello di sviluppo regionale dell'agricoltura. By Giampiero Franco. Milano: Dott. A. Giuffre, 1962. Pp. viii, 132. Lire 1000.

Sviluppo economico e struttura del mercato finanziario. By Luigi Frey. Milano: Dott. A. Giuffre, 1961. Pp. 278. Lire 1800.

Cooperazione internazionale e sviluppo economico. By Alberto Bertolino. Firenze: La Nuova Italia, 1961. Pp. 198. Lire 2000.

The three small volumes under review almost illustrate a Parkinsonian expansion of activity within the analytical field of economic development. Franco first formulates an elementary version of the theory of linear programming and then applies a model to an agricultural region in Rovigo province. Confirming the usual elementary views of the international economist, namely, that too much of Italy's land is devoted to the production of cereals, Franco's careful analysis shows that a shift from present practices to those called for by his model would enable the region to effect a substantial increase in output with existing resources and production techniques. Since his optimal conditions would require treating the region as one vast "farm" under unified management, Franco seems quite uneasy about the existing situation in which there are farms of all sizes under numerous managements. In the light of his model, he seems to prefer that operations be under a tight, centrally planned system, though he admits that the theoretical optimum might also be attained autonomously in a market system in which, somehow, perfect competition and knowledge prevailed. Actually, apparently because Franco is not a revolutionary, he is prepared to settle for less: the use of his model to orient and "discipline" private farm activity. The compromise is also attractive to him in the light of the fact that in practice some allowance has to be made for uncertainty in connection with the technical production parameters, especially under dynamic conditions.

The reviewer, though not questioning Franco's logic or his competence as a craftsman, is concerned about the way he prematurely advocates an extreme "solution." The inherent dynamics of demand and other market factors are as important for analysis (and policy) as the model's main elements—the quantum of resources and known technical production processes. Would Franco have expressed himself about the centralist solution, at least in anything like the way he does on page 103, if he had first considered how attention to the dynamics would have "complicated" the analysis?

Frey's book, on economic development and the structure of financial markets, is mainly an attempt to trace the impact of technical progress on a closed economy's arrangements for financing economic development. The author candidly admits that technical progress is not easy to define—but then most of us are not averse to bandying about the "productivity" term without doing more than admitting that its conventional measurement in man-hour inputs isn't very satisfactory. In view of the wealth of relevant analytical pieces in the Anglo-Saxon financial literature, it is not surprising that the author devotes a good deal of space to reviewing non-Italian analyses of such questions as financial market structure, capital formation, the firm as a capital-using entity, the savings issue, and financial intermediaries. He reviews most of such matters effectively in the first nine chapters, and in doing so makes frequent reference to Italian contributions.

Because Italy has gone so far with public ownership of both industry and banking, the reviewer hoped that Frey would probe deeply in this area, which he covers in his tenth chapter. Instead, the treatment is limited pretty much to familiar propositions about the burden of public debt, the incidence of inflation, and uncertainty stemming from technical progress, coupled with a limited analysis of the "need" for public financing to assist weak firms, businesses or areas hurt by industrial concentration, and individuals disadvantaged by the impact of technical progress. The discussion on these points is exceedingly general, and almost "welfaristic" in the popular sense. One misses an awareness of relevant developments not covered in the overabundant Anglo-Saxon literature, such as Japan's success in achieving rapid growth partly by consciously hastening the demise of unproductive industries and expediting the transfer of workers from them to productive areas without elaborate retraining schemes in which the national government played a major role. One also misses an analysis of the ways, chiefly administrative, by which the same Italian government, by owning both the main banks and many leading industries, could effectively discriminate against the private sector, especially if the executive branch's make-up should include—as now seems probable—representatives of far-left parties.

Bertolino's volume on international cooperation and economic development, a collection of essays most of which have appeared over the last decade in semi-popular magazines, seems to be noted more for its kindly spirit than for the quality of its analysis. The book covers such diverse topics as international organizations, the United Nations (mainly in its earlier years), European to the policies pursued by the dominant "European" minority to promote its interests through development of the money economy.

To the dual economy of British Central Africa Barber applies a dual test of economic development, calling not merely for an increase in the per capita real income of the money economy (undoubtedly accomplished by the dynamism of the export-oriented money sector, particularly since the end of the Second World War) or even of the income of the economy as a whole, but also for a rise in the per capita real income of the indigenous population. The latter can only be accomplished by reducing the degree of dualism, and the study centers accordingly on the various pressures operating either to induce or impede the transference of economic activity from the indigenous to the money economy.

The study falls naturally into three parts, prefaced by a useful sketch of the historical background. The first part examines the economic behavior of the indigenous population. Here Barber considers in turn the response of the African as a supplier of agricultural produce to the money economy and as a supplier of wage labor. His conclusion on the former point is that when the opportunity has arisen to increase real income from the sale of agricultural surpluses the African has responded positively, with due allowance for the inevitable frictions and particularly for the restraints imposed by discriminatory policies of land distribution and a bias in public expenditure programs devoted until recently largely to assisting agricultural production in the (European) money sector.

Concerning the African's transfer from the indigenous economy to the role of wage laborer, Barber's most interesting contribution is to demonstrate that the position of the temporary migrant worker who shuttles between the indigenous and money economy, retaining a permanent stake in the former, is a rational one, representing an equilibrium position for the worker at the prevailing level of wages. By retaining a position in the indigenous agricultural sector he maximizes the real income of his family unit, from which, under prevailing agricultural practices, he can absent himself for a period without impairing output. That this arrangement reduces his productivity as a wage laborer and hence limits the possibility of increasing real wages is yet another vicious circle in the road to economic development.

The second part of the study is a rather conventional but useful review of the extraordinary expansion of the money economy of the Rhodesias since the Second World War. The impact of export expansion on European agricultural and industrial production for the domestic market is interestingly demonstrated, and here again the influence of dualism on the pattern of domestic production is carefully traced. With a certain satisfaction Barber notes that the pessimistic diagnosis that excessive concentration on export of primary products will inhibit industrial development has not been borne out during the postwar period, since the growth in aggregate effective demand has provided a base upon which manufacturing industries are now being built. He notes, incidentally, that the propensity to import has been higher than it would have been had there been a higher rate of transfer of labor from the indigenous sector with its simpler consumption requirements.

Perhaps the most significant part of the study is the third section in which Barber analyzes the pressures that arise in a dual economy of the Rhodesian type as the monetary sector expands and increases its effective demand for labor. In the background of this analysis are the various opposing forces operating in the labor market. The growth of the money economy has clearly created new employment opportunities for the indigenous population. But the most attractive opportunities in the money economy have been closed to Africans, and the limitations on expansion of African commercial agriculture continue to check the African's access to opportunities created by the growth in the money economy.

For the purpose of analyzing the interaction between the indigenous economy and the expanding money economy Barber elaborates the model developed by Arthur Lewis in which initially the supply of labor originating in the indigenous (subsistence) sector is assumed to be perfectly elastic at a relatively low real wage rate. Since the 1930's the pressure on the supply of indigenous labor in the Rhodesias has been mounting (but not in Nyasaland, where a classical Malthusian situation persists). The impact of this tendency on real wages in the Rhodesias was mitigated for a time by the encouragement of immigrant labor, including labor imported from Nyasaland. But the influx has not kept pace with the growing demand, and a condition of "quasi-full" employment has resulted, even though a large part of the population remains in the indigenous economy. Since a further withdrawal of indigenous labor into wage employment threatens the disruption of indigenous agricultural production, there has been a pressure towards rising real wages for African employees. This pressure has even induced some limited reforms in discriminatory labor practices exemplified by the 1955 agreement opening more skilled jobs to Africans in the Copperbelt.

Thus, the expansion of the money sector has begun at long last to draw the indigenous sector along with it at a rising real wage rate. Barber notes, however, that certain responses by the money sector to rising real wage rates (for example an increase of capital-intensity in industry and heavy investments in power and other public facilities serving the money sector) may slow up the process; furthermore, there is a continued threat of population pressure in Southern Rhodesia which is not being sufficiently met by policies for developing African agriculture. Even if the balance of forces favors an accelerated reduction of the barriers supporting dualism, Barber is too sophisticated to draw the conclusion that this hopeful tendency may make a major contribution to easing the rapidly developing political tension in the Rhodesias.

Because of the special institutional features in the Rhodesias much of the analysis in this study may lack general applicability. It is interesting, however, to speculate that the conflict of interest between the monetary and indigenous sectors that is revealed may, in fact, appear in developing economies even in the absence of a dominant nonindigenous minority that aggravates the problem.

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The Structure of the Australian Economy. By P. H. KARMEL AND MAUREEN BRUNT. Melbourne: F. W. Cheshire, 1962. Pp. xii, 154. 18s. 6d.

This book was originally prepared as part of a research project covering the economic systems of twelve British Commonwealth countries sponsored by the Commonwealth Studies Center of Duke University. Its purpose is "to compare the structure of the Australian economy with the competitive *laissez-faire* model" (p. vii). Published in Australia, it is presumably intended primarily for Australian readers; readers elsewhere, assisted by some of its elementary background material, should also find it useful, even if they have some difficulty in digesting the detailed statistical analyses of the degree of concentration of economic power and of market concentration in Australia.

Anyone who uses "structure" and "structural" so frequently as do Karmel and Miss Brunt might reasonably be expected to define these terms more carefully than they have done. Apparently they had not, before accepting *The Structure of the Australian Economy* as the title of their book, perused Machlup's wise words, published in 1958,[1] on "Structure and Structural Change: Weasel Words and Jargon." According to the interpretation which I happen to prefer, "structure" is a metaphorical term conveniently used in analyzing changes in the allocation within an economy of its productive resources. On this subject Karmel and Miss Brunt have a little, but not much, to say; nor do they explain at all clearly why and how structural changes in this sense occur in Australia. If their rather sharply drawn distinction between changes in response to consumers' preferences, said to be characteristic of "the competitive model," and changes "determined by the policies of giant firms," which occur "under non-competitive conditions" (p. 3), is to be taken seriously, we might expect to be told something about the relative quantitative importance of those two kinds of change as causes of the sharp differences between the recorded rates of change in the numbers employed in various Australian industries.

Setting aside the claims of semantic precision, the reader will find in *The Structure of the Australian Economy* a summary account of Australia's economic and institutional background, followed by a series of descriptive surveys, with some commentary, of some outstanding features of its economic life, including those which distinguish it most sharply from other economies; for example, the arbitration system and its effect upon the labor market, the place of the Commonwealth Bank and the banks associated with it in Australia's banking system, and the economic significance of its federal constitution. While the influence of arbitration upon Australian wages is described as being "virtually complete" (p. 37), there is also, it is said, "no convincing statistical evidence of any radical change" in labor's share in the national income over the past 50 years (p. 44). Inflation, which tends to be endemic in Australia, is, however, thought to have been mainly cost-induced, and "the main causes of increases in costs have been increases in wages" (p. 133), prescribed, or permitted, by the Commonwealth Conciliation and Arbitration Commission. Australia's federal constitution is such that "the nationalization of major industries has been made virtually impossible" (p. 19). "The economic powers of the Commonwealth," moreover, are said to be "insufficient for the pursuit of economic policy in the modern world." So far, however, efforts to persuade the majority of the electors of the validity of this thesis have on the whole been unsuccessful, and the checks thus imposed upon the growth of the Australian economy are not precisely indicated.

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[1] *Zeitschrift fur Nationalökonomie*, 1958, *18*, 280-98.

The usefulness of a book of this kind to some extent depends upon its brevity. Brevity however may mean that some interesting and important questions remain unanswered. "Restrictive practices have come to be regarded by businessmen and consumers alike as normal business behaviour" in Australia (p. 94). "Monopolistic and tightly oligopolistic markets are overwhelmingly more important than in the United States" (p. 87). There is "singularly little interest in the efficiency of resource allocation in the Australian economy" (p. 142). "The pioneering spirit in Australia has been associated not with 'individualism' and 'rivalry' but with 'mateship' and 'fair shares' " (p. 136). Weighty evidence can be adduced to support all these theses, but when we note that in recent years real gross national product has increased at the respectable annual rate of 4½ per cent, we suspect that the story is incomplete. Some readers would be happy to forego some of the detail engendered by the natural ambition to keep up with the statistical Joneses—the end-product of which must in the nature of things be rather inconclusive—if the space thus saved could have been devoted to a more penetrating analysis of the work of the Tariff Board, now one of Australia's most significant economic policy instruments.

Allan G. B. Fisher

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La concentración del poder económico—su teoría: realidad chilena. By Ricardo Lagos-Escobar. Santiago de Chile: Editorial Del Pacífico, S.A., 1961. Pp. 186.

Economic power in Chile is concentrated in high officials of three commercial banks, interlocking directors, and a few of the governmental elite, most of whom are brothers-in-law, siblings, or otherwise related to each other. This is the gist of this book by a young alumnus of the Law School of the University of Chile and the Department of Economics of Duke University.

The first part of the pocket-sized book discusses a few aspects of a few contemporary theories on the centralization of economic power in the private sector and its effects chiefly in North Atlantic countries. Here are to be found banner-line statements about monopolies and a talent for synthesizing.

On "Economic Groups in Chile," the core of the work is the second and last part. The motif is that interlocking directorships have invested the Sud-Americano, Chile, and Edwards banks with predominant control or influence over nearly three-fourths of all corporate stock in Chile. Achieved in part by the links of the three superbanks with eight smaller ones, this is a long-trend endowment of power, as evidenced by statistics from the Central Bank, Bureau of Corporations, and other governmental units.

An amalgam of 22.4 per cent of firms, in which the troika of banks can retard or speed up development, has control over 70.6 per cent of all corporate capital. The axle of this power wheel is made up of 4.2 per cent of firms having command over 59.2 per cent of all business capital. These are secondary *raisons d'être* for the study, originally an academic thesis. It ignores the impact on the economy of enterprises such as airlines and merchant shipping, which are not incorporated in Chile.

Lagos-Escobar's research indicates that bank officials are kinsfolk to numerous corporation directors. Directors of more than 100 corporations are in Congress. Apparently not associated with any corporation when he took office, a former president of Chile was in 1958 head of a bank and director of six other large firms; a foreign minister afterwards became a director of twelve corporations. Whether annuity provisions for a foreign minister are more opulent than those for a president is, nonetheless, not made clear.

A graph shows the Bank of Chile is affiliated through directorships, stock ownership, and other devices with ten other principal banks, Sud-Americano with nine, and Edwards with seven. Two other banks have five such affiliations.

This power nucleus is responsible, the study says, for 9 per cent of the population of Chile getting 43 per cent of the national income, for the public media defending the *status quo,* and for governmental authorities having no incentive to encourage economic growth and a more equitable distribution of aggregate income. In a proposal to recast the Chilean economy, Lagos-Escobar says (p. 172) that:

> The only and true solution is, therefore, the abolition of private property over the means of production, which should pass to the state. In the environment of this aforementioned property, all laws that are enacted can only be palliatives which will never succeed in definitely eliminating the diverse forms of concentration.

Initially, the government would acquire gradual control over all financial capital. Nothing is said about the speed of this gradualness. As Lagos-Escobar thinks the elite will resist until death any profound changes, he must be hopeful its progeny will be different.

The Fable of the Shark and the Sardines by Arévalo has helped to mold the concepts of Lagos-Escobar. He quotes it often but never makes clear the shark is the United States. Other chief sources for the book's theoretical analysis include a work by the Socialist Party of Uruguay, one by Varga, an article by Victor Perlo, and Lenin's *Imperialism.* Scattered among the thirty titles in the bibliography are translations of works by Samuelson, Schumpeter, Arthur W. Lewis, and E. A. G. Robinson.

Lagos-Escobar says the second part of his work, on Chilean realities, details of which seem to be largely a contribution to knowledge, is a pioneer survey. As an economist and lawyer, he is almost as impersonal as a corporation. In my book are ink interlineations on six pages to correct major errors by type being omitted or mixed up. But Lagos-Escobar and the composing room get high marks on the surname, often repeated, of one economist: Marx.

BYRON WHITE

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Europe's Future in Figures. Edited by R. C. GEARY. Amsterdam: North-Holland Publishing Company, 1962. Pp. viii, 343. $9.00.

The present volume is the first publication of the European Scientific Association for Medium and Long-Term Forecasting (ASEPELT), which was founded in March 1961 for the purpose of organizing and promoting "original scientific studies, either on methods of medium and long-term economic forecasting and programming, or on the preparation of specific forecasts." The book contains eleven separate contributions (eight in English and three in French, with English summaries) dealing with long-range forecasts of GNP or similar macro-aggregates. Eight chapters present specific forecasts for 1970 or 1975 for individual countries—Belgium, West Germany, France, Italy, the Netherlands, Switzerland, and the United Kingdom (two chapters)—followed by two papers of a more general theoretical nature and a lengthy concluding comment by the editor.

A work of this sort obviously cannot be evaluated as a whole. Indeed, the different levels of sophistication of the contributors, the various approaches taken in preparing the projections, and—last, but not least—the different degrees of comprehensiveness of available national-account statistics all make it inevitable that the result is a series of quite disparate papers. The best a reviewer can do under the circumstances is to indicate the scope of the individual contributions.

The Belgian National Accounts Study Group forecasts the rate of growth of GNP by projecting the value added in what it regards as exogenous sectors—agriculture, housing, government, and domestic services—and applying a Cobb-Dougles function to the other "endogenous" sectors. Rolf Krengel (Germany) bases his forecasts (which are confined to industrial production) on fixed capital, the supply of energy, and employment—in that order—and thus puts considerable emphasis on estimating changes in fixed capital at constant prices. Jean Benard (France) rejects a model of simultaneous equations and prefers a method of trial and error, his so-called "iterative scheme," which includes partial models and a number of exogenous-variable estimates. Vera Cao-Pinna (Italy) regards three sectors—agriculture, public administration, and services—as "nonsystematic" or exogenous and focuses therefore on industrial and tertiary activities. A remarkable table (p. 115), giving constant price series for value added, employment, and fixed capital for these two sectors during 1922-39 and 1950-58, is used to compute derived data and six functional relationships between the three series for each of the two periods covered.

J. Sandee (Netherlands) prefers simple input-output estimates, while Francesco Kneschaurek (Switzerland), after noting that "Switzerland is still to be considered a statistically underdeveloped area," employs what he calls a "very crude model" in which GNP is simply the product of the labor force and productivity. B. M. Deakin (United Kingdom) uses trade cycle theory, which leads him to the "not very specific conclusion" that the British economy may be in the latter stage of the expansionary long-term half-cycle and that a decline into the negative half-cycle is unlikely to occur before 1970. His projection is based largely on forecasts of gross productivity and of civilian employment. A similar method is adopted by C. T. Saunders (United Kingdom), who makes it clear at the outset that he is less concerned with the assumed rate of growth than with the pattern of the economy, changes in which "would mostly be more marked if growth were faster."

In the first of the two theoretical papers, Ragnar Frisch contributes a comprehensive and lucid survey of types of economic forecasting and programming, in which he distinguishes between onlooker and decision models—the former producing straight forecasts of what is likely to happen and the latter designed to influence the course of events. Finally, Richard Stone and J. A. C. Brown submit an elegant econometric model of the British economy which, aiming "to investigate the possibility of a faster rate of growth," assumes various rates of growth of total consumption and then works out the composition of these totals for 1970.

Thus, rather than discussing "Europe's future," the volume basically is a loose collection of individual country forecasts that permit few, if any, broad comparisons and certainly no aggregation. Even if "the overriding consideration was that a volume of reasonable dimensions should be published with the least delay," this lack of integration, comparability, and over-all focus is somewhat disappointing and tends to weaken the value of the work as a whole. These features are all the more striking if one recalls the Twentieth Century Fund's recent *Europe's Needs and Resources* (New York, 1961), which offered comprehensive and integrated projections to 1970 of GNP and major sectors and industries for 20 European countries. Apart from being of interest to the student of forecasting techniques, the ASEPELT publication therefore would seem to appeal primarily to country specialists for whom the various papers provide a wealth of historical data that often are unavailable elsewhere.

JOHN HEIN

Federal Reserve Bank of New York

Succès et faiblesses de l'effort social français. Edited by PIERRE LAROQUE. Paris: A. Colin, 1961. Pp. 368. NF20.

This collection of essays has the rare merit of attempting systematic discussion of up-to-date facts concerning social change in France. It mercifully avoids sociological category-building in favor of current information and essential background. It has the defects of its objective: practically no analytical depth, and a tendency toward insufficiently supported assertion of the interpretations the particular authors favor. For economists, the book has an additional defect in that the authors' concerns are primarily sociological. Most of the topics concerned are within the boundaries of economics, but questions of opportunity costs, incentives and resource allocation receive strictly secondary attention.

The three sections of the book are concerned respectively with problems of "the individual and the family," methods of "action on the structures of French society," and techniques of administration and financing. In case the first section sounds much too far from economics, it should be noted that the essays here include such questions as social security, education, housing, and urbanism.

The study of recent changes in the French educational system, by Michel Morisot, is one of the two best essays in the book. It both straightens out facts in a most confusing field and goes beyond them to analyze (too briefly but well) the consequences of the educational system for economic and social mobility and for political position. The author is at pains to emphasize the inadequacy of the changes made so far in the postwar period, with good reason. But the study conveys such a strong picture of experiment, argument, and willingness to alter old methods, that an American reader may well become wistful. The vitality of a society must be high indeed to keep pressing for needed changes in this style, treating each compromise step forward as a point from which new departures are necessary rather than as a sufficient excuse for acceptance of continuing inequity.

The second of the two particularly interesting essays is that on agriculture, by Gilbert Guillaume. Again, much of the space is taken by helpful factual description, but the quality of the essay resides in the way in which the author utilizes an intelligent theme. The theme is that the cherished "stability" and "balance" of French agriculture are euphemisms for inability to adjust to economic opportunity. The author blames this inability largely on protectionist devices, both against imports of competing agricultural products and against domestic market forces. He may well be considered to err on the side of lack of sympathy for the problems of people now in agriculture, particularly in the care with which he brings out the degree to which they are subsidized by the rest of the society. But the general note of interest in market forces as a means of promoting desirable change makes one wonder if a new wind is not beginning to stir in a country where the leading economists have long exhibited sophisticated disdain of such thoughts.

One of the essays dealing with economic issues might be accused of distortion to support a familiar point of view. This is the study of "the workers" as a social class, by Michel Gentot. It does report a good deal of improvement in conditions for the workers, but somehow it all adds up to a situation of worse exploitation than ever. Apparently this is indeed the view of a good many of the workers themselves. A survey reported in January 1957 asked them whether life in their circles was "in a general manner, more difficult or easier than, or about the same as," it had been before 1939. The reply of 36 per cent was that things are worse now. Only 22 per cent thought that there has been an improvement. The author clearly sides with the 36 per cent, emphasizing at one point (p. 250) the growing sense of insecurity among the least favored workers because of the strain of installment debts incurred in buying household electrical equipment, automobiles and television sets.

Students of the French economy will probably wish that the sociological studies had included some attention to changes in attitudes of and toward French businessmen, to economic repercussions as distinct from financial costs of extensive family aid and social security, and perhaps above all to the possible connections between greatly improved personal security and greatly increased economic flexibility during the postwar period.

JOHN SHEAHAN

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Land and Labour in India. By DANIEL AND ALICE THORNER. New York: Asia Publishing House, 1962. Pp. vii, 227. $8.50.

The story told by Sir Josiah Stamp of how a judge reprimanded a pleader for quoting Indian statistics in court, "... the governments collect them, add them, raise them to the *n*th power, take the cube root and prepare wonderful diagrams. But what you must never forget is that every one of those figures comes in the first instance from the *chaukidar,* the village watchman, who just puts down what he damn pleases ...," acquires new freshness and new meaning in this collection of essays by the Thorners.

The large and systematic effort of the government of India to move the country's economy in some planned fashion calls for the development of specific data on which planning could be based. The 1951 Population Census, a Census of Landholding, the All-India Rural Credit Survey, and the Agricultural Labour Inquiry are part of this development. In Indian statistics reliance on the *chaukidar* has stopped almost entirely; field interviewers have taken over. They do not fill their schedules with what they please; instead they follow instructions laid down at the center and tabulate oral responses to a limited number of "simple" questions. The new procedures do not do away with the old problem; the innocence, the ignorance, and the social pressures that controlled the hand of the village watchman are now re-embodied in the enumerators' questions and in the respondent's answers.

At the heart of the matter there are intertwined two very old problems. The first is the inapplicability of general definitions to the diverse and fluid conditions of the subcontinent. The inapplicability of definitions is compounded by the fact that they are derived—as Bagehot, much earlier, and D. R. Godgil, more recently, repeatedly noted—from the structures and postulates of Western political economy. The second difficulty is the desire to obtain data justifying (as in the case of the rural credit survey) a priori, albeit often well-intentioned, policy positions.

The glowing reports of a more egalitarian structure of landholding and of improvement in the positions of agricultural labor which followed the publication of the census are thus unjustified; the censuses permit only more limited and more tentative inferences. The censuses understate the number of *rentiers* and inflate the number of cultivating owners. The more favorable statistics are due to census redefinitions, "category-climbing," and the introduction of mock-owners and mock-cultivators. The favorable changes were made possible also by instructions to census enumerators that did not place sufficient weight upon inquiries as to who actually did the work, and that equated direction of an agricultural venture with actual cultivation.

Economic progress, a kind of democratic industrial revolution, is indeed going on in India. But it is limited to manufacturing and to industrial centers.

Nothing of the kind is occurring on the land. Here the weak and the strong are mostly the same as before independence, be it in terms of land ownership, in the distribution of irrigation water, credit, or labor. At the center of the impasse is the failure of land reform, the frustration of the central government's announced aim of bringing the cultivator into a direct relationship with the state. Indeed in spite of the land-reform legislation of many states, in spite of the passage in 1950 of the much publicized U. P. Zamindari Abolition Act, land reform has not succeeded and the "agrarian problem in India today remains basic, serious, and deeply rooted" (p. 5).

This is the thesis and the unifying theme of five articles on the methods of the censuses and of three more general papers published by the Thorners in various Indian economic journals between 1955 and 1960 and reproduced in this collection. The thesis is a serious one and the findings are justified. Indeed as so much of its economy is based on the land, land reform is the prerequisite of India's future development. The Thorners' work on the schedules, the samples, the evaluations, is thorough and judicious. The polemical edge and the informed sympathetic approach derived from the authors' almost 10 years in India enrich the argument. There is only one point—but a crucial one—at which this reviewer boggles. Attention is drawn to it here because of the generality of the problem and its presence in all underdeveloped areas.

Land reform—more by implication than by direct statement—is here, as elsewhere, equated to reform in the ownership of land. This equation is unwarranted. Indeed were the structure of ownership in land—and only the structure of ownership—changed overnight one could anticipate no solution therefrom to India's agrarian problems. On the contrary the only reasonable forecast would be a drop in agricultural output. This objection is old hat and could be disposed of by the Thorners and others, with an offhand "of course." The rebuttal would note that changes in land ownership are nevertheless a necessary prerequisite for modern, functional, rationally planned land reform. But the rebuttal is invalid. India's own experience in the nationalized sector, as well as a priori analysis, indicate that what matters indeed is not so much who works the land—or the factory—as how it is worked, and there is wisdom in focusing on the direction of the agricultural venture. The modern notion of land reform which includes concern with production methods and yields makes land reform part of a broader design whose chief objectives are to lay the basis for economic development and to create the social and political conditions which are its prerequisite. Whether the social, political and technical prerequisites are or are not being built up in India is so far unclear. As modern land reform is often viewed as fast-paced one may be tempted to say that the social and political preconditions necessary for land reform are not being developed; the emphasis on basic literacy, universal suffrage, and family planning in India's policy, on the other hand, suggests that they are. Whatever the pace of development, the Thorners do not make a case against India's recognition of the importance of such prerequisites.

Three articles on long-run trends in product and output review available data and previous analysis to conclude that between 1880 and 1947 the Indian economy was stagnating in terms of agricultural output and that its per capita income was declining.

The presentation throughout is detailed and thorough. It reflects the joining of careful statistical surveys with the testing of the implications of basic propositions from economic analysis. No serious student of Indian economic history can afford to miss this work. No person concerned with economic development and with its planning and policy corollaries should miss noting the advantages to policy of the approach exemplified in this work.

OSCAR ORNATI

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Capital Formation in Japan, 1868-1940. By HENRY ROSOVSKY. New York: The Free Press of Glencoe, 1961. Pp. xii, 358.

Professor Rosovsky's two-year stay in Japan (1956 to 1958) as a guest of the Institute of Economic Research of Hitotsubashi University was used to very good advantage. His scholarly study of capital formation in Japan is a very useful addition, in English, to the literature on Japan's economic development.

As a disciple of Kuznets and Gerschenkron, Rosovsky's effort was to produce a work in quantitative economic history consistent with the standards and objectives outlined by Kuznets:

> Quantitative economic history is being used in an attempt to provide an empirical foundation to economic generalizations. It is thus one form of integration of economic theory and history, even though concentration on quantitative measurement limits the type and variety of historical experience that can be used for the purpose.

The major portion of Rosovsky's research effort involved estimating long-term capital formation in Japan, by components, from 1849 to 1940. His estimates are based on "by-type"' rather than commodity flow concepts.

Of the two related parts of the book, Part I, Analysis, contains an interpretation and discussion of Japanese capital formation. Chapter 1 summarizes the findings on domestic and national capital formation. Chapter 2 is devoted to capital formation in the public sector; Chapter 3, to capital formation in the private sector; while Chapter 4 is a rather speculative essay which tries to contrast economic development in Europe and in Japan.

Part II, Measurement, two-thirds of the book, explains in detail how the series on capital formation were derived and their relation to previous estimates. After a chapter on "Introduction to Measurement," and one on "Previous Attempts at Long-Term Measurement," Rosovsky treats, in detail, government construction, government investment in durable equipment, residential construction, private nonresidential construction, and private investment in producers' durables.

The author is disarmingly modest about both his objectives and his achievements. First, he notes that his measurements are not "definitive." He feels he has succeeded in producing only a "first approximation." The data for long-term measurement of economic growth are almost always imperfect and Rosovsky found that there existed no adequate historical quantitative series for investment in most sectors of agriculture or for inventories. He therefore had to make some very rough assumptions and estimates for these major omissions. Secondly, he notes that his study concentrates almost exclusively on one aspect of capital formation, yearly capital accumulation in terms of construction and investment in durable equipment. It largely ignores both the sources of investment funds and the important entrepreneurial function which translates the available funds into real capital. Finally, the general focus of the study (except for Chapter 4) is extremely narrow. As the author notes, capital formation is only one aspect or symptom of industrialization. There are of course other aspects of equal or greater importance.

With respect to capital formation, he finds that in Japan from 1887 to 1940 gross domestic capital formation (GDCF) averaged 16.1 per cent of gross national product (GNP). Within this average there were four periods of distinctly different investment activity. The first extended from 1887 until about 1907-16, and was characterized by a rather steady investment ratio of about 12 per cent. This was followed by the remarkable second period, lasting until approximately 1922, the intensive advance associated with the First World War, during which the investment ratio rose rapidly to about 18.5 per cent. The third period included the troubled 1920's and the Great Depression. It was characterized by a slightly sagging investment ratio or a plateau—at a considerably higher level, however, than that prevailing during the first period. Finally, the 1930's saw another advance which carried the ratio to its all-time high at the end of the decade, during the course of which it averaged 25 per cent. It was then the fortuitous (for Japan) circumstances of the First World War and the military adventures and expansion (ultimately disastrous) of the 1930's that gave Japan its highest levels of capital formation, hardly a desirable pattern for other developing countries to emulate.

The most imaginative and interesting section of the book (to this reviewer, at least) is the fourth chapter, entitled "Japanese Economic Development and the Western Model," wherein Rosovsky attempts to compare Japanese economic development with the Gershenkron model explaining the process of European economic development. Since the international record of total product growth rates is richer than the record of capital formation, using either total or per capita concept, Rosovsky concludes that, by world standards, Japanese growth was extremely rapid. Japanese industrialization, as expressed in the vigor of its capital formation, is, he concludes, a twentieth-century phenomenon.

Rosovsky has contributed significantly to the developing body of knowledge on the economic growth of Japan.

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The Economic Development of Uganda. Report of a Mission Organized by the International Bank for Reconstruction and Development. Baltimore: The Johns Hopkins Press, 1962. Pp. xviii, 475. $8.50.

Roads and Road Transport in an Underdeveloped Economy: A Case Study of Uganda. By E. K. HAWKINS. London: Her Majesty's Stationery Office (American Agent: British Information Services, 45 Rockefeller Plaza, New York, 20), 1962. Pp. 263. $8.40.

These two books have originated as officially-sponsored advisory studies on economic policy in Uganda. Though their central concern is with the issues now being confronted by a government which acquired its independence in October, 1962, they also have a wider interest as contributions to the growing literature of applied economics on the problems of underdeveloped economies.

The Report of the International Bank Mission, prepared by a team led by Edward S. Mason, is intended as a comprehensive set of recommendations for a development program extending through the fiscal year 1965-66. Two basic considerations underlie its approach to development planning in Uganda. In the first place, the immediate prospects for industrial expansion are far from bright, not only because of deterrents common to other parts of the underdeveloped world, but also because of Uganda's participation in the East African Common Market (an arrangement which the Mission assumes will be continued at least during the initial planning period). Secondly, it is maintained that important opportunities are open for raising productivity in agriculture, though, as the Report warns, "the return to investment in agriculture for the most part is bound to be low since knowledge of the best techniques to use in the difficult tropical environment is still imperfect, the existing institutions, such as land tenure systems, inhibit effort, and productivity depends on the willingness and capacity of hundreds of thousands of peasants still living in a traditional society to adopt new methods of work." This assessment governs the selection of priorities discussed in the main body of the report.

The "plan" proposed is first presented in the form of a program for public expenditure. The Mission strongly recommends high priorities for agricultural and other productive services, while development expenditures on most other items (notably on social services) are to be curtailed in relative, though not in absolute, terms. This recommendation runs sharply counter to trends in the allocation of public expenditures in Uganda during the 1950's. When the costs of the Mission's program are measured against the financing expected to be available locally, a substantial short-fall emerges. The Mission anticipates that the gap (which amounts to roughly half of the total budgeted development expenditure) can be filled by external loans and grants if the new government demonstrates a determination to carry through the program.

Of the discussions of the programs for individual sectors, the most detailed is that devoted to agriculture. In the Mission's view, readjustment in agricultural exports is an urgent task. Two commodities (coffee and cotton) account for roughly 80 per cent of foreign exchange earnings and are the prime generators of money income. Outputs of coffee expanded rapidly during the 1950's while cotton production showed little change; export markets in the 1960's, however, are likely to be more favorable for the latter than for the former. The policies of the statutory marketing boards are brought under fire in this connection, but not for the reasons advanced by some recent critics. The Report offers no support to those who would dismantle controls or would insist that the boards distribute all sales proceeds to producers. Instead, it proposes the adoption of a forward pricing policy to encourage expansion in cotton growing and recommends that reserves be accumulated to finance productive improvements in agriculture, rather than to dampen fluctuations in producers' incomes. But adjustments in relative prices are not regarded as necessarily sufficient to produce shifts in export crop specialization. One of several imaginative proposals advanced in the Report is that the government accord preferential treatment in the provision of primary educational facilities to districts co-operating in expanding cotton production.

Far more difficult is the long-term task of promoting general agricultural reform, particularly among peasants whose contact with the money economy is peripheral to their activities as subsistence producers. The Report's observations on these problems largely cover familiar ground. Changes in the indigenous institutional structure are regarded as desirable, but no radical transformations are proposed. Measures to encourage gradual change are offered, with emphasis on the extension of "out-grower" schemes (in effect, an agricultural putting-out system through which unfamiliar cash crops are introduced, under careful supervision, into African cropping patterns) and on the award of individual land tenure rights to progressive farmers. A sweeping conversion of traditional land tenure arrangements into individual ownership is discouraged for the immediate future on the ground that the costs are likely to outweigh the expected gains.

Less attention is given to other sectors of the economy and the views expressed are generally conventional. A warm welcome for private foreign investment is recommended, as well as special tax inducements to foreign enterprise. In addition, the new government is advised to assign a much higher priority to geological surveying and exploration than did its predecessors.

Even if the Mission's recommendations are minutely followed and its expectations about future trends—particularly in markets for agricultural commodities—do not prove to be overly optimistic, no dramatic economic improvement is promised to an independent Uganda. The Report states the prospect as follows: "With reasonable luck, the rate of growth should accelerate and gross domestic product should increase on the average by 3-4 per cent a year, or at a rate higher than the rate of increase in population" (p. 39). The basis of its calculation of aggregate growth is not, however, entirely free from ambiguity; elsewhere, the same rate of growth is expressed, not in terms of gross domestic product, but in terms of "domestic product going into the market sector" (p. 62).

In the reviewer's judgment, the main recommendations of the Report are inspired by a pragmatic appraisal of economic realities in Uganda. Reshaping the agricultural structure is a slow and frustration-inviting job. In some respects, development proposals calling for a rapid build-up of manufacturing industry can be made to sound more exciting and analyses which interpret the development process as one of manipulating the values of one or two strategic variables may even make more engaging reading. But in African conditions, there can be no shirking the hard tasks of remoulding institutions and attitudes if lasting foundations for economic progress are to be laid. As the Report stresses, there is unlikely to be any short cut.

Hawkins' study is primarily concerned with problems of road transport, though part of his analysis is directed to the relationships between road and rail. The first part of the study presents an impressive mass of data, much of which has been collected by the author in field surveys made in 1957-1958, on the structure of the road transport industry and on the composition of traffic. This material, much of which is here published for the first time, is later used to good effect in the discussion of policy. One of his major conclusions is that governmental policy toward the commercial use of the road system has been inappropriately influenced by imitation of the patterns of licensing and regulation practised in the United Kingdom. The effect, he argues, has been to encourage excessive specialization in commercial road services, to suppress fuller utilization of existing vehicle capacity, and to sacrifice the flexibility of road services desirable in Uganda's economic circumstances. In addition, he notes that the established pattern of controls has also tended to restrict entry of African entrepreneurs into the industry, as well as to impose an excessive drain on the territory's scarce supplies of administrative skills.

On the question of road-rail competition, Hawkins finds no case for special restrictions to defend rail freight traffic from possible encroachments by road carriers, and he maintains further that "defense of the railway" arguments should carry no weight in decisions on extensions to the road network. The long-run relationship between road and railway is viewed as complementary rather than competitive, with the rail system (which specializes in long-hauls, particularly in the movement of export crops) standing to gain from any expansions in economic activity stimulated by development of the road network.

Hawkins' conclusions on these aspects of transport policy are well argued. On another point fuller treatment would have been useful. One chapter is devoted to an exercise in cost-benefit analysis, and is offered as a guide to investment choices on alternative road improvement projects. The author emphasizes the weakness of the data available for such calculations and notes particularly that the selection of a rate of interest involves an element of arbitrary judgment. This bit of analysis raises a further question which the author does not ask—namely, to what extent is the application of an essentially static marginalist calculus legitimate in a nonhomogeneous society in which dynamic supramarginal changes are required?

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Essays on the Soviet Economy. By NAUM JASNY. New York: Praeger, 1962. Pp. xiv, 297. $6.00.

The four essays in this volume deal with the problems of establishing the real growth, associated burdens, and *modus operandi* of the Soviet economy on the basis of the incomplete and often misleading evidence which emerges from the Soviet Union. The first essay, "Research on the Soviet Economy," investigates the reliability of Soviet statistics. Jasny shares the view of most Western specialists that official physical production statistics are usually reliable, but that aggregative indices in value terms are generally not acceptable by Western standards of statistical practice. This position is supported by an extensive survey of Soviet statistical transgressions and by application of the consistency tests which the author has developed.

The chief value of this essay lies in its wealth of information concerning the interpretation of a wide variety of Soviet economic statistics. The author also has succeeded in his more general goal of establishing the need for a skeptical approach to official Soviet data. On the other hand, he has tended to overstate his case in a number of ways. First, by underplaying the significance of genuine methodological problems, particularly in regard to index number construction. Second, much space is devoted to justifying the terms of opprobrium which the author applies to particular products of the Soviet Statistical Office. Finally, a similarly unproductive endeavor is the argument that certain other Western scholars hold very different positions from his concerning the reliability of Soviet data. After checking the sources, one cannot help feeling that in a number of cases the brief quotations offered by Jasny do not fully characterize the viewpoints to which he refers.

In the second essay, "Peasant Incomes Under Full-Scale Collectivization," Jasny returns to a subject to which he has devoted much study: the changes in the per capita real income of the peasantry, in money and in kind, since 1928. Much as this new index merits careful appraisal, only a few remarks can be offered here. Most significant is the ambitiousness of the undertaking in view of the limited amount of data (and research facilities) available to the author. Even for the most exhaustively analyzed years in the series, many components had to be approximated from scattered and incomplete information. As a result, the estimates of income in kind measured in current rubles and the very important price indices with which deflation to base-year farm prices is performed are, at best, risky ventures.

Here, however, Jasny's ingenuity and famous "feel" come into play in fitting the available data into a pattern which is consistent both internally and with the impressions gathered over 20 years of study. The results are not entirely reproducible and will therefore be difficult for other scholars to build upon, but they do have Jasny's batting average behind them. Finally, regardless of the precision of the results, the student who has the stamina to follow the derivation of these estimates will learn much about Soviet agricultural institutions and statistics.

In his recent volume, *Soviet Industrialization, 1928-1952,* Jasny demonstrated that the successive 5-year plan periods do not represent a meaningful subdivision of the industrialization era. In "Soviet 'Perspective' Planning," the major essay in the present collection, a rationale for this finding is sought in an evaluation of the operational significance of the plans themselves. Contrary to official propaganda, it is argued, the Soviet economy has never been guided by an interrelated set of comprehensive plans covering periods of one, 5, and 10 to 15 years. The "perspective" (i.e., 5-year and 10- to 15-year) plans were, at least until the current 7-year plan, primarily propaganda documents which bore little resemblance to or influence upon reality. Actual guidance of the economy was limited to the annual plans and to sectoral development programs in which interrelationships with other branches of the economy were inadequately reflected.

The essay is built around a history of the "general" (i.e., 10 to 15-year) plan in the USSR and of the individual 5-year plans. The former describes the early failures and the rapid degeneration of general planning into absurd projections of explosive growth; the abandonment of such long-range planning in the 1930's; and its postwar revival in the form of a limited number of targets of dubious import. Having concluded (with Jasny) that comprehensive planning so far into the future is impossible, the Soviets have long since lowered their sights while maintaining the official fiction concerning the role of the "genplan."

With imaginative exploitation of the primary sources, the operational significance of the 5-year plans is appraised with reference to a number of indicators: completeness, internal consistency, punctuality of approval (relative to the official beginning of the plan period), relationship to contemporaneous annual plans, and, most importantly, the "realism" or *ex ante* attainability of the planned targets. There is also much interesting and relevant information on the political and economic setting, planning organization, and primary literature of the various plan periods. On the strength of this analysis, the comprehensive, operational, 5-year plan is also consigned to the realm of Soviet mythology.

The firmness of this conclusion notwithstanding, the argument is not without its rough edges. A ubiquitous problem is the intermingling of an evaluation of the quality of Soviet planning (primarily in terms of its rationality) with the attempt to determine its operational extent. As a result, Jasny often seems to be arguing that the 5-year plans were both meaningless propaganda and unsound economics. If the reader wonders what significance, if any, to attach to the latter finding, the reason is that too little attention has been given to those parts of the plans which *did* reflect operational decisions, whether sound or unsound.

Jasny's application of the realism criterion also raises a number of questions. First, noting that the discrepancies between targets and results under the postwar plans were often smaller than before the war, the author refers to the "relative moderation" of the fourth 5-year plan (1946-50) and, from the context, would apparently describe the two succeeding plans in the same way. What is not clear is why this closer fit of goals and achievements should not be viewed as "increased realism." The point is of more than semantic interest since the latter approach would imply that the recent reform of Soviet planning was less abrupt than Jasny maintains and that its origins might lie in an earlier period.

Second, in applying this indicator to aggregative goals in value terms, targets measured by Soviet methods are compared with results estimated by Western standards. For present purposes the comparison should be entirely in the terms of the target. Finally, more recognition could have been given to factors other than propaganda as causes of the disparity between targets and results. But these, to be sure, are minor qualifications to a very useful study.

The brief, concluding essay deals severely with a series of claims concerning real wages which were made in the official plan fulfillment report for 1948. The title, "The Summit of Falsehood," refers to the assertion that real wages in the state sector of the economy doubled during the single year 1948.

In sum, with an unusual combination of scholarship and polemics, these essays offer an empirical appraisal of several aspects of the Soviet experience. If his view is generally negative, this does not prevent the author from calling attention to recent improvements in the areas which he explores. The collection can be recommended to those who are studying the Soviet economy with the uncertain aid of official information. Nonspecialists may find the essay on planning to be of greatest interest.

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Prices and Production of Machinery in the Soviet Union 1928-1958. By Richard Moorsteen. Cambridge: Harvard University Press, 1962. Pp. 498. $15.00.

The major objective of this study is the computation of a Soviet machinery price index, to be used for deflating the current-ruble investment data developed in the RAND national income and product studies. The index represents an important methodological advance over earlier computations, such as Jasny's which deflated current-ruble investment figures by a base-year-weighted price index under the illusion that the deflated investment index was also base-year (1926-27) weighted. Moorsteen's index employs variable weights of the given year ($\Sigma p_1 q_1/\Sigma p_0 q_1$) which, when divided into a current-ruble investment figure ($\Sigma p_1 q_1$), yields a true base-year-weighted investment index ($\Sigma p_0 q_1$). The significance of the new index is that it corrects the grave understatement of the growth of Soviet investment due to the earlier method.

The mass of data compiled in pursuit of the main objective is further exploited by the author in the computation of a set of fixed-weight indexes of machinery prices and production. As a contribution to the "index-number problem," the results show that indexes based on 1937, 1941 (Plan) and 1955 weights move very closely together, but (for both prices and production) all three differ widely from the 1927-28-based index. The author concludes that the growth pattern which yields a strong Gerschenkron effect (inverse correlation between relative prices and relative output) had virtually ended in the USSR by the mid-'thirties, in the same way as it had been confined to a certain period of time in the United States. The implied hypothesis is that the magnitude of the Gerschenkron effect in any economy would vary directly with an index of the rate of structural change, an index which could probably be constructed with the abundant raw data presented in the 322 appendix pages.

The pre-1949 official Soviet production indexes have been criticized for their overlong reliance on 1926-27 price weights, a criticism sustained by the results noted above. But a second basis of criticism must now be reconsidered in the light of Moorsteen's findings. That criticism is that new products were priced-in at the current prices of their first year of production, yielding an upward bias because of (a) inflation and (b) relatively high costs during the first production year. Moorsteen's 1937-weighted machinery price index, however, which is dominated by new products, actually falls from 141 in 1927-28 to 100 in 1937. Hence the official method of pricing-in new products would lead to a downward bias, though the first-year-of-production principle still tends in the opposite direction. Thus the cautions of writers like Dobb against overstressing the new-product problem appear, in the light of Moorsteen's findings, to have been well-founded, a point made in Bergson's latest work. In any case the simple Gerschenkron effect is so massive in the 1928-37 period as to account for the overwhelming proportion of the bias in the official indexes.

With the abandonment of 1926-27 price-weights in 1949, the principal objection to the official indexes has been removed. Moorsteen describes the evidence on the new official index as inconclusive. His own 1955 index of machinery production at 1937 prices is 148 (1950 = 100), while the official index at 1952 prices is 232. Although his index is believed to understate the rate of growth because of incomplete adjustment for quality improvement, a variety of indications suggest that the official index is too high. Among them is the fact that the deflated official index of investment in machinery is much closer to Moorsteen's than to the official machinery production index. The latter must still be regarded with qualified caution.

The 1955-weighted Moorsteen production index for the period 1928-37 (1937 = 550) is remarkably close to the Gerschenkron-Erlich index which was weighted by 1939 United States prices (1937 = 525). This similarity supports the other evidence that the history of Soviet machinery price movements has roughly paralleled that of the United States, with the 1955 Soviet price structure quite similar to that of the United States in 1939. Hodgman's 1934-weighted index, on the other hand, grows much more slowly (1937 = 625) than Moorsteen's 1934-weighted index (1937 = 1385) for the same period, 1928-37. Evidence is adduced that the estimating make-shifts forced on Hodgman by deficiencies in the data available at the time account for much of the understatement. The Nutter index, which unfortunately appeared too late for Moorsteen to take into account, also grows more slowly. With both at 1955 weights and 1928 = 100, Moorsteen's rises to 550 in 1937 and 1795 in 1955, while Nutter's rises to 436 in 1937 and 974 in 1955. However, the two indexes are relatively close for the 1945-55 period. A detailed comparison of the two indexes would be a major undertaking, but among the differences that may prove significant are Moorsteen's consistent use of price weights and imputation, compared to Nutter's occasional reliance on employment weights and preference for direct weights rather than imputed.

Moorsteen's work is based on what is without doubt the most thorough dragnet of Soviet raw data on machinery thus far published. Yet it is inevitably incomplete and therefore subject to the ancient questions about representativeness. The study is confined to civilian machinery, and the items for which data were found amount to 55-70 per cent of total Soviet investment in machinery. The author employs a battery of painstaking indirect tests of representativeness of the data and legitimacy of estimating procedures, none of which is decisive, but which in aggregate justify a high level of confidence. Moorsteen's work is an important addition to the growing list of monographic studies of the Soviet economy.

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Sebestoimost' produktsii v tiazheloi promyshlennosti SSSR. (Production Costs in Soviet Heavy Industry). By R. S. LIVSHITS. Moscow: Academy of Sciences of the USSR, 1961. Pp. 291. 1 ruble.

This informative monograph, whose author is already known to students of the Soviet economy thanks to her studies of the spatial distribution of Russian industry before and after the Revolution, was published under the auspices of the Institute of Economics of the USSR Academy of Sciences. It sets itself the task of inquiring into the structure, composition, and dispersion of production costs in the coal mining, iron-ore mining, iron and steel, and machine-building industries. Each instance is examined in the light of differences in technology, size of plant, vertical integration, and, where applicable, natural conditions. The data are generally as of 1958 or 1959. In line with the usual Soviet approach, the author does not limit herself to a positive inquiry into costs but also engages in a normative search for possibilities ("reserves") for their reduction.

The book brings out forcefully the fact that, except for some auxiliary processes, Soviet industrial cost accounts are broken down by major input categories (materials, fuel, wages, depreciation, etc.) rather than by elements of the production process. The latter would of course afford management better control over the production process as a whole. But, as Robert W. Campbell justly pointed out some years ago, the nature and peculiarities of Soviet cost accounting derive from the nature of the economic system, that is to say, from the logic of detailed central planning and management. The central authorities' chief concerns are appraisal of the managers' performance, especially in regard to economizing inputs, and the allocation of materials and wage funds. To these ends Soviet cost accounting would seem to be quite well suited. But, should greater autonomy ever be granted to the Soviet enterprise, a thorough overhaul of its cost accounting practices would be in order.[1]

Quite striking, even if not surprising to students of Soviet industry, are the data on the dispersion of labor productivities and unit costs in individual industries. For example, as among major basins, labor productivity in ore mining in 1958 varied in the ratio of up to 11:1 (p. 29). Distinguishing between underground and open-cut mining, in 1959 cost per ton of iron ore varied, among whole basins, in the ratios of 2:1 and 8:1, respectively (p. 61). As between individual mines in a single "ore-mining administration" in the Krivoi Rog basin, labor productivity and unit cost varied in the ratio 4:1 (p. 25-26). Given such dispersion in ore cost and similar dispersion in coal cost, it is hardly surprising that unit cost of pig iron even among the larger mills varied in the ratio of up to 2.6:1 (p. 217). The reasons for this extreme dispersion are complex, and the author examines them at length, but the fact that such differences exist and persist is of course in good measure attributable to the planners' emphasis on sheer physical output and de-emphasis of economic calculation.

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[1] This point is made again and strongly in Campbell's *Accounting in Soviet Planning and Management,* Cambridge, Mass. 1963, which was published since this review was written.

On the question of the size of plant the author follows the usual Soviet view which believes in economies of scale almost indefinitely and with few qualifications. In this connection she adduces a good deal of detailed data that generally support this position, though with some exceptions.

The normative sections of the book bring in some interesting comparisons with foreign performance, particularly that of the United States. There is also much of what is in reality advice of an engineering nature; one necessarily wonders how sound it is. This, however, is a failing common to much Soviet writing on questions of "concrete economics," and is probably explainable with reference to the technocratic penchant of the society, the weakness of economic analytical tools, and the pressure to be normative on every subject.

Speaking of economic analysis, the book under review, while comparing not unfavorably with other Soviet monographic studies, is still quite weak on this score. An example will suffice. The author repeatedly complains of the unduly low price of scrap in the USSR, its inadequate collection, and its low rate of use by the iron and steel industry. (There is a uniform national price of scrap, which is less than half the average price for conversion pig iron.) The case is interesting if only because the problem of pricing scrap, where both demand and supply curves must be quite elastic, puts to a severe test the Soviet principles of average cost pricing and of uniform nation-wide price on a given commodity. But, curiously, the author does not rest her case in favor of a higher scrap price on supply conditions and the demand derivable from its substitutability for pig iron. Instead, she argues that a large part of scrap is a by-product of steel production, and hence the low price of scrap leads to an exaggerated price of its joint product, steel! I shall not take the space to unravel this one.

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Statistical Methods; Econometrics; Social Accounting

Seasonal Adjustment on Electronic Computers (Proceedings of an international conference, held in Paris in November, 1960). Paris: Organization for Economic Cooperation and Development, 1961. Pp. 403. $9.50.

Under the auspices of OEEC and of the Organization of European Statisticians, a conference was called in November 1960 to discuss problems of seasonal adjustment of economic time series. The various papers presented to this conference and a summary of the general discussions constitute the volume here under consideration. The volume is a compendium of the problems and experiences which constitute the staple of a seasonal adjuster's diet. As such, it is at times somewhat tedious to read, but it provides a faithful picture of the present state of the art of seasonal adjustment. In the past decade, techniques of seasonal adjustment have become more refined, the ability of these techniques to accommodate special problems has been increased, and the availability of seasonal adjustment for a great number of series at moderate cost and at short notice has been greatly enhanced. All of these developments are fully reflected and discussed in this volume with an eye towards further improvements in the future. On the negative side, however, the volume conveys a persistent sense of perplexity and ambiguity with regard to the basic conceptual notions which ought to underlie the operation of seasonal adjustment. This sense of vagueness-in-the-background is present in almost all of the papers which comprise the volume. Admittedly, the conference was called to discuss the methods, and not the theory, of seasonal adjustment, and thus should not be criticized for theoretical deficiency. Unfortunately, however, theoretical deficiency seems, in some instances, to be a source of empirical vagueness. For example, when attempting to examine the "goodness" of a given method of seasonal adjustment, one must have in mind a well-defined notion of what constitutes "good" seasonal adjustment.

The sense of ambiguity begins early, namely, with the definition of seasonality. In the first place, most of the discussions in the volume seem to entertain the notion that every economic series, in some true statistical sense, has a unique break-down into components, of which one is the seasonal component. The job of the seasonal adjuster is then viewed as being the recognition and estimation of this "true" seasonal component. Actually, the process of seasonal adjustment consists of estimating an arbitrary characteristic of a series, and it is quite conceivable that for two different purposes we may wish to "seasonally adjust" the same series in two different ways. In the second place, when it comes to describing what, in fact, this "seasonal component" is, one would presumably expect a statement to the effect that this component is a periodic function of time, with a period equal to 12 (for a monthly series). However, the reader is cautioned not to identify seasonality with periodicity, since periodicity is too narrowly defined. While this statement is never fully explored, the result is that seasonality remains altogether undefined. Only S. N. Marris, in the first of three articles which he contributed to the volume, actually defines what seasonality is, but his definition cannot be taken as a guide to the perplexed. It may be well to mention at this point that under fairly general assumptions concerning the nature of the time series, seasonality can be defined unequivocally, and its extent quantitatively asserted, by means of the so-called power spectrum associated with the series.

The ambiguity with regard to the very definition of seasonality manifests itself in discussions of criteria for "good" seasonal adjustment. J. Shiskin, whose contributions in the area of modern seasonal adjustment are well known, in a comprehensive article which he has contributed to the volume states the following conclusion with respect to how good Census Method II has proven to be: "There can no longer be any doubt that Method II adjusts a very large proportion of series very well." There is no question but that the author of this statement would not make it unless he had more than ample evidence to rely on, and indeed this evidence is summarized in the article itself. Still, when it comes to stating what "adjusts very well" means, Shiskin offers four criteria which he himself calls "vague" and "by no means definitive." Other authors, notably Marris, assess the goodness of a given seasonal adjustment method by comparing its results to a free-hand curve, or by applying it to an isolated preconstructed example for which the seasonal "component" is known in advance. All of these authors no doubt have a very good intuitive feeling for the quality of the seasonal adjustment in any given instance. However, when it comes to writing an account for general professional circulation, more definite statements are necessary if the reader is to gain the guidance which he is seeking. Once again, it may be mentioned here that the quality of seasonal adjustment can, in many cases, be accurately assessed by means of spectral analysis. While spectral analysis has not yet been useful in the search for a "best" seasonal adjustment, it has been very useful in the evaluation of existing methods of seasonal adjustment. With respect to U.S. unemployment data, this has been done recently in a (yet unpublished) study by Marc Nerlove. He examines the seasonal adjustment of the various unemployment series by first estimating the linear filter which best approximates the adjustment filter actually used (a variant of Method II) and then evaluating the frequency-response function of that filter. As a result, he finds that in most of the series seasonal adjustment has induced artificial leads and lags of varying severity, and that in some series the adjustment caused a great deal of distortion in the so-called nonseasonal components.

Finally, it should be stressed that the criticisms voiced here necessarily do an injustice to some of the papers which comprise our volume. As an example one may cite a paper by J. Bongard, "Some Remarks on Moving Averages," to which none of the above criticisms apply. Bongard's article provides an excellent discussion of the smoothing characteristics of moving averages and points out the important fact that moving averages are capable of inducing periodicity just as they are capable of removing it. The author then uses what is essentially quadratic programming to derive a "best" moving average according to a criterion which he formulates. The article is very instructive and worthy of attention.

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Seminario de las Naciones Unidas sobre cuentas nacionales para América Latina, Río de Janeiro, Brasil, June 11-26, 1959. New York: United Nations, 1960. Pp. 608 mimeo.

This work contains the papers submitted to the conference that was organized by statisticians of the United Nations and the Inter-American Institute of Statistics and held in Rio de Janeiro, June 1959, on the problems and methodology of the national accounts of Latin America. Of the 31 papers, not counting the introductory summary, two are on different aspects of the national income of Ecuador and 15 are straightforward surveys of the basic concepts and methods of preparation of the national income accounts of other Latin American countries and Puerto Rico. Nine papers are anonymous studies by United Nations statisticians and five are by individual experts (Abraham, Crocco, Dorrance, Geary, and Ruggles) on financial flows, input-output analysis, and one or another aspect of national income. The study is entirely in Spanish, except for one chapter in Portuguese, and it is a valuable volume of reference and analysis.

The general impression is one of slow but real progress toward improvement of the national economic accounting of most Latin American countries. The best sign of this progress is the open recognition sometimes made of the great deficiencies still present in the estimates, especially in the vital fields of farm output, private consumption, handicraft production, personal savings, and capital formation. The bad habit is manifested, however, of confining this recognition to the text, while tables are prepared that in appearance have all the precision and completeness that one could hope for; it is as if the progress of quantitative economics depended upon the quantity, not the quality, of data. To the all-too-human tendency to arrive prematurely at final results there is added the pretentiousness that is not rare in scientific work.

A useful feature of this volume is that some of the chapters on the individual countries reveal something of what goes into the making of the figures on national accounts. Adequate revelation is still far off, however, so that although the present-day estimates may meet the casual criteria of national and international bureaucracy, they still cannot be used with confidence by the careful investigator who rightly insists in retracing, step by step, decision by decision, calculation by calculation, the whole process of estimation. It is strange that the United Nations Economic Commission for Latin America notices this lack of disclosure (p. 411) without criticizing it as an obstacle to statistical progress. For the fact that few private research groups or individuals in Latin America make independent moves to improve the data is no doubt due, at least in part, to the inaccessibility of the basic materials. Inadequate disclosure also offers temptation for the political manipulation of data.

One short chapter gives an account of the training program and the technical assistance offered by international, inter-American and United States government institutions. It all makes up a combined effort of great weight, especially if measured by the hundreds of young men and women who have passed through training programs on national income accounting or related branches of statistics. It would be interesting to know, however, what happens to these trainees, especially to discover what "leakage" there is from statistics to greener pastures, for this loss may possibly be an important element holding back statistical improvement. Nevertheless, as many papers in the volume point out, the chief impediment to adequate national accounting is the lack of basic data. One therefore learns with regret of the intention of some statistical authorities to enter the fashionable fields of flow-of-funds and input-output accounting—an unfortunate diversion of effort if it is made at the expense of building up a better and richer corpus of partial series on income and output. It is apparently easier to express (p. 13) than to follow the view that income accounts (and by extension any manufactured system of complex data that is built-up piecemeal) are only as good as the series that enter their manufacture. There is only some classroom value to a systematic presentation of accounts when its component parts fall short of acceptability even by easygoing statistical criteria.

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Economic Systems; Planning and Reform; Cooperation

Protiv burzhuaznykh ekonomicheskikh psevdoteorii sotsializma—Kriticheskii ocherk. (Against Bourgeois Economic Pseudo-Theories of Socialism—A Critical Essay). By S. A. KHAVINA. Moscow: Ekonomizdat, 1962. Pp. 195. .71 rubles.

In the great debate on the "economics of socialism" of the 'thirties and 'forties one voice was conspicuously silent, the voice of Soviet economists. Those who were speaking for what was then the only major socialist economy in the world, and were claiming a higher order of rationality for it in comparison with capitalism, would not or could not reply to von Mises's and Hayek's strictures. Nor, on the socialist side of the debate, did they even throw the weight of their authority and experience on the side of the "centralists" and against the proponents of a "competitive solution." While occasionally through the years Soviet economists have taken public notice of the Western literature on the economics of socialism, the first Soviet book on the subject, the one here under review, has only just appeared. Needless to say, this belated response solidly upholds the centralist socialist position, *à la russe,* against both the competitive-solutionists and the Mises-Hayek school. The irony—or the historical logic?—is that Soviet economists are joining the issue just as the centralist form of socialist economic management is on the defensive throughout the communist world, and, at least in its more extreme and rigid aspects, is being ever more vocally challenged in the USSR itself.

Khavina's book is much more than a critical essay of Western economic theories of socialism. It is a critique of virtually *every* shade of economic opinion on the subject of socialism and planning—bourgeois, socialist, and communist—that is not in tune with the currently orthodox Soviet doctrine. A brief recitation of the book's contents will illustrate the breadth of its concern. The first chapter purports to be a summary and critique of Western theorists, ranging in time from Pareto and Barone to C. A. R. Crosland and W. W. Rostow, with surprisingly great attention to Schumpeter. These are divided into "bourgeois" and "petit bourgeois" authors. The second chapter continues the attack with special reference to three problems: the concept of public ownership of means of production, the class structure of the socialist society (particularly of what we call the communist countries), and income distribution under socialism. The third chapter deals with "anti-Marxist" views on planning: a criticism of both the very notion of planning under capitalism and the Western analysis of Soviet planning. There are also a foreword by the eminent Soviet economist, Ia. A. Kronrod, and an epilogue by the author. Unfortunately for a work of this sort, but as is usually the case with Soviet books, there is no index.

The general tenor of Khavina's theoretical critique of the Western economics of socialism has been well summarized by Maurice Dobb in a recent review of an essay of hers on the same subject: "The writer is well informed and well-read . . . ; but her treatment is disappointingly superficial and lacking in light and shade . . . [The Western writers'] points of contrast are not explored, and the general tendency is to treat them as a 'homogeneous reactionary mass'—as writers who uniformly try, in obscuratist fashion, to oppose an idealist subjectism notion of socialism to real socialism as actualized in Soviet economy" (this *Review*, Sept. 1962, *52*, 864). Specifically, Khavina's theoretical strictures (pp. 22ff.) rest on the grounds that Western economic theory of socialism: (1) rejects inquiry into production relations among men, fetishizes economic relations, and makes scarcity the chief concern of economic science; (2) ignores the historical relativity of economic laws; (3) treats economic categories and laws in a subjectivist-idealist and voluntarist fashion, denying their objective existence; (4) detaches distribution from production; (5) stresses consumer sovereignty to the detriment of "the primacy of production"; and (6) prefers formal, functional relations to causal inquiry, and quantitative analysis to qualitative analysis. Thus, the issue is joined on the level of basic epistemological premises, and so much for the Western economics of socialism.

But the book is at least as much a weapon of political counterattack as an essay in theoretical criticism. As already mentioned, the author's guns are leveled against all those who do not accept the Soviet model, dare to admit the possibility of planning in other systems, or deviate from orthodox Soviet economic doctrine. The Hayek-Mises school is antisocialist, but their opponents in the famous debate are not much better—indeed, they are more dangerous, for their anti-Marxism is camouflaged by socialist appearances (p. 9). The author's epilogue, clearly an afterthought, concentrates its fire on contemporary West European (democratic) socialists, who, with their reformism and acceptance of a multiplicity of socialist forms, undermine the proletarian cause. "The duty of Marxist-Leninist scholars is to arm the working class theoretically for the struggle against the baneful theories of pseudo-socialism" (p. 194). Elsewhere (p. 16), Yugoslav "revisionism," with its workers' management and a socialist market economy, is singled out for attack. But it is Kronrod's foreword that brings the argument much closer home and more up to date. Kronrod welcomes the book for its attempt to fill a gap in the Soviet economic literature, to set the record straight with regard to "various reformist and revisionist perversions of the true essence of socialist economics," and to counteract the influence that these perversions may have on "economists poorly prepared in Marxism." The last he links with the recent admission of mathematics into Soviet economics: its correct and successful use is possible only on the basis of Marxist-Leninist political economy. Moreover, Khavina's critique, he asserts, hits directly at those who would bring into Soviet economics notions of marginalism, "conventional valuations" *(uslovnye* *otsenki)*, etc., under the guise of the scientific application of mathematics. The last is of course a thinly veiled reference to Novozhilov, Kantorovich (with his "objectively determined valuations"), and the other mathematically-oriented innovators in Soviet economics that are sometimes referred to collectively in the West as "rationalists." Kronrod has been one of their most vocal opponents. They are, it would seem, cut of the same political cloth and tarred with the same ideological brush as the competitive-solutionists, reformists, revisionists, and other enemies of true socialism.

This review cannot conclude without recording a notable feat performed by the book: the writing of an analysis and history of the Western economics of socialism without giving the least indication that Oskar Lange had anything to do with it. True, he is mentioned—exactly once (p. 9), where a reference to his *On the Economic Theory of Socialism* (with F. M. Taylor, 1938) is made—but apparently only *pro forma* so as not to omit entirely his name and that of his key essay. He is quoted with regard to Hayek's and Robbins's amendment of von Mises's stand (p. 62 of Lange's essay); not as a positive contributor to the literature, let alone a leading one. Apparently, it will not do to lump the vice-president of a fraternal socialist country with the enemies of the USSR and the pseudosocialists, even if he has since openly revised his views. Time was when Very Important Economists were turned into Nonpersons; now a Very Important Person is turned into a Noneconomist. And thus "economic science" marches on.

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Business Fluctuations

The Panic of 1819: Reactions and Policies. By MURRAY N. ROTHBARD. New York: Columbia University Press, 1962. Pp. vi, 261. $6.00.

The National Bureau of Economic Research starts its calendar-year reference dates of business cycles in the United States with the trough of 1834 and its monthly reference dates with the trough of December 1854.[1] But Murray N. Rothbard presents considerable evidence that the student of business cycles may have much to gain if he focuses some of his attention on "America's first great economic crisis and depression" (p. v), for *The Panic of 1819* was indeed "marked with strong hints of modern depressions" (p. v).

In Chapter 1, written as background, Rothbard discusses very briefly economic conditions during the War of 1812 and, somewhat more extensively, economic conditions in the postwar period through 1821. Though the United States was then merely on the threshold of industrialization, with close to three-fourths of its labor force still engaged in agriculture, parallels with the period of the First World War and the subsequent depression of the 1930's immediately become apparent: the rapid economic expansion during the war, the financing of the war via extensive borrowing, the inflation that continued into the postwar period, and the eventual deflation accompanied by inability of debtors to meet their obligations, by widespread bank failures, business bankruptcies, and unemployment. Contrary to the experience in 20th century crises, manufacturing was in a depression in the period following the War of 1812 while the rest of the economy was still booming. But even this state of affairs which Rothbard, quite correctly, refers to as "one of the most vital points of difference between the economy of that period and of the modern day" (p. 18), had its counterpart in the years following the First World War, though in the "Golden 'Twenties" it was primarily the agricultural sector of the economy which found itself unable to readjust to peacetime conditions.

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[1] Arthur F. Burns and Wesley C. Mitchell, *Measuring Business Cycles*, New York 1946, p. 78.

The bulk of the book is devoted to a very detailed account of contemplated, proposed, scrutinized, rationalized, tested, instituted, rejected, legislated, and repealed remedies for the panic. State by state, territory by territory (and where necessary county by county and city by city), the reader is given the opportunity to listen to the arguments and debates in newspaper editorials, in letters to the editor, in monographs, in petitions, in gubernatorial messages, in proposed bills, and in speeches before legislative assemblies. He is exposed to the pros and cons of such controversial topics as direct relief to debtors, expansions of the money supply, and protection of domestic producers by means of protective tariffs, as seen through the eyes of farmers, merchants, manufacturers, bankers, lawyers, legislators, and other "leading citizens" of the era. Wherever proposals were acted upon on the local, state, or national levels the reader learns about their relative success or failure. The student of business cycles in the 1960's may smile at what now appears as the gross naïveté of some proposals and policies, but he can hardly help admiring the level of economic sophistication and understanding reflected in many others.

Two main theses seem to predominate throughout the book: first, that many of the ideas, proposed remedies, and instituted policies were precursors of similar ideas, remedies and policies of later eras and secondly, that apart from the tariff question the controversy cut across regional, geographic, class, and income boundaries. It is this reviewer's opinion that the author was not quite as successful in proving the latter as the former thesis.

As to the first point, Rothbard is certainly correct that among the propositions advanced nearly a century and a half ago, there can be found many familiar arguments and counterarguments: on the desirability of protecting domestic industry against foreign competition, on the responsibilities of debtors and the rights of creditors, on the steps necessary to restore public confidence, on the advisability of various monetary and fiscal policies, on the necessity for stricter regulation of private banking, on the proper function of government as a whole during periods of economic crises, etc. And the proposals and policies of the era included "a rudimentary forerunner of open-market operations" (p. 117); "pump-priming" proposals (e.g., pp. 75, 118, 133); public works projects (pp. 118, 186); an "early version of a specie exchange standard" (p. 114); a 100 per cent reserve plan (pp. 140, 142-43); demands by "embryonic Veblenians" that the wealthy set an example to the poorer in thrifty living (p. 186); a provision for double liability of bank stockholders (p. 155); and many other previews of coming events.

As to Rothbard's second main thesis, that the controversies showed no stratification along geographical, occupational, or income lines, the author offers substantial but not conclusive evidence. Numerous examples of divided opinions within states, counties, and economic classes on such topics as stay laws (moratoria) and easy money policies present convincing evidence that man is not exclusively an economic man or, at any rate, not a creature endowed with perfect knowledge and understanding. But, on the other hand, the western states passed considerably more debtors' relief legislation than the eastern states (pp. 40, 56); the "most spectacular expansionist measures," i.e., the establishment of state-owned banks for the purpose of issuing nonconvertible currency, took place in several western states (p. 110); almost all the states that passed stay and minimum appraisal laws were western states;[3] and the relief bill for public land debtors was voted down by a slim majority in New England but passed without a dissenting vote in Ohio, Illinois, Indiana, Kentucky, Louisiana, and Alabama (p. 30). These facts certainly seem to indicate that there is some correlation between geographic-economic interests and the stand taken by those who searched for remedies for the economic crisis of 1819.

The Panic of 1819 is primarily descriptive in nature and therefore can do no more than provide spadework for a more intensive theoretical analysis of pre-1834 business cycles. However, the book is thoroughly documented, and more than fifty pages of reference notes and bibliography bear witness to the author's extensive research. To the best knowledge of this reviewer, Rothbard's work represents the only published, book-length, academic treatise on the remedies that were proposed, debated, and enacted in attempts to cope with the crisis of 1819. As such the book should certainly find a place on the shelf of the student of U.S. business cycles and of the economic historian who is interested in the early economic development of the United States.

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[3] On p. 184 the author mentions, in conclusion, that eight states, two of them in the East, enacted stay laws, but on p. 56 he lists seven western and two eastern states. Again, on page 184 the author lists Indiana, Missouri, Ohio, and Tennessee as states which enacted minimum appraisal laws, while on p. 56 he fails to list Tennessee but lists Pennsylvania and Kentucky instead.

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Some Aspects of Industrial Finance in India. By GEORGE ROSEN. New York: Free Press of Glencoe for Center for International Studies, Massachusetts Institute of Technology, 1962. Pp. xiv, 144. $6.50.

This is a very lucid and competent analytic study of the role of banks, special financial institutions, and life insurance companies in the financing of industries in India, particularly since the beginning of the planning era (1950-51). In the first three chapters, Rosen briefly discusses the origin of the Indian banks, their particular approach to the problem of industrial finance, and their current lending policies with regard to industry, and presents an analysis of bank advances. The relationship between bank finance and the size of the firm is analyzed in Chapter 4, while the distribution of bank credit among industries is discussed in Chapter 5. The next two chapters deal with the role of the Life Insurance Corporation and the special financial institutions in providing industrial finance. There follows a chapter on the structure of interest rates in India. Finally, the credit requirements of the private industrial sector during the third plan are estimated and the policy changes needed to meet them are indicated.

The general conclusion that emerges from this study is that the attitudes and policies of the banks towards industrial finance are changing under the stress of the new situation created by planned development, and they are not only meeting the working-capital requirements of organized industry quite satisfactorily but are also trying progressively to increase their term-loans to industry; though the special financial institutions were set up only a few years back, they are already having a significant effect upon the Indian capital market.

Rosen's specific conclusion (p. 57) that there is no positive relationship between an industry's use of bank credit and its gross profitability and/or rate of growth does emerge from the data presented but is not very meaningful. This conclusion should not be taken to suggest, as is implied by the author, that there is no positive relationship between a *firm's* use of bank credit and its gross profitability and/or rate of growth. Such a positive relationship may not exist for an industry, composed of good, bad and indifferent firms, but it may exist with regard to firms making use of bank credit. In fact, in analyzing banks' policies, the author should have tried to examine this relationship at the firm rather than at the industry level.

His other specific conclusion (p. 49), viz., the relative insignificance of bank finance as a source of funds for firms below a size of Rs.500,000, is based on inadequate data, and that too for only one year, 1955-56. In fact, recent data for the years 1957-59 indicate that the proportion of bank finance to all sources of funds was 24.5 per cent for public limited companies below the size of Rs.500,000, while it was 9-10 per cent for the large companies. Similarly, his conclusion (p. 43) that small firms rely relatively more on external sources of finance than the large ones is not borne out by more recent data; in fact, during 1957-59, the proportion of external finance to all sources of funds was a little higher for the large companies than for the small ones.

In the final chapter, Rosen attempts a projection of the credit requirements of the private industrial sector and the probable supply of credit during the third-plan period on certain assumptions. However, he obtains a rather large gap in short-term finance largely because of an error in his computation. He assumes a 7 per cent per year increase in money supply (defined to include currency plus all deposits) and further assumes that the ratio of currency to deposits would remain constant during the third-plan period. On this basis, he should get a yearly rate of increase in deposits of 7 per cent; however, he computes this rate, through mistake, at 3.5 per cent (p. 124). Further, on the basis of past experience, he should have assumed a rising proportion of deposits to currency.

He seems to believe that because of scarcity of capital, interest rates in India should be higher than they actually are (p. 92). However, his own calculations show that for large companies, the over-all cost of credit is about 5.5 per cent as against an average rate of return on capital of 7.5 per cent. This difference in the cost of credit and the rate of return hardly justifies higher interest rates. Further, for a large number of small industrial firms in agriculture and other noncorporate sectors, the cost of credit is much too high (p. 137). There are sound reasons, in fact, for lowering the cost of credit for these sectors.

As the author rightly points out, the banks as well as the specialized financial institutions are rather cautious and unenterprising in their lending policies, with the result that they tend to judge the credit-worthiness of a party on the basis of its standing and the personal and physical security that it can offer rather than on the basis of the economic and financial soundness of the project it seeks to undertake. For this reason there seems to be real difficulty in new entrepreneurs without past experience in industry obtaining required credit even for projects which are technically and economically quite sound.

To enable the financial institution to judge the credit-worthiness of borrowers on the basis of the technical and economic soundness of their projects, he suggests the setting up of a Central Credit Rating Bureau jointly by the banks and the financial institutions. This suggestion has been also made by others like P. D. Ojha and the reviewer. Such an institution would help the financial institutions in judging the credit-worthiness of new entreprenuers and make it possible for them to jointly underwrite as well as participate in the financing of new projects. A system of credit guarantees, such as the one already adopted by the government of India for small firms, is also suggested.

What is required, according to the author, is a reorientation in the attitudes and policies of the financial institutions so that they can play a much more active role in the industrialization and development of the Indian economy. To this end, he has made several sound suggestions, which merit close study. The book is, indeed, a valuable addition to the literature on financial institutions in the developing economies.

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Group Banking: A Form of Banking Concentration and Control in the United States. By W. Ralph Lamb. New Brunswick: Rutgers University Press, 1961. Pp. xiv, 334. $7.50.

This study of group banking turns out to be a warm endorsement of its subject. The author concludes that further banking concentration is inevitable. Among its various forms he finds the alternatives impractical or inadequate, while he concludes that group banking is both practical and adequate.

The study starts with a leisurely survey of the various forms of banking concentration. Considerable attention is focused on changes in our banking structure during the course of this survey. Group banking history is then reviewed in considerable detail. The policy conclusions the author reaches are based essentially on a belief in the continuity of historical trends.

Lamb's study provides an excellent brief history of group banking. His judgments with respect to various economic influences, and the regulatory actions of public authorities, seem to be accurate and to reflect considerable insight. The one serious flaw in the policy conclusions is that they lean on a quite unwarranted assumption: that the rule of self-denial under which the federal government has not permitted national banks any greater branching authority than that permitted state banks is inviolable. The case for group banking assumes implicitly that state laws with respect to branch banking will be changed only slowly and that branch banking across state lines will never be permitted.

This may be the most realistic political probability. It is notable, however, that the Commission for Money and Credit recommended a breach of this rule as did the more recent "Saxon" report.[1] The economic forces making for further concentration of banking continue to operate; the cost structure of banking is creating strong pressures for such concentration. If lower levels of interest rates should ever again prevail, this pressure would increase. Economists certainly should not dismiss solutions or resolutions of such pressures on the basis of a judgment of political inexpediency. Look at what has happened to silver!

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[1] *National Banks and the Future* (a report of the Advisory Committee on Banking to the Comptroller of the Currency). Washington 1962.

Conjuncture et monnaie. By GÉRARD-COLIN DE VERDIÈRE. Paris: Librairie du Recueil Sirey, 1961. Pp. iv, 127. 12 NF.

In this modest monograph, M. de Verdière sets himself the task of discovering the relationship between the "monetary mass" (demand deposits and currency held by the public) and the gross national product. He wishes not only to test the validity of certain theories of the value of money, but also to uncover guides to monetary policy.

He starts by computing coefficients of correlation between the two variables, using annual figures from 1929 to 1958 for France, Great Britain, Germany, and the United States. All the coefficients are positive and high, ranging from .80 for Great Britain to .98 for France; they are still higher when the GNP of one year is correlated with the money in circulation of the previous year. These high coefficients are undoubtedly due in part to the fact that no effort is made to deflate either series. The ratio of money to GNP varies from one-third to one-sixth, tending to be lower in periods of increasing economic activity. The author concludes that increasing the quantity of money does not necessarily increase economic activity; on the contrary, the degree of economic activity seems to determine the amount of money in circulation.

How this is accomplished in France is shown in a chapter which analyzes the demand for money from business establishments, from households, and from government agencies. The supply of money and credit provided by savings, profits, bank credit and public subsidy is presented in a series of tables, but without any elaborate statistical analysis. One which is of particular interest, in view of the traditional French emphasis on thrift, shows an 18-fold increase in consumer credit between 1948 and 1958.

The second part of the monograph studies the effect on the French economy of the increasing funds available for production from 1949 to 1958. This was a period of full employment, and the increase in money in circulation was *"encaisse supplémentaire"* which might have been used to increase consumption and raise prices. He shows however that both business enterprises and households used the increase to rebuild their balances rather than to buy goods and services. In other words, liquidity preference increased sufficiently to absorb the new money, which was thus neutralized and prevented from raising prices.

The total of idle balances was very highly correlated with new investment (+ .93) when investment includes time deposits and net receipts of insurance companies as well as purchases of government and business securities. The author points out that the high ratio of self-financing in recent years, a kind of forced saving, has brought an element of rigidity into the investment sector which makes it less sensitive than formerly. The rate of interest seems to have little influence, except in the case of the building industry where cost of mortgage credit is an important part of the total cost.

A final chapter deals briefly with the United States from 1929 to 1937, and with France from 1940 to 1948. He concludes that in both cases the quantity theory of money fails to explain either price levels or levels of economic activity, and that it is only in underdeveloped areas that this theory is still valid.

Much of the argument rests upon the author's definition of inactive balances (which he borrows from Pierre Dieterlen) as the difference between the lowest amount of money in circulation at any point of the period in question, and the total amount at each date of the same period. This definition assumes that the lowest amount must be the necessary and therefore the active balance, and that any surplus above the active balance is inactive. It is to be hoped that the rapid improvement in French financial statistics which is now going on will soon provide a better measure of active and inactive balances than this rather crude device—a measure more worthy of the author's talent for careful analysis and theoretical conclusions. There is an extensive bibliography of books and articles in French and in English. The diagrams of the correlations and their regression lines are interesting and illuminating except in a few cases in which the labels are not clear.

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International Economics

Concentration in International Trade. By MICHAEL MICHAELY. Contributions to Economic Analysis No. 28. Amsterdam: North-Holland Publishing Company, 1962. Pp. 167. $5.00.

The perennial fascination of economists with the pattern of world trade has resulted in a number of useful studies. Evidence abounds on, say, the share of a given country's imports in world trade or on the proportion of a particular exported commodity originating in a given country. Yet no previous research has provided a detailed picture of the important aspects of commodity and geographical trade structure to which this small volume is devoted. Michaely's study records the commodity and geographic concentration coefficients and the commodity- and geographically-weighted shares of both exports and imports for 44 non-Communist countries for a "representative" postwar year, 1954. His basic data are the United Nations' compilation of the value of traded goods classified according to a 150 commodity group breakdown.

To marshall and generalize from these data, Michaely has computed a number of ingeniously devised indexes designed to portray neglected structural characteristics of international trade. A brief description of his concept of a commodity-weighted share will give some flavor of the novelty of his approach. He contends, with merit, that the relevant export share is not the conventional one of a country's share in total world exports; rather it is the country's share in the total world exports of those commodities which it does in fact export. Equally inventive, Michaely's commodity concentration coefficient reflects both the variety of a country's exports or imports and the relative proportions of each of these commodities in the country's export or import total. Michaely is correspondingly perceptive in conceiving indexes of other significant structural phenomena. It is apparent, then, that this is an intriguing treatment of international trade patterns.

The study goes beyond mere reporting, however. Michaely finds support in his calculations for the a priori view that a country typically tends to be relatively specialized in its export goods and diversified in the commodities it imports. Contrastingly, countries' geographically-weighted shares in exports and imports are usually not significantly different. In an important piece of analysis, valuable evidence is adduced which suggests that foreign exchange markets arising from trade in goods are more stable than might be supposed—a boon to the "elasticity optimists." The amount of international reserves, as a percentage of national product, that are required to meet fluctuations in the terms of trade is also crudely estimated, although the reader should be warned, since it is not altogether clear, that Michaely's "excess reserves" (p. 126) refers to the reserves that underdeveloped countries would hold over and above the percentage held by developed nations. By way of revision of accepted doctrine, Michaely finds no evidence that export prices of primary goods are subject to wider year-to-year fluctuation than those of manufactured goods. Similarly, he observes no tendency for the typical primary good to be exported under more geographically concentrated conditions than the average manufactured commodity.

The number of observations of this sort is legion. They are developed primarily by examining the trade concentration and weighted-share indexes that are typical of groups of countries at various per capita income strata and different stages of industrialization. Likewise, study of commodity groupings is undertaken so as to bear materially on questions relating to primary product export dependence. Nevertheless, the observations, while capably supported by the evidence, must be accepted with great caution. As Michaely realizes, significant portions of his argument depend upon the commodity classification scheme utilized, and it is not entirely obvious that the 150 commodity grouping is, on several critical issues, free of systematic bias. Contributing further uncertainty is the necessity, inevitable in a study of this kind, of making qualitative evaluations and drawing critical lines of demarcation on the basis of personal judgment. While Michaely undoubtedly has the best appreciation of the data, the conscientious reader will be pardoned if he feels uncomfortable with particular decisions when explanations are so often lacking. But basically, the research difficulties that limit the acceptability of Michaely's conclusions are unavoidable. As a source of fruitful hypotheses for further scholarly activity his study is a significant contribution. It should be read carefully and searchingly by all who are examining the problems of international economics and development.

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Foreign Trade and Human Capital. PAUL D. ZOOK, ed. Dallas: Southern Methodist University Press, 1962. Pp. viii, 102. $3.00.

Seven studies, presented as lectures at Southern Methodist University in 1960 and 1961, have since been put into book form. The studies cover: (1) the role and importance of investment in human capital, (2) the U. S. foreign aid program, (3) economic integration, and (4) a comparison of development rates in three developing countries. These essays cover some of the more important questions in the forefront of international economic discussion and U.S. policy problems.

1. Theodore Schultz continues to treat the thesis that human capital as an investment is strategic and has been neglected in the growth of developing economies. He argues that welfare programs can no longer be postponed until after basic plant and equipment are in place. This is similarly true of agriculture where he finds that inadequate technology is the cause of low productivity. He concludes that reallocation of existing types of resources will not increase income in traditional economies.

A more realistic approach to the problem of investment in education and training is by J. G. Maddox. He properly shows concern that training be in the right fields and be tied to the particular development level. He cautions that traditional allocation theory will not apply to economies which do not reflect free markets and full employment. Maddox attempts to show how total output can sufficiently outdistance population growth for the two or three decades needed to accustom the population to growth and change. His answer is to increase agricultural productivity through the introduction of modern technology. The investment in human capital would take the form of large numbers of government-employed local-level agricultural technicians. For special cases where countries are densely populated and the farmers are engaged in commercialized agriculture, out-migration of population from agriculture and development of cheap processing industries provide the means of inducing growth.

Lee Martin covers much the same ground as does Schultz using U.S. development as a case study. Particularly important is the increasing role of productivity, as compared with physical inputs, in explaining output increases. He finds that differences in the growth of individual U.S. states can be explained by the different investment-mix on social, human and community capital.

None of the three chapters will provide the answer to the question of how much should be invested in human capital. We are left with the vague admonition that this type of investment is usually at levels holding back optimum development. The concept has been treated without adequate definition, leaving the impression that it includes general education and other welfare measures. In actual practice one must restrict it to the more limited concept of training, and more particularly, training for specific skills and tasks if it is to have meaning to the development process. In Mexico, experiments have shown that investment in rural schools may not result in increased productivity or changed attitudes. Development follows quickly when isolated areas are connected to the industrial urban areas, primarily by means of roads.

The programming aspects of investment in human capital deserve greater emphasis since without it resources may actually be wasted and development hindered. Education and training are strategic when they fit into over-all development, and other than economic considerations are often more important determinants of the level of investment. Thus, assuring that jobs will be available and training used for specific tasks requires quarantees that skilled and professional workers will return to their country after training; that they will have incentives to serve in isolated and rural areas; and that the type of training is relevant to the development needs of the economy and to the existing technology.

The typical rate of population growth used by Maddox (1.5-2.0 per cent), while reflecting world-wide levels, is too low for Latin America, which approaches a rate closer to 3 per cent. This high population growth rate is basic to an understanding of the development targets set by the Latin countries themselves and to an understanding of why real rates of growth of the GNP of less than 5 per cent are usually unsatisfactory.

2. Walter Krause reviews the need for foreign assistance and provides a brief review of the Mutual Security Program from its inception in 1951. He notes that, with very rare exceptions, the rate of growth in countries which obtained assistance has not increased to any significant extent during the last decade as a result of such assistance. He concludes that the amount of aid was too small to be spread over so many countries and that no prospects exist for a greater level in the future. He suggests that, alternatively, the United States should concentrate available aid in a few select countries to assure impact and greater likelihood that they will reach a self-sustaining level. He reviews criteria for determining which few (primary) countries would be eligible for foreign assistance. Countries not meeting the criteria would receive only marginal assistance; might be assisted by other friendly developed countries; or, as members of regional economic arrangements, would tie in with primary countries.

It is not clear that the United States will find it possible to eliminate assistance for many secondary countries. Economic development is not the only U.S. objective, and political realities will often determine whether assistance will continue and in what form. In some countries, use of surplus agricultural commodities, Peace Corps, and supporting assistance provide alternative means of continuing assistance. In the short run, membership in regional economic arrangements would not seem to be meaningful to single-product countries as an alternative to U.S. assistance.

3. In one of the best treatments of economic integration, Erik Thorbecke compares the functional with the institutional approach to integration. For those interested in a simplified history of the postwar movement to European (regional) integration and some of the important welfare implications and criteria, this chapter will prove interesting and rewarding.

Unfortunately, the companion piece dealing with the Latin American Free Trade Zone by Mariano Alcocer is limited in both coverage and historical description. Alcocer views the free trade zone as another trading bloc or as the first stage of an ultimate common market. To Latin Americans, LAFTA is conceived as a broad and comprehensive program of regional economic integration which will stimulate intratrade and investment. Alcocer's preoccupation with LAFTA as a potential market for the United States is out of step with the prevailing Latin American view that LAFTA is a self-help measure. No mention is made of the controversy now under way relating to tariff reductions on new products and the merits of a payments system for LAFTA members.

4. E. N. Baklanoff compares the economic development of Mexico, Argentina and Chile during the last 30 years and attempts to determine why Mexico has performed so well while the other economies have either stagnated or retrogressed. He notes that the latter two countries (1) followed short-run uncoordinated economic policies aimed at correcting temporary structural imbalances, (2) failed to give priority to sufficient levels of investment and to the need to improve productivity in agriculture, (3) relied on quantitative restrictive controls, and (4) failed to maintain an export base to finance needed imports while attempting to industrialize and diversify. Mexico, on the other hand, while benefiting from relative advantages of location and tourism, showed constant preoccupation with control of inflation and pursuit of more general and conservative monetary and fiscal policies to protect the balance of payments in the short-run, made public-sector investments in infrastructure to avoid internal bottlenecks and gave leadership to the direction and location of industrial development in the long run.

Basic reforms and more general and longer-run solutions, rather than sharp distinctions between monetary and structural approaches, appear to be important to high-level economic development. The case of Brazil, which developed despite the lack of such conditions and with heavy inflation as a major characteristic, suggests that solutions to an understanding of growth patterns are still more or less unique.

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Business Finance; Investment and Security Markets; Insurance

Corporation Finance. By ELI SCHWARTZ. New York: St. Martin's Press, 1962. Pp. xiv, 370. $9.50.

This book was written as an undergraduate text for a one-semester course in corporation or business finance. The author considers it suitable for students who will take only one course in the subject, for those who are planning to have an undergraduate major in finance, as well as for graduate students who have had little or no preparation in the field.

In this rather brief volume, principles and concepts are stressed rather than description and detail. Because Schwartz does not think that a majority of the students who enroll in the basic finance course will become financial specialists, he considers the strictly managerial approach to be inappropriate and prefers a more general treatment as an introduction to specialized work in managerial finance and investments. He has tried to cover not only the major areas of corporation finance and financial management but also introduces material on investments, tax problems, and economic policy. The author thinks that the book's "brief but comprehensive discussion of modern areas of financial policy" will make it useful as a key manual for graduate students in economics and business.

The subject matter of the text is divided into three parts. In Part I, entitled "Fundamentals of Corporation Finance," there is a standard treatment of such subjects as: the forms of business organization, the balance sheet, the operating statements, asset and financial structures, capital markets and their regulation, promotion, common and preferred stock, and long-term debt. Part II is much less standard in its presentation. It is called "Financial Management" and discusses leverage, investment in assets and the cost of funds, debt retirement, and the financing of current operations. The final section is designated "External Relationships of the Corporation" and has chapters on economic growth, combinations, failure and reorganization, management-stockholder relations, and the corporation and the competitive system.

In attempting to write a text that (1) can be considered appropriate for a diversity of students, (2) combines the institutional and managerial approach, and (3) adds material on investments, tax problems, and economic policy, including a discussion of antitrust policy, the author has assumed a monumental task. To do all of this satisfactorily in such a short text is virtually impossible.

The most severe criticism to be levied against this book is that it does not cover adequately even the traditional topics in corporation finance and yet it brings in additional features. Standard texts for the beginning course in corporation or business finance ordinarily require from 600 to 1,000 pages, depending on their breadth of coverage, the amount of detail included, and the disposition of the author. Although it must be admitted that undue verbosity is a bore to any student, he can wade through excess verbiage, if necessary, to grasp the important factors presented. Too much brevity, on the other hand, leaves gaps to be filled.

Most of the important concepts of business finance are introduced but few of them are developed into a complete picture. Too much of the material represents essentially a summary. Indicative of the book's brevity, the whole subject of asset and financial structures covers only 16 pages, with approximately half the space filled with tables. Fifteen pages are devoted to liquidation, failure, bankruptcy, and reorganization. Mergers, combinations, acquisitions, and holding companies are also allotted 15 pages.

An instructor using this book as a basic text will need to supplement it with lectures and with substantial collateral reading. The selected references at the end of each chapter are reasonably sufficient for the undergraduate level. They are not adequate for graduate students, however, since the lists of readings consist primarily of other texts and include few of the recent studies to be found in the periodical literature.

It would not be equitable to make only critical comments about this book. The author has written with skill, and his presentation has a preciseness and a clarity of expression. Certain chapters, notably those on financial management, are very well done. Discrimination is shown also in the introduction of macroeconomic concepts. If Schwartz had either concentrated on fewer phases of corporation finance or had elaborated more thoroughly the many features he mentions only briefly, his presentation would have been markedly improved.

RICHARDS C. OSBORN

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Industrial Organization; Government and Business; Industry Studies

Competition and Monopoly: Legal and Economic Issues. By MARK S. MASSEL. Washington, D.C.: The Brookings Institution, 1962. Pp. xiii, 477. $6.75.

It is unusual perhaps for a reviewer to express his enthusiasm for a book at the outset but Mark Massel has made such a splendid contribution that praise seems fully justified in this instance. This volume deserves a wide audience. It is recommended to legislators, government administrators, the judiciary, practicing lawyers, economists, students of law and of the several social sciences.

Massel is well equipped for the task; for he is trained in the law and its practice, he is an able economist, and he speaks the language of both groups. Recognizing the valuable rôles of each discipline, actual and potential, in the area of the broad problem he discusses, he can plead effectively for a better understanding between lawyers and economists in analyzing and resolving the sticky problems involved in the application of the antitrust laws and the supporting legislation pertaining to market deportment.

The range and the depth of Massel's basic preparation for the study has been far reaching, as examination of the many pages of footnote documentation will attest. But the author has not been overwhelmed by his material. His contribution is a solid one, for he has maintained discerning perspective, judicious objectivity, and clarity of presentation.

An elaboration of an earlier Brookings lecture, the book deals with the problems of formulating and administering public policies which influence competition. Since the structure of ultimate goals with which these public policies are concerned is complex the public policies themselves are often in conflict, sometimes even contradictory. In this atmosphere of compromise and confusion, rules and administration are likewise confused and often ineffectual. It is Massel's contention that in all policies affecting competition are entwined components of political, social, and economic goals having both domestic and international implications. Nevertheless, he maintains forthrightly that we are interested primarily in maintaining a democratic society; in providing maximum personal freedom; in attaining personal economic security while nurturing incentives for increased efficiency to attain higher standards of living. He strongly advocates economic analysis as a means of clarifying the issues, reducing confusion, and attaining sensible balance in the promotion of various objectives. He calls for the clarification of the role of competition, and he urges research and empirical studies to measure the influence of competition on efficiency, growth, stability, and international trade. Only in this way, he maintains, can we reduce dependence on personal hunches and emotional value judgments.

In a chapter labeled "Government Activities" Massel reviews the present legislative and administrative framework which has grown like Topsy in response to various public pressures applied more vigorously at one time than another. Confused and conflicting laws foster a confused pattern of administration and interpretation. The author calls for a substantial review of the present structure of laws and agencies and a clearer definition of the paths to be followed in seeking public goals through competition. In our highly industrialized society he rules out government in the *simple* role of an umpire settling disputes and insuring fair play. Whether we like it or not government's economic role today is so large that its daily participation in our economy inevitably affects competition greatly. Government's current role is the product of a larger environment and cannot conform to any nostalgic concept of *laissez faire* under simpler conditions.

Space limitations prohibit an extensive treatment of another key chapter in which Massel deals with policy formulation. He takes the position that our attitude towards competition is at best ambivalent and accounts for much of our policy confusion. He contends that ambivalence produces a lack of legislative directions for the courts and the many administrative agencies; a lack of operating guides for judges and administrators; and a need for the development of nonjudicial programs to reduce the forces of monopoly. Reviewing these deficiencies and the ensuing confusion, the author calls for a closer working relationship among the several disciplines of law, economics, and political science. While he maintains that economic analysis should be used more widely in this area he firmly insists that economists must put their tools to use in more practical ways. There must be much more empirical analysis in the field of industry, many more specific and continuing industry studies. Evaluation of the consequences of judicial and administrative decrees is also highly important but long neglected.

In a subsequent chapter the author expresses appreciation of the problems of economic analysis within the framework of antitrust litigation and negotiation. He discusses lucidly the nature of litigation; the functions of the judge; the definition of issues; the collection of data; the presentation of evidence; the lawyers' arguments; circumstantial evidence; decision and decrees, including judicial supervision and modification of decrees. He calls special attention to the judge's burden in the big case which often extends over several years, producing voluminous testimony, and mountains of documentary evidence.

Following this discussion there is an excellent chapter on economic analysis in legal proceedings. The decision to prosecute is a function of the legal administration, but careful economic inquiry and analysis are basic to wise judgment before litigation is undertaken and also throughout the case once litigation has been initiated. Close working relations between lawyers and economists who understand each other and each other's language and methods are highly important.

In separate chapters, each skillfully presented, the author discusses such subjects as indicators of competition, defining the market, and statistical evidence. The substance of each of these chapters is meaty and well worth careful reading.

Finally he presents some recommendations. For his conclusions he will find much support among scholars in the field. His major recommendations are soundly conceived: for a closer working arrangement between lawyers and economists; for continuing studies of basic and important industries by government agencies and by research and academic institutions (the Wharton School study of Mutual Funds for the Securities and Exchange Commission is a good example); for continuing reviews of the operations and influences of the antitrust laws; for policies that are flexible enough to fit into the dynamics of technology, domestic business conditions, and international economic and political developments. If these recommendations, together with others he has made, have the right impact in the right places our future public policies in the field of competition and monopoly will be more clearly defined, more effectively pursued and administered, and more likely to perpetuate the virtues of competition with fewer of its vices.

William A. Carter

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De l'intégration des marchés. By Louis Phlips. Louvain and Paris: Nauwelaerts, 1962. Pp. 313. 320 Bfr.

Professor Phlips' book deals with an important problem: the competitive effects of the removal of trade barriers in the formation of a common market. In his basic conception of competition and monopoly, Phlips turns his back on the fundamental advances in the theory of monopoly and competition of Chamberlin, Joan Robinson, *et al.*, and goes back to the simple Marshallian view of competition as sellers' or buyers' rivalry aimed at profit maximization by offering better prices than one's rivals. The modern view is rejected because it considers pure or perfect competition as involving an absence of active bidding on price whereas for Phlips, it is not the structure of the market (number of sellers, degree of product differentiation, etc.) that determines the extent of competition, but solely the vigor of the rivalry on the part of producers and sellers, as manifested in bidding up or bidding down of prices.

It has been widely assumed that the elimination of internal tariff barriers in the Common Market would facilitate market entry and thereby stimulate competition. It has also been argued, on the other hand, that while many restrictive price and marketing agreements would be eliminated, one should anticipate a considerable restructuring of industry, with more intensive specialization on agreed-upon lines between former competitors, thereby introducing new and perhaps more durable types of imperfection in the competitive process. It is noted that Paragraph 85 of the Treaty of Rome exempts from condemnation restrictive agreements between competitors which "contribute to the improvement of the production or distribution of goods or to the promotion of technical or economic progress while reserving to users an equitable share in the profit resulting therefrom."

Phlips challenges these generalizations. He argues that the existing oligopolistic arrangements rely to only a negligible degree upon tariff barriers, and that the formation of a common outer tariff by providing a greater uniformity of costs within the European Economic Community may largely offset any competitive stimulus arising from the elimination of the internal tariffs.

His statistical study of parallel price movements between major brands in three industries in the Common Market countries strongly suggests the existence of restrictive price-leadership and market-sharing understandings. Particularly interesting is his demonstration that price divergences between the brands of leading competitors are often substantially higher than could be accounted for by tariffs or freight costs. For example, he shows that in 1959 Kodak sold its rolls of color film at retail for 125 per cent more than the comparable product, Gaevert, in Belgium, for 78 per cent more in Holland, for 50 per cent more in Germany, and at identical prices in France. Under this arrangement Gaevert sold its film for 57 per cent more in France than in Belgium. Meanwhile, the tariffs on films were about 19 per cent in France, 18 per cent in Germany and 24 per cent in Benelux. Transportation costs in relation to value were, of course, quite low. Thus, the existing price structure is only to a limited extent a reflection of the protection afforded by tariff barriers or transportation costs. Moreover, historical charts of the movement of prices, suggest quite clearly that the main producer in each area (Gaevert in Benelux, Kodak in France, Agfa in Germany, and Ferrania in Italy) have acted as price leaders—though the response to price changes by the followers has been typically quite slow.

In the cement industry a cartel structure has been successfully erected upon a basing point system. In the market for various types of nitrogeneous fertilizers, there is clear evidence of concentration of production in a small number of producers, and of price and market-sharing understandings between various national groups which are only to a negligible degree dependent upon existing tariff barriers. The reduction of tariff barriers will not therefore automatically intensify competition to any significant extent, though vigorous enforcement of Paragraphs 85 and 86 of the Rome Treaty could do so.

Phlips thinks it unlikely that existing market-sharing agreements along national lines will be preserved, but expects them to be largely replaced by tacit price leadership arrangements. These would create close parallelism in prices between rivals over the whole area of the Common Market, with tightly controlled selling arrangements with distributors primarily intended to provide "transparency" in pricing, thus reassuring all parties to the understanding against secret violations. This in turn will require unremitting warfare by the principal producers against unauthorized distributors and discounters whose operations would tend to break down the agreed structure of distribution. Phlips' views on this point appear to be confirmed by the intense concern now being felt by European businesses about the cases now coming up under the EEC's antitrust regulations concerning the status of exclusive distributorships and resale price maintenance agreements, and about the right of discount houses to sell standard-brand products below established prices.

Phlips has made an important contribution. It is limited however, by his somewhat old-fashioned methodology and interpretations of competition, by his exclusive concern with price competition to the virtual exclusion of product differentiation and quality competition, and by his arbitrary delimitation of the field of interest to the short term. He thereby ignores the important possibility of an increase in the number of competitors through the widening of markets as national barriers are eliminated, and also by the improved opportunities for entry of new firms financed either with foreign or domestic capital. He also ignores the possibility that cooperation between rivals in the direction of product specialization may be very much in the public interest *despite* the limitation on competition implied.

I suspect that students of competition and antitrust are going to find the developments in the next few years in Europe an extremely valuable field of study. It may be hoped that these studies may help us to break through the rigid confines of traditional monopoly-competition theory and lead to more realistic and less hypocritical regulatory standards than those now prevailing in the United States.

EMILE BENOIT

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Public Enterprise Economics and Transport Problems. By TILLO E. KUHN. Berkeley: University of California Press, 1962. Pp. xiii, 243. $5.00.

Though not separating public enterprise sharply from general government, Tillo Kuhn focuses on functions that largely serve identifiable beneficiaries and seem eligible in principle for financing through user charges. While he stresses transport investment in particular, his analysis is generally relevant where collective considerations do not intrude unduly.

His book follows other recent studies that deal mainly with water-resource development,[1] and its appeal is to specialists who would transfer theory and method to the transport sphere. This is to say that it is not an introductory treatise which examines the general case for using the benefit-cost approach where feasible or recounts the considerable record of this approach to transportation from its exciting emergence in the 1920's in the case of highways and waterways.[2] Not all the old issues are covered, nor all areas of public transport activity, but the theory of optimization in public investment is treated with skill and many insights.

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[1] Kuhn provides a useful "summary comparison" of such studies in Appendix E. One of them, by Julius Margolis, is itself a critical review of several studies: "The Economic Evaluation of Federal Water Resource Development: A Review Article," *Am. Econ. Rev.*, March 1959, *49*, 96-111.

Solution of the problem requires identification of numerous gains and costs, their measurement, and their manipulation in accordance with a complex methodology. It is simplified if budgetary limitation is assumed, for then only a ranking of projects is necessary; but Kuhn essays the more difficult task of full integration of public and private investment, so that absolute measurement becomes necessary. Not only are the direct magnitudes elusive, especially the gains, but indirect system effects intrude, as when new highways variously increase or reduce the service value of existing roads; and remoter impacts enter, as on land values in one category, and national defense in another, that require scrutiny if not inclusion.

Gains and costs appearing at different points in time become comparable only through suitable discounting; and projects face the challenge of an interest rate, a "social rate of time preference," that must reflect the opportunity cost of resource use throughout private as well as public enterprise. Noting the difficulty of selecting such a rate, or rates, Kuhn concludes (p. 130) that "a good case can be made for using an average low-risk rate from the private economy," but as to why, in reflecting private alternatives, it should be low-risk and, indeed, whether it should be pre- or posttax, he does not explain. On the whole, Kuhn's method for handling gains and costs is notably firmer than his proposals for ascertaining them, especially the "nonmarket" benefits; and his main examples are limited to urban access and egress situations.

Lack of a market test is, of course, a chief source of difficulty. Estimating gains for freight traffic may be manageable; but quantifying passenger comfort and aesthetic delight, along with time-saving and value of lives saved, is an uncertain business. Moreover a summation of benefits, as Kuhn observes, puts consumer surplus into the total that is set against costs, with some threat to market simulation in the absence of discriminatory pricing. Nevertheless the difficult task must be undertaken if the enterprise principle is followed.

Kuhn has something to say about user charges, but mainly as they pertain to investment justification and guidance. He notes the separability of the justification and financing problems and the weakness of user charges as a measure of service value under monopoly, but without showing how seriously they may lack the *quid-pro-quo* character necessary in testing specific investment worth, as also in rationing use. They possess allocative usefulness, however, as a substantial cost which enters the competition with railroads of other means of transport; though in this connection Kuhn belittles, as merely distributional, the considerable effort to assign highway costs to types of trucking.

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[2] This reviewer should confess that he is less familiar with the present than with the earlier period, which induced his doctoral thesis and an article, "Highway Policy on a Commercial Basis," *Quart. Jour. Econ.*, May 1932, *46*, 417-43.

Perhaps the chief service of user charges in the investment context is in dramatizing the enterprise concept in a way the public can understand. Indeed the main contribution of such studies as Kuhn's—an important one—may lie not in refinement of method in the use of imperfect data but simply in furthering an approach to the public economy that, where usable, is superior to the political evaluation of collectively conceived ends.

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Flags of Convenience—An International Legal Study. By BOLESLAW ADAM BOCZEK. Cambridge: Harvard University Press, 1962. Pp. xvi, 323. $8.00.

Hoary maritime traditions have come in for re-examination as a result of the substantial growth of the phenomenon of "flags of convenience" ships in the past decade. This economic and legal phenomenon, not new but highly intensified, has been the subject of study and activity in widely-ranging spheres. Originally the concern primarily of the maritime unions through the International Transport Workers' Federation as far back as 1928, the maritime unions were then quick to react to the possibilities of competitive undercutting of standards and the threat to institutional arrangements in Panamanian and Honduran registry. The period since the Second World War has seen Liberia rise to third place in national ship tonnage registries, and further though less rapid growth in Panamanian and Honduran registry.

The unions have cooperated forcefully through their international organization to seek to organize these ships, including a 4-day international boycott, and in efforts through their governments and through international bodies to obtain some measure of control over these ships. Western European shipowners in recent years have expressed their opposition to the unrestrained and uncontrolled development of this fleet. It has been the subject of protracted legal discussions including those at the U.N. Conference on the Law of the Sea, as well as the subject of international governmental maritime discussions. The object of organizing activities by U.S. maritime unions, ships under flags of convenience have become the subject of substantial litigation before the National Labor Relations Board, appeals in the lower federal courts and impending action by the Supreme Court.

Boczek's study of flags of convenience is subtitled, "An International Legal Study." It is, however, substantially more than a legal study. Rather, it is an analysis of the manner in which an important economic development in the most international of industries has resulted in the re-examination of some of the precepts of international maritime law. The function of international law, to paraphrase one authority, is to provide a common economic and social milieu in which international trade and intercourse may be carried on among diverse political units with diverse cultures and varying legal and economic systems. However, while there may be agreement on general principles, agreement as to specific applications is another matter entirely. This has been particularly true of the flags-of-convenience question.

How has this phenomenon developed? In replying to this question, Boczek states that the well-known fact of substantially higher labor costs on U.S. flag ships is insufficient to explain the growth of Panlibhon shipping. Greek shipowners could pay the lower scales prevailing on their own ships, and U. S. owners could transfer ships to registries of traditional maritime countries. The operating costs on such Panlibhon ships are not less than those of operating under the flags of most European countries. "What has made Panlibhon flags especially attractive is the fact that Panama, Liberia and Honduras have provided certain additional advantages such as very liberal corporation and shipping registration laws, and—what has been even more important—extremely liberal taxation laws favoring foreign investment, in particular, foreign-owned shipping, and providing virtual tax immunity for such enterprises. This tax immunity has, in turn, given Americans an immense competitive advantage over their European rivals, and it is mainly the relief from taxation resulting from registration under Panlibhon flags that has caused grave concern among European shipowners" (pp. 38-39). The result has been the building of a substantial shipping tonnage in recent years in world shipyards providing the most advantageous terms. Tankers and bulk carriers built and operated under these conditions represent the most modern portion of the fleet under U.S. ownership.

The term flags of convenience, while not acceptable to the antagonists in this arena, is treated as most appropriate by the author. Such registry has been provided readily to ships which are beneficially-owned and controlled by nationals of other countries, are manned by foreign crews, and hardly ever put into their ports of registry. This, as the Maritime Transport Committee of the OEEC contended in 1958, is "in contrast to the practice in the maritime countries (and in many others) where the right to fly the national flag is subject to stringent conditions and involves far-reaching obligations."

It is this absence of actual control over the operations of these companies and their ships that has resulted in concern in international law with the appropriate standards to be made applicable. In his review of the development of international law as it relates to shipping, Boczek points out that, as a general rule accepted in international law, the country of the flag of the ship has determined the ship's nationality. His presentation of evidence indicates that the standards of the national state are alone determinative of the conditions of registry. This matter of standards was the subject of international-law study at the turn of the century, as it was again more recently in connection with flags of convenience. These studies showed that individual maritime countries fixed fairly stringent legal requirements with regard to national ownership, shipbuilding in the nation, and requirements as to crews. There were exceptions to these requirements, however, as well as substantial variations even among the maritime countries. In the international legal discussions in the 'fifties, the question of a "genuine link" between the ship and the country under whose flag it operates was a matter of major discussion and was incorporated in the United Nations Law of the Sea. However, in Boczek's view this is a general principle and one which in fact cannot receive practical implementation.

U.S. maritime unions have been in the forefront in the effort to assign as a standard to flags-of-convenience ships the test of beneficial ownership. They have sought to have the National Labor Relations Act made applicable to the ships which are in the trade of the United States. In a series of decisions, the National Labor Relations Board has held the National Labor Relations Act applicable to these foreign flag ships when the shipping operations are found to be essentially those of the United States in the following respects: nationality of beneficial ownership, direction of ship operations, visits to ports location of repair and provisioning, and kind of involvement in the trade of the flag country. U.S. operators whose ships are viewed as available to the United States in the event of an emergency have contended that the assertion of such jurisdiction would result in the necessity for transfer to the flags of other maritime countries to assure their competitive operation. The Defense and State Departments have held the view that jurisdiction should not be asserted on the grounds that these ships would be lost to the control of the United States in the event of an emergency, and that difficulties would arise in our international relations. The actions of the NLRB are pending review in the courts.

Boczek's treatment is an impressive analytical review, in no wise affected by his effort to provide an answer to a controversial dilemma which will remain in the event of any answer. He finds little in international law that would preclude the operation of these ships under Panlibhon registry, or that would require the application of more stringent standards. On the matter of the application of the National Labor Relations Act, he appears to hold to the view that there has been no clear assertion of congressional intent to make this law applicable to such foreign flag ships. Generally, in his view, the maintenance of stability would warrant a denial of the assertion of jurisdiction.

The NLRB view, on the other hand, is that Congress did not set forth specific jurisdictional inclusions in the statute, and further that stability in labor-management relations in this sphere can only be maintained by ensuring recourse through the statute. While the matter remains for decision by the Supreme Court the dilemma continues. The international economic problems created by flags of convenience, as stated by the Maritime Transport Committee of the O.E.C.D. in 1961, "are temporarily lessened to a certain extent because of the poor conditions of the freight markets [but] they still exist and may become acute again when the markets revive."

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Freight Transportation in the Soviet Union. By ERNEST W. WILLIAMS, JR. Princeton: Princeton University Press, 1962. Pp. xxi, 214. $4.50.

This small book provides valuable insight into the role that transportation has played in the growth of the Soviet economy, assisted by comparisons to U.S. transport media, particularly the railroads.

In this latest study to emerge from the National Bureau's investigations of Soviet economic growth, Ernest Williams draws heavily on statistical series and analyses developed by George Novak, aided by Holland Hunter, and reproduced in the last 60 pages of the book. He acknowledges the existence of wide gaps in the data, of inadequate audit, of statistical misrepresentation by employees, but concludes that the Soviet transportation system effectively achieves the goals which the planners have set for it.

In the United States a variety of competing transport media and firms offer a wealth of shipping facilities, permitting the shipper to pick his carrier in line with his preference for speed and service, and often to select the route of carriage. A large freight plant is further augmented by facilities for carrying passengers—by rail and highway—in volume and at high speeds. The transportation system of the United States is customer-oriented, sharply competitive, and has developed considerable excess capacity.

By contrast, the Soviet system consists of a limited number of transport media and appears to be so small in capacity as to suggest inadequacy to cope with the volume of freight thrust upon it. Railroads handle 90 per cent of Soviet intercity freight traffic and an equal proportion of passenger travel, both commutation and intercity. Waterways and pipelines perform the other 10 per cent of freight service and are closely coordinated with the rail system. Highways and motor vehicle traffic are noticeably lacking. Intensive use of the rail plant is practised, with high traffic density, concentration on freight traffic, heavy utilization of light equipment, rapid turnaround of cars, accomplished in part by employing large gangs of labor to load and unload cars promptly and on a full-time basis. Trains lack the speeds achieved by fast freight service in the United States, and the Russian shipper does not have the choice of routing or the free time for the unloading of cars available to his American counterpart. But the operation results in an average density of freight traffic for the entire Russian system matching the peaks handled on American lines of high use. It represents a system which sacrifices amenities to the stern necessities of economic performance.

The structure and functioning of the Soviet railroads reflect the need to use scarce capital intensively, while employing to a maximum the relatively abundant supply of unskilled labor. Lack of capital is complicated by a shortage of fabricated steel products required in railroading and of heavy machining equipment necessary to work to close tolerances. The minimizing of capital inputs derives also from the Soviet philosophy that production is central and transportation marginal in the operation of an economy, and that transport should not be permitted to drain physical resources and skilled labor from the area of production.

Williams makes various interesting comparisons between Soviet and United States transportation history. In the USSR freight traffic multiplied about 13.5 times and industrial output about 7.5 times between 1913 and 1958; in the United States transportation output multiplied about 13.9 times and industrial output about 8.9 times between 1875 and 1920, a comparable period since U.S. industrial output in 1875 approximated that of the Soviet

Union in 1913. Today Soviet railroads carry 50 per cent more freight traffic than do U. S. railroads, and represent the largest passenger-carrying system in the world, having surpassed the British. In productivity per man, however, rail labor in the United States exceeds that of Soviet rail employees by about a third.

Williams presents evidence to show that despite shortcomings—in service and equipment, in the existence of cross-hauls and excessive hauls—Soviet transport planning is effective in obtaining high operating efficiency. Unfortunately his discussion of rate-making methods employed by Russian planners to promote economic use of the various media suffers from insufficiency of data and analysis. Also while he speaks of the lack of seasonality of Soviet freight traffic, he cites statistics (p. 66) which show that over a five-year period the high traffic month exceeded the low month by an average of 26 per cent, and he states that unshipped freight tends to pile up during the peak.

Students of transportation and of the Soviet system will find Williams' book a resourceful and useful analysis of the Russian transportation system, a starved but sturdy agent of Soviet economic growth.

W. N. LEONARD

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A History of the British Steel Industry. By J. C. CARR AND W. TAPLIN. Cambridge: Harvard University Press, 1962. Pp. 632. $11.00.

This volume is an exceedingly thorough study of the history of the British steel industry from the middle of the 19th Century to the present day. It is written by two men who have been closely associated with the industry for more than a quarter of a century, and they have been able to draw extensively not only on published materials in the public domain, but also upon the archives of the British Iron and Steel Federation and their own experience.

All aspects of the industry's history are touched upon in more or less detail. The volume deals not only with technological changes in the industry, but with its problems of industrial organization, trends of prices and of exports and imports, and with labor relations. Although voluminous, it is well written. Although a layman may tend to become somewhat confused with the plethora of technical steelmaking terms which inevitably are used, the authors have done a creditable job in trying to make these terms intelligible to the uninitiated.

For an economist there are several aspects of the history of the British steel industry as presented in this volume which should be of particular interest. One is the relative slowness with which the British steelmakers resorted to combination and cartelization as a remedy for the difficulties which the industry faced virtually from the 1890's on. It was not until the First World War that there began a marked trend towards consolidation of firms within the industry, a process which gathered momentum during the following two decades. And it was not until the 1930's that the British steel firms were willing to accept any kind of central coordination of their activities through the British Iron and Steel Federation and the government's Import Duties Advisory Committee.

The authors make it clear that one of the roots of the more or less constant crisis in the British steel industry during the first three decades of the present century was the fact that Britain fell far behind in the competitive race. By the turn of the century, the United States and Germany, and to a less degree France and Belgium, had pulled considerably ahead of Britain in technological developments in steelmaking. It was not until the 1930's that the British seriously undertook to do much more than adopt and adapt technological improvements originating on the European continent or the New World. Only then did they begin to establish research facilities with a view to putting Britain abreast of accomplishments elsewhere.

One is struck, too, by the slowness with which the British steelmakers came to accept tariff protection as one key to their salvation, even when they were witnessing large increases of imports of steel products into Britain, including extensive dumping of U.S. and continental output at prices below full costs of production. It was not until the late 1920's that the majority of the leaders of the British steel industry became converted to "tariff reform."

An economist, too, will be interested in the effects of the adoption of a system of protection upon the British steel industry. The authors provide considerable evidence that the basic result was to increase greatly the efficiency and the technological level of the industry—quite contrary to the free-trade doctrine to which the British steelmakers themselves were so long loyal.

Finally, the volume is full of incidental bits of information which were new and of interest, at least to this reviewer. For instance, throughout virtually all of its history, the British steel industry has relied more on British-mined iron ore than on imports—only in 1960 did imports outrun local sources. It may be surprising, too, to learn with what comparative grace the British steel magnates seem (at least according to Carr and Taplin) to have accepted very extensive government control over their business.

Although this volume should be of principal interest to the specialist in industrial organization, it will also make worth-while reading for anyone concerned with international trade or economic development.

Robert J. Alexander

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Land Economics; Agricultural Economics; Economic Geography; Housing

Agricultural Policy Under Economic Development. By Earl O. Heady. Ames: Iowa State University Press, 1962. Pp. ix, 682. $10.50.

Ten years ago Earl O. Heady published his major work on agricultural production economics. He now brings the same comprehensive, rigorous application of basic economic theory to analysis of agricultural policy.

Heady depicts three stages of economic development relevant to agricultural policy. In the first, rapid population growth and high income elasticities of demand for agricultural commodities provide shifts in demand which generally exceed shifts in supply. In this stage, policies designed to increase the efficiency of agriculture do not conflict with goals of income parity.

In the second stage, representing the present and recent past in the United States, demand shifts less rapidly as population growth levels off and income elasticities of demand for agricultural commodities at the farm level decline to zero or close to zero. Policies developed for the needs of the earlier period continue to provide rapid shifts of the supply schedule. This benefits consumers but leads to lower returns for farmers. Heady's book is directed largely to discussion of agricultural policy in the second stage.

The third stage is one in which a new balance is struck between the goals of increased efficiency and income parity. Secretary Freeman will be disappointed to find that this period is said to lie 10 to 20 years ahead for the United States.

Heady reminds us that much of rural poverty stems from the inadequate resources available to a large segment of the rural population. The same paucity of land, capital, and education, which causes failure in agriculture also inhibits the shift of these people to alternative occupations. He demonstrates that the current income problems of commercial agriculture arise from the combination of (a) inelastic demand which shifts only slowly over time, (b) a persistent shifting to the right of the supply schedule in response to publicly and privately supplied technological advance, and (c) an inelastic supply schedule due to an inelastic supply of inputs. Transfer of labor out of agricuture has little effect on production because the land resource is normally recombined into the remaining farm units, often with an increase in intensity of use. The burden on labor transfer is further increased by technological advance which increases substitution of capital for labor.

Heady concludes that in the short run, the major objective of policy should be to compensate farmers for the losses they sustain in increasing efficiency to the benefit of society as a whole. Because all compensation devices tend to be capitalized into fixed assets and enter the cost structure, long-run policy must concentrate on efforts to increase supply elasticity. This must occur largely through increasing the mobility of labor.

The broad outlines of Heady's thesis are presented in Chapters 1, 2, 3, and 10. In Chapters 4 and 5 he provides rigorous analysis of supply conditions for agricultural commodities. The algebraic forms are often difficult to follow due to failure to identify crucial variables. Chapter 6 reviews literature concerning demand for agricultural commodities. Chapter 7 digresses to describe the geographic pattern and structure of U.S. agriculture through the use of interindustry input-output tables. Chapters 8, 9, and 11 turn to application of welfare economics and game theory to problems of choice of policy. A number of interesting problems of equity are explored.

Chapters 12 through 16 discuss the supply of the major agricultural production inputs of labor, land, capital, and research and education. A number of interesting observations are made suggesting misallocation of research and education expenditure both to and within agriculture. The author cites numerous cases in which the marginal product of agricultural economists would, it may be hoped, have been higher if they had been allocated to other sectors of the economy.

Chapter 17 is largely concerned with the use of U.S. surpluses as they relate to low-income countries of the world. The oversimplification of issues in this chapter is apt to be misleading.

This book departs little from the main stream of current professional thought regarding United States agricultural policy. However, a number of economists, including this reviewer, will take issue with some of the author's subsidiary hypotheses and emphases, e.g.: overstatement of the immobility of agricultural labor as compared to technologically displaced labor in other sectors of the economy; major emphasis on the development role of capital gains from appreciation of land values as a means of financing the early development of U.S. agriculture; and disregard of the implications for supply elasticity of the withdrawal from agriculture of large areas in the Northeastern and Southern United States.

The strength of this book lies in rigorous analytical treatment of the structure of U.S. agricultural, and of alternative agricultural, policies. Its value would probably have been greater if the author had confined himself to this important objective. He has, however, broadened his task to include presentation of a number of hypotheses for which he does not provide rigorous testing, considerable description of history and institutions, and a vast array of empirical data. These efforts greatly lengthen the book and yet they are not approached sufficiently systematically or critically to be definitive.

By concentrating largely on the theoretical analysis, thereby greatly shortening the work, the author and editors might then have taken the time to eliminate extensive repetition, unusual grammar, missing captions on graphs, and errors of fact and citation. Their cumulative effect detracts significantly from the usefulness of the book.

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Labor Economics

Wage Policy and Long-Term Contracts. By Joseph Garbarino. Washington: The Brookings Institution, 1962. Pp. 145. $3.00; paper, $2.00.

The consequences of unrestrained conflict in labor relations in various sectors of the modern, complex U.S. economy are of such a potentially destructive nature that perforce constraints have to be placed upon them. It is therefore readily understandable why so much time, thought, and energy have been devoted to the study and devising of ways to prevent or minimize such conflict. It is against the backdrop of the heavy economic losses flowing from labor-employer disputes that the author of the volume under review discusses the supplanting of annual wage contracts by long-term contracts since the 1950's.

Along with many other specialists in the field, Garbarino views collective bargaining as an evolving system of resolving conflict between management and labor. Implicit in his approach is the concept that long-term contracts have gone hand in hand with the desire on the part of both parties to the fray to remove some of the uncertainty bound up with the bargaining process and so readily apparent in annual wage bargaining.

Although the postwar trend toward the multi-year collective bargaining contract is of importance, of more significance, according to the author, is the trend to development of a system of automatic wage adjustment during the life of these contracts. Especially interesting among the automatic wage-adjustment mechanisms is the one, first pioneered by General Motors and the United Automobile workers in 1948, which combines an escalator clause with annual wage increases. In the GM-UAW contract, the formula wage policy was implemented by linking wage rates with the Consumer Price Index, and by providing for an annual increase in money wages based on an annual improvement factor. Although temporarily on the wane, the wage-formula approach is still of importance in certain major areas of the industrial sector of the economy, most notably in automobiles.

The volume describes the objectives of the wage-formula approach as follows: ". . . to reduce the frequency of bargaining by permitting long-term contracts to be negotiated, to narrow the range of dispute, and to achieve economically viable settlements within the framework of private dispute settlements" (p. 5). The wage formula is an example of attempts to substitute what are sometimes called analytic processes for bargaining processes. On the basis of detailed analysis, the author deems the wage-formula approach to be unsuitable as a general wage policy, but he regards it as "workable from the standpoint of both the community and individual companies if confined to prosperous companies with an above-average rate of productivity increase and preferably with expanding employment" (p. 6).

Especially valuable is the summary of the experience in various major industries with the formula wage policy. Both economists and labor historians can read with profit this account of the changing fortunes of a novel approach aimed at diminishing conflict and stabilizing conditions in a notoriously unstable field. They can also benefit from the comparison made in the concluding chapter between the formula wage approach and alternative forms of wage determination, and the specification of conditions where each is most appropriate.

Garbarino has an enviable ability to write lucidly, forcefully, and with economy of expression. Within an astonishingly brief compass, he has directed attention to innovations that hold much promise from the point of view of mitigating management-labor conflict. That such experiments have been attempted, even when their chances of success were small, is a high tribute to the increasing social-mindedness and maturity of labor and management in the United States.

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Die branchenmässige Lohnstruktur der Industrie: Ein intertemporaler und internationaler Vergleich. By WALTHER G. HOFFMANN. Tübingen: J. C. B. Mohr (Paul Siebeck), 1961. Pp. xii, 191. DM 23.50; Paper, DM 19.50.

This work developed from Hoffmann's earlier studies of wages and productivity. In a sense, it is an outgrowth of the author's first work, *The Growth of Industrial Economies,* published in German in 1931 but not translated until 1958. There he had noted a remarkable similarity in the wage structures of different countries. This book represents an attempt to make a more precise analysis. Hoffmann is careful to state that it is only an attempt—but it is a bold one. We have here an empirical study that is well enough grounded in theory to place it above the usual run of German data-grubbing works.

More than other prices, the price of human labor refuses to behave with the nice regularity that so pleases the theoretician. Customary and historical factors, differences in methods of rewarding labor—in short, that whole congeries of cultural and political matters that were once called institutions—interfere with the operation of the labor market and often serve as the convenient repository for unexplained differences between theoretical and empirical data. And yet, there is enough regularity in wage movements to make the economist hope that perhaps conventional wage theory, in its operative rather than its tautological sense, is after all a useful explanatory tool. Hoffmann proceeds with that hope, and manages to deal with some, but by no means all, meta-economic factors affecting wages.

The structure of the book reveals Hoffmann's highly systematic approach, one that some of his fellow empiricists would do well to emulate. The problem is stated: what similarities and differences exist in industrial wage structures at different times and among different nations. This is followed by an exposition of the theoretical and statistical tools of investigation. Intertemporal comparisons are then made for the long period for Germany, United States, Canada, Norway, Switzerland, Japan, India, United Kingdom, Sweden, Australia, and New Zealand. The actual time-spans used vary from country to country, depending on the availability of data. The results are analyzed in the light of the age and sex composition of the work force, job qualifications, size and regional dispersion of enterprises, cost structures, and government and union policies. After a similar analysis for the short period, Hoffmann proceeds to make his international comparisons of 24 industries in 9 countries for 1949-55. The findings are discussed in the light of stages of economic development, political and trade union measures, and other factors.

A number of important conclusions emerge. The wage structures of given countries over the long run remain constant, with a tendency toward narrowing of differentials at higher stages of development. In the short run, wage structures expand and are compressed with the up- and downswing of the business cycle. Different countries display similar wage structures regardless of the stage of economic development.

In weighing the significance of his findings on wage theory, Hoffmann points out that no simple model can explain them. However, he rejects Hicks' statement that "wage differentials are always, to a considerable extent, historically determined," because it is inconsistent with the stability that he finds. Unfortunately, his tentative hypothesis regarding the development of the demand for the commodities and of the production functions involved is less than satisfactory. Hope springs eternal, as the poet would say. Nevertheless, Hoffmann's inability to make revolutionary changes in wage theory does not vitiate the value of his work. The questions he asks are well worth answering, and in this way the paths to further investigation are outlined.

Stylistically, the work suffers from having been written in the densest possible German academic prose, so that the ordinary U.S. economist with a schoolboy knowledge of German will find it difficult to read. It is to be hoped that the book will be translated. It is well worth it, and can provide much grist for the mills of economists working in the areas of labor markets and economic development.

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Industrial Organization; Government and Business; Industry Studies

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Population; Welfare Programs; Consumer Economics

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Related Disciplines

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NOTES

Paul A. Samuelson, department of economics, Massachusetts Institute of Technology, has been appointed chairman of the American Economic Association nominating committee. Suggestions for officers of the Association in 1964 should be sent to him as early as possible.

EARLY CENSUS REPORTS AVAILABLE

A number of preliminary reports have been issued from the 1962 Census of Governments. For example:

No. 2. *State Sources of Data on Local Government Finances.* (19 pp., 25c.) A descriptive bibliography.

No. 4. *Property Tax Assessments in the United States.* (15 pp., 25c.) Provides statistics on assessed values officially set in 1961 on property subject to general property taxation, showing for each state: valuations of state-assessed property, locally assessed real property, and locally assessed personal property; an estimated distribution of assessments for locally assessed real property by use class (residential, commercial, etc.); and an estimated count of taxable real properties by use class.

No. 5. *Assessed Values for Property Taxation.* (69 pp., $1.00) In addition to data originally issued in Preliminary Report No. 4, provides: related statistics for the metropolitan area portion of each state; figures on assessed values for all individual counties and for cities of over 300,000; and further information on assessed values of personal property and on special property taxes.

No. 7. *Long-Term Debt of State and Local Governments.* (3 pp., 10c.) National totals of debt as of June 30, 1962, with estimated distributions of long-term debt by purpose of issue and by scheduled maturity periods.

Copies of these publications may be ordered from the Bureau of the Census, Washington 25, D.C. A document summarizing publication plans of the 1962 Census of Governments is also available from the Bureau upon request.

Final reports of the 1962 Census of Governments, which will deal with a considerable range of subjects—governmental organization, public employment, taxable property values, and governmental finances—are scheduled for publication from early 1963 until mid-1964.

PERIODICALS FOR BERLIN

The Free University of Berlin is in urgent need of back-files of economics periodicals, particularly for the period preceding 1945 and subsequent to 1925. Parcels of back issues may be sent, at the book rate (3 cents for first 2-ounces; 1½ cents for each additional 2-ounces) to: Dean Helmut Arndt, Volkswirtschaftliche Bibliothek, Free University, Berlin-Dahlem, Garystr. 21. Duplicates will be passed on to other libraries in Berlin or West Germany, or will be exchanged with other libraries for books.

CONFERENCE ON ECONOMIC GROWTH IN THE MIDWEST

A Conference on Economic Growth in the Midwest was held at Racine, Wisconsin on October 22-23, 1962. The purpose was to provide an opportunity for economists and administrators to "explore the appropriate role of the midwestern university in regard to the question of economic growth in the region." The conference was sponsored by the Committee on Institutional Cooperation, an organization of eleven midwestern universities whose purpose "is to improve the educational and public services offered by its member institutions . . . by fostering cooperation in instruction and research, particularly at the graduate level."

National Science Foundation Advisory Panel

An advisory panel of economists has been assisting the National Science Foundation with its program of research grants. The membership of the panel is as follows: O. H. Brownlee (University of Minnesota), Werner Z. Hirsch (Washington University), Tjalling Koopmans (Yale University), Stephen McDonald (University of Texas), Gerald M. Meier (Wesleyan University), and Ralph W. Pfouts (University of North Carolina).

Announcements

The Research and Publications Committee of the American Economic Association has funds available through a grant from The Ford Foundation "for the translation and publication of significant foreign books in economics not currently available to Americans." Any individuals interested in any such project should communicate with the Acting Chairman of the Committee: Willard L. Thorp, Amherst College, Amherst, Massachusetts.

The Office of Education has announced the initiation of Project Social Studies, a project intended to improve research, instruction, teacher education and the dissemination of information in the social studies. Colleges, universities, state education departments, and through the latter, local elementary and secondary schools, are eligible to participate in the program.

Funds are available for (1) basic and applied research projects, (2) curriculum study centers, and (3) research development activities. The next deadline for submitting basic and applied research proposals is September 1, 1963, with August 15, 1963, as the deadline for curriculum study center proposals. Information and instructions for submitting applications may be obtained from:

Special Projects Area
The Cooperative Research Branch
U.S. Office of Education
Department of Health, Education,
and Welfare
Washington 25, D.C.

The Harvard Graduate School of Business Administration announces the availability of funds to aid archivists, librarians, and researchers interested in investigating topics in economic and business history or in studying the acquisition and handling of archival material, manuscripts, and books in these fields. The School's unique resources in these areas will be available to such persons during the summer of 1963. Members of the Business History Group and the staff of Baker Library will be available for consultation and guidance, but applicants who receive assistance will be free to pursue their projects as they think best. The criterion for awarding financial aid will be primarily the extent to which the use of the School's resources can be expected to advance proposed projects. This decision will be made by a committee of faculty members at the Harvard Business School. The amount of aid will be adjusted to the requirements of the individuals who are selected. Inquiries should be made by April 1, 1963, and may be addressed to Professor Ralph W. Hidy, Morgan 304, Harvard Graduate School of Business Administration, Soldiers Field, Boston 63, Massachusetts.

Deaths

Horace Belshaw, while on assignment for FAO, Rome, March 1962.

Wenzil K. Dolva, dean, School of Business, Western Reserve University, November 7, 1962.

Herman G. Enterline, professor of business education, School of Business, Indiana University.

Frank F. Hargrave, Purdue University, May 1962.

George C. Haun, Windson, California, July 21, 1962.

C. Ward Macy, professor of economics and head of the department of economics, University of Oregon, November 11, 1962.

Charles J. Tobin, market research manager of Oscar Mayer & Co., Madison, Wisconsin, November 21, 1962.

Retirements

Ernest J. Eberling, Vanderbilt University, June 1963.

Michael T. Florinsky, Columbia University, June 1962.

Harry Franklin, Georgetown University.

Carter Goodrich, Columbia University, June 1962.

Robert H. Montgomery, University of Texas, January 1963.

Samuel S. Shipman, Graduate School of Business Administration, New York University.

George W. Stocking, Vanderbilt University, June 1963.

Visiting Foreign Scholars

Torsten Gardlund, Stockholm School of Economics, Stockholm: visiting fellow, Center for Advanced Studies, Wesleyan University, spring semester, 1963.

G. E. Mingay, London School of Economics and Political Science: visiting professor, University of Wisconsin.

Alban W. Phillips, London School of Economics: visiting professor of economics, Massachusetts Institute of Technology.

G. David N. Worswick, Oxford University, England: visiting professor of economics, Massachusetts Institute of Technology.

Eugéne Zaleski, Centre National de la Récherche Scientifique, Paris: senior research scholar, Thomas Jefferson Center Studies in Political Economy, University of Virginia.

Promotions

John W. Beall: associate professor of economics, Harpur College.

Robert C. Beetham: associate research officer, Teachers Insurance and Annuity Association and the College Retirement Equities Fund.

Robert A. Behren: assistant professor, department of economics, Brooklyn College.

M. Steffy Browne: professor, department of economics, Brooklyn College.

John S. deLeeuw: associate professor, department of economics, Old Dominion College (formerly the Norfolk College of William and Mary).

Joseph R. Ewers: associate professor of business administration, School of Business, Indiana University.

Martin T. Farris: professor of economics, Arizona State University.

Louis Fier: associate professor, department of economics, Brooklyn College.

George Garvy: economic adviser, Federal Reserve Bank of New York.

Alex Hawryluk: associate professor, Old Dominion College (formerly the Norfolk College of William and Mary).

Frederick O'R. Hayes: assistant commissioner for urban planning and community development, Urban Renewal Administration, Washington.

Fred C. Hung: associate professor of economics, University of Hawaii.

John E. LaTourette: assistant professor of economics, Harpur College.

Stewart M. Lee: professor of economics, Geneva College, Beaver Falls, Pennsylvania.

Robert Lindsay: senior economist, research department, Federal Reserve Bank of New York.

Shelley M. Mark: professor of economics, University of Hawaii.

Bert C. McCammon: associate professor of marketing, School of Business, Indiana University.

Jack W. Nickson, Jr.: associate professor, department of economics, Old Dominion College (formerly the Norfolk College of William and Mary).

Alice M. Rivlin: senior staff, Brookings Institution, Washington.

Arnold W. Sametz: professor of finance, Graduate School of Business Administration, New York University.

Donald H. Sauer: associate professor of finance, School of Business, Indiana University.

Robert Solomon: associate adviser, division of research and statistics, Federal Reserve Board, and adjunct professor, The American University.

Charles H. Spencer: professor of accounting, School of Business, Indiana University.

Peter D. Sternlight: manager, securities department, Federal Reserve Bank of New York.

Peter N. Vukasin: professor of economics, Harpur College.

Jack R. Wentworth: associate professor of business administration, School of Business, Indiana University.

Ronald P. Willett: associate professor of marketing, School of Business, Indiana University.

Administrative Appointments

Harold J. Barnett: professor and chairman, department of economics, Washington University.

Arthur F. Belote: chairman, department of business administration, Old Dominion College (formerly the Norfolk College of William and Mary).

Robert Campbell: acting head, department of economics, University of Oregon.

Paul G. Clark: acting chairman, Center for Development Economics, Williams College.

John A. Cochran: chairman and professor of economics, Arizona State University.

Edward J. Cook, Fordham University: associate professor and chairman, department of economics, St. John's University.

Alexander Firfer: director, U.S. Agency for International Development Mission to Bolivia.

Ralph T. Green, Texas Commission on Higher Education: vice president in charge of research, Federal Reserve Bank of Dallas.

Robert M. Kamins: chairman, department of economics, University of Hawaii.

Laurence E. Leamer: chairman, division of social sciences, Harpur College.

Danilo Orescanin: assistant professor of business administration and assistant dean, University Extension, Indiana University, second semester 1963.

Murray E. Polakoff: chairman, finance, economics, and marketing areas, College of Business Administration, University of Rochester.

Ray C. Roberts: chairman, department of economics, Old Dominion College (formerly the Norfolk College of William and Mary).

Delbert A. Snider: chairman, department of economics, Miami University, Oxford, Ohio.

Laurence D. Stifel, Willamette University: assistant program economist, Agency for International Development, Burma.

Robert W. Stone: vice president, Federal Reserve Bank of New York and manager of the System Open Market Account.

Herbert E. Striner: director of program development, W. E. Upjohn Institute for Employment Research, Washington.

Robert R. Sturgeon: acting assistant dean, College of Arts and Sciences, Indiana University, 1962-63.

Merlyn N. Trued: assistant vice president, foreign department, Federal Reserve Bank of New York.

Robert Wessel: David Sinton professor of economics and head, department of economics, University of Cincinnati.

Appointments

Merritt G. Abrash: assistant professor of history and economics, Rensselaer Polytechnic Institute.

Richard M. Bailey: lecturer in business economics and public policy, School of Business, Indiana University.

Joseph M. Belth: visiting assistant professor of insurance, School of Business, Indiana University.

Barbara R. Berman: associate professor, department of economics, Brandeis University.

Naser G. Bodiya: assistant professor of management and marketing, University of Detroit.

Glenn Burress: assistant professor of economics, University of Cincinnati.

Earl A. Cecil: instructor in economics, College of Commerce, West Virginia University.

John D. Coupe: associate professor of economics, University of Maine.

Howard D. Crosse, Federal Reserve Bank of New York: adjunct associate professor of finance, Graduate School of Business Administration, New York University.

Kenneth E. Daane: assistant professor of economics, Arizona State University.

R. Kirby Davidson, Purdue University: position with Rockefeller Foundation.

John D. DeForest: program economist in the mission at Bogata, Colombia, Agency for International Development.

Horace J. DePodwin, General Electric Company: adjunct lecturer in finance, Graduate School of Business Administration, New York University.

Christopher Diakopoulos: assistant professor of economics, Marquette University.

Dorothy W. Douglas: special professor of economics, Hofstra College.

Holger L. Engberg, Columbia University: assistant professor of finance, Graduate School of Business Administration, New York University.

Stephen Enke, Duke University: assistant to the President, Institute for Defense Analyses, Washington.

Gerald C. Fischer: assistant professor of finance, School of Business, Indiana University.

Gary Fromm: senior staff, Brookings Institution, Washington.

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Miscellaneous

Robert A. Love was presented with honorary membership in Mu Gamma Tau, National Honorary Fraternity in Management by Beta Chapter, City College, New York, in recognition of his outstanding achievements in the field of management.

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BERNARD F. HALEY

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THE AMERICAN
ECONOMIC REVIEW

VOLUME LIII JUNE 1963 NUMBER 3

INNOVATION, CAPITAL ACCUMULATION, AND ECONOMIC DEVELOPMENT

By JOHN C. H. FEI AND GUSTAV RANIS*

A distinctive feature of the underdeveloped economy with labor surplus is the predominance of an agricultural sector characterized by widespread disguised unemployment and high rates of population growth, side by side with a small but growing industrial sector and an acute shortage of capital. In such a dualistic (i.e., two-sector) setting, the heart of the development problem lies in the gradual shifting of the economy's center of gravity from the agricultural to the industrial sector through labor reallocation. In this process each sector is called upon to perform a special role: productivity in the agricultural sector must rise sufficiently so that a smaller fraction of the total population can support the entire economy with food and raw materials, thus enabling agricultural workers to be released; simultaneously, the industrial sector must expand sufficiently to provide employment opportunities for the released workers. Moreover, in countries exhibiting the above characteristics, labor reallocation must be rapid enough to swamp massive population increases if the economy's center of gravity is to be shifted over time. Our concern is with the development process in this type of underdeveloped economy.

While we have previously analyzed the role of the agricultural sector in some detail [14], this paper concentrates on the role of the industrial sector. The growth of industrial employment opportunities and output depends mainly on the strength of two forces, capital accumulation and technological change. The greater the rate of capital accumulation, the greater the rate of expansion of industrial employment and output. Moreover, in an economy with substantial surplus labor, innovations can also make significant contributions in the same directions, especially if they are sufficiently "strong" and are biased in the labor-using direction—thus making possible maximum use of the relatively abundant factor.

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* The authors are associate professors of economics at Yale University. They wish to thank James Hanson for assistance with the statistical work of this paper.

If the combined forces of capital accumulation and innovation yield a rate of labor reallocation in excess of the rate of population growth, the economy may be considered to be successful in the development effort in the sense that the center of gravity is continuously shifted towards the industrial sector. As this process continues the underdeveloped economy will sooner or later exhaust its surplus labor and reach a significant turning point in the development process.

We aim to examine carefully the role of industrial capital accumulation and technological change during the development process. This will first be attempted abstractly in Section I, and then with reference to the actual experience of Japan (1888–1930) and India (1949–60) in Section II. In the next section we consider the growth of industrial output as well as that of employment and derive some short-run policy conclusions for development planning. Finally, we employ our theoretical apparatus to analyze in Section IV the long-run growth prospects of the dualistic economy.

I. *Capital Accumulation and Technological Change*

It is reasonable to assume that the industrial sector of our dualistic economy is "fully commercialized" in the sense that the quantity of labor demanded by that sector depends, on the one hand, on the prevailing level of real wages and, on the other, on the marginal physical productivity of labor (MPP_L). With regard to real wages, to the extent that there exists a reserve army of the disguised unemployed in the agricultural sector, it is unlikely that significant upward pressures on industrial real wages will persist. It is the recognition of this that led to the celebrated condition of an "unlimited supply of labor" (i.e., the constancy of the real wage level) by Professor Lewis.[1] This assumption, truly essential for the analysis of the development process in a dualistic economy, will be accepted as the starting point of this paper.

With regard to the MPP_L, it is clear that the size of the industrial capital stock determines this demand curve. A rapid rate of industrial investment will lead to a rapid increase of the MPP_L, and, at the constant real wage level, a rapid rate of labor absorption. However, the

---

[1] See [9]. We have attempted to link this condition with the dynamics of the agricultural sector [14].

MPP_L can also be raised by innovational activity. In fact, in view of the acute shortage of investment funds, the underdeveloped economy may have to rely heavily on technological change to supplement investment, if not to provide the major means by which the MPP_L can be raised. It is for this reason that the impact of innovation on the MPP_L needs to be as carefully investigated as that of real capital accumulation.

There are two facets of innovational activity that directly concern us in this connection. First, innovations may occur with varying intensity; the higher the intensity of innovation, the higher the MPP_L. Second, two innovations with the same intensity may differ with respect to their factor bias in the Hicksian sense [7]; the more labor using (or capital saving)[3] the innovation, the more will MPP_L be raised. Hence higher innovational intensity and a greater bias toward labor-using innovations contribute to a higher rate of labor absorption by the industrial sector. A more rigorous definition of the concepts of innovational intensity (to be denoted by J) and of the degree of labor-using bias (to be denoted by B_L) is given in the Appendix.

When the industrial real wage is constant, the relevant labor-absorption equation [see equation (A1.1) in the Appendix] becomes:

$$\eta_L = \eta_K + \frac{B_L + J}{\epsilon_{LL}} \cdot \tag{1.1}$$

In this equation,

$$\eta_L\left(\equiv \frac{dL}{dt}\Big/ L\right) \quad \text{and} \quad \eta_K\left(\equiv \frac{dK}{dt}\Big/ K\right)$$

are the rate of increase of industrial labor and capital, respectively, and ϵ_{LL} is the elasticity of the demand curve for labor (which may be thought of as an index of the strength of diminishing returns to labor). We may think of the terms on the right-hand side as causally determining the rapidity of labor absorption (η_L). Thus, η_L is higher when (1) the rate of capital accumulation is high (η_K is high), (2) the innovation occurs with a high intensity (J is high), (3) innovation has a high degree of labor-using bias (B_L is high), and (4) the law of diminishing returns to labor does not operate strongly (ϵ_{LL} is low).

The full economic significance of equation (1.1), which lies at the base of the analytical framework of this paper, may be further illustrated with the help of a simple diagram. In Diagram 1a we present the production contour map for the industrial sector with labor plotted on the horizontal and capital on the vertical axis. The solid contour lines Y_1, Y_2, etc. belong to a family of contour lines for a production function

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[3] The expressions "labor using" and "capital saving" will be used interchangeably throughout.

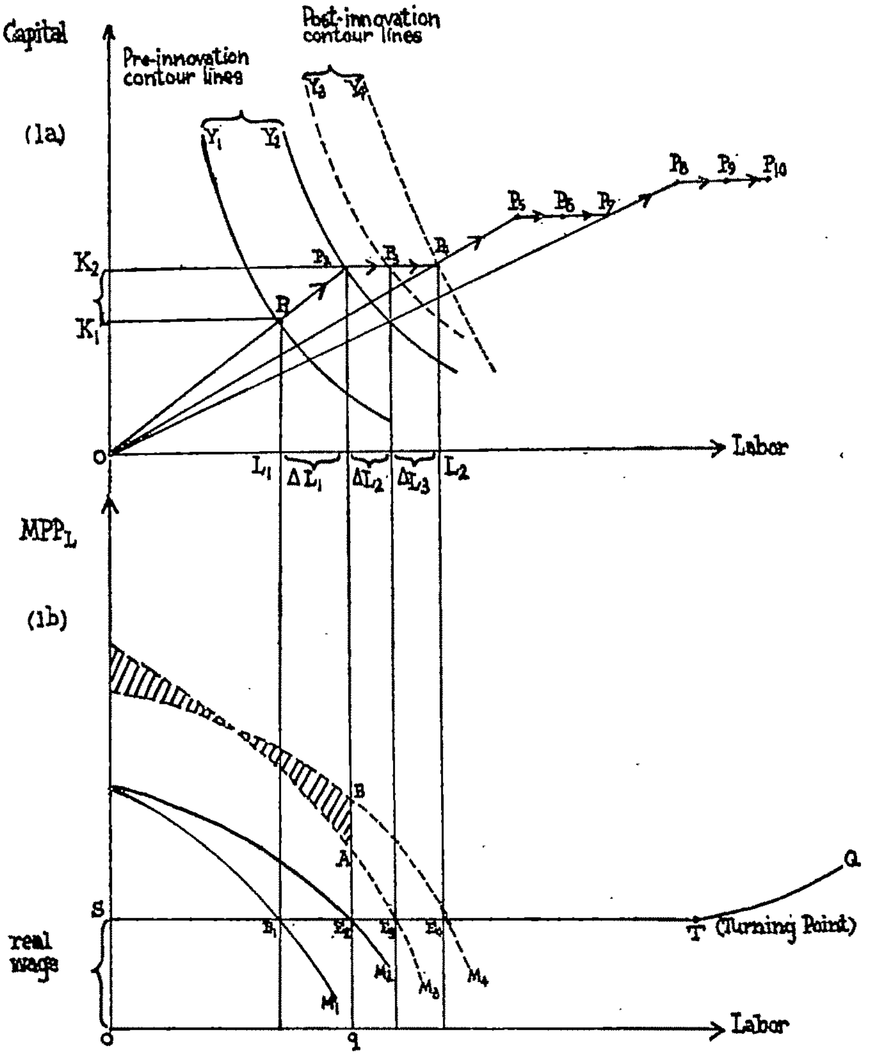

DIAGRAM 1. PRODUCTION CONTOUR MAP
INDUSTRIAL SECTOR

with constant returns to scale. In Diagram 1b, lined up vertically with Diagram 1a, the horizontal supply curve of labor *ST* (with constant real wage *OS*) is given. In this diagram negatively sloped demand curves for labor (MPP_L curves) are shown.

Suppose that, initially, the industrial sector has a capital stock K_1. Relative to K_1 and the solid contour map in Diagram 1a, the relevant MPP_L curve M_1 may be drawn in Diagram 1b leading to an equilibrium employment position at E_1 units of labor (or input point P_1 in Diagram

1a). Suppose now that capital accumulation takes place in the industrial sector increasing the capital stock from K_1 to K_2 in Diagram 1a. Relative to this larger capital stock the demand curve for labor (MPP_L) shifts up to position M_2 in Diagram 1b leading to a new equilibrium employment point at E_2 (or input point P_2 in Diagram 1a). Owing to capital accumulation, $\Delta L_1(=E_2-E_1)$ units of labor have been absorbed by the industrial sector.

Now assume that, as capital accumulation takes place, technological change also occurs. If such innovations are neutral in the Hicksian sense, the MPP_L curve M_2 is "blown up" to the position M_3 by a constant, the magnitude of which depends upon the intensity of innovation.[3] The equilibrium employment point is E_3 (and the new input point P_3 in Diagram 1a). As a result an additional $\Delta L_2(=E_3-E_2)$ workers have been allocated owing entirely to the intensity of the innovation (in the absence of any labor-using bias).

However, innovations in the industrial sector are not likely to lie on this razor's edge of neutrality. Let us instead assume that the innovation is, in fact, labor using, but of the same intensity. This may be indicated by the MPP_L curve M_4 (in Diagram 1b) drawn so that the areas of the two shaded triangles are equal (thus preserving the same output-raising effect, or intensity, of the innovation).[4] The actual equilibrium employment point after innovation is thus located at E_4 (and the new input point at P_4 in Diagram 1a). As a result of innovational bias an additional $\Delta L_3(=E_4-E_3)$ workers have been reallocated.

Thus, during any given period, the labor absorbed by the industrial sector (L_2-L_1) has three components: ΔL_1 due to capital accumulation, ΔL_2 due to the intensity of innovation (J), and ΔL_3 due to the degree of labor-using bias (B_L). Moreover, Diagram 1b clearly indicates that the "steepness" or elasticity (ϵ_{LL}) of the relevant MPP_L curve also affects the final location of the equilibrium employment point; i.e. the lower ϵ_{LL} (or the "flatter" the MPP_L curve), the more labor will be absorbed.

We can see in Diagram 1a that capital accumulation leads to a radial movement from P_1 to P_2, while innovation leads to a horizontal movement from P_2 to P_4. The first movement is radial because, under the assumption of constant returns to scale, the MPP_L can be brought into equality with the constant real wage only when the input ratio is unchanged. The second movement is horizontal because capital is being held constant. The labor-absorption effects of capital accumulation (ΔL_1 units of labor) may thus be called the ***radial effect*** and the labor-absorption effects of innovational activity (ΔL_2 plus ΔL_3) may be called the ***horizontal effect***. With innovation and capital accumulation occurring simultaneously over time, the smooth growth path in the contour map may be approximated by $P_1P_2P_3P_4P_5P_6P_7P_8P_9 \ldots$ formed of a radial effect and a horizontal effect in each finite time period.

---

[3] The intensity of innovation J is the percentage increase in output, holding inputs constant. In Diagram 1a, $J=(Y'_2-Y_2)/Y_2$ where Y'_2 is the quantity of output along the same contour, Y_2, after innovation, and in Diagram 1b the MPP_L curve M_2 is everywhere "blown up" by $1+J$ to arrive at M_3.

[4] The degree of labor-using bias is the percentage deviation from neutrality of the MPP_L-raising effects of innovation, i.e., in terms of Diagram 1b, $B_L=BA/E_2q$.

Since changes in the capital stock lead to a radial movement in the contour map, the capital-labor ratio is not affected by capital accumulation. Thus, in the course of expansion of factor inputs in the industrial sector, innovational activity determines the capital-labor ratio. Specifically, there will be capital shallowing in the industrial sector if and only if the horizontal effect is positive. These conclusions can be easily derived from (1.1). Letting the capital-labor ratio be denoted by q (i.e., $q=K/L$) and letting

$$\eta_q\left(\equiv \frac{dq}{dt}\Big/ q\right)$$

denote the rate of increase of the capital-labor ratio, we have

$$(1.2)\quad (a)\qquad \eta_q = -\left(\frac{B_L + J}{\epsilon_{LL}}\right)$$

and hence

(1.2) (b) $\eta_q<0$ when $B_L>-J$ (Capital shallowing implied by innovations which are not very labor saving, i.e., $B_L+J>0$)

(c) $\eta_q>0$ when $B_L<-J$ (Capital deepening implied by innovations which are very labor saving, i.e., $B_L+J<0$)

which is elaborated in the Appendix.[5] Thus we see that capital shallowing will occur in our economies unless innovations are very labor saving. This important conclusion will be made use of below.

II. *Analysis of Labor Reallocation*

A basic policy aim of the underdeveloped dualistic economy, which is faced with an initially unfavorable factor endowment and increasing population pressures over time, is to reallocate the maximum labor from the agricultural to the industrial sector. This central issue, which goes to the heart of the development problem, may now be more succinctly stated with the aid of our labor-absorption equation (1.1). Let the annual rate of population growth be denoted by g; it is then clear that the rate of labor absorption by the industrial sector (η_L) must be higher than g if the economy's center of gravity is to be gradually shifted. Thus we have:

---

[5] See equation (A0.8). It will also be shown in the Appendix (A0.2) that ϵ_{LL} is always positive.

$$g < \eta_L = \eta_K + \frac{B_L + J}{\epsilon_{LL}}, \tag{2.1}$$

which may be called the "critical minimum effort criterion" (CMEC) in the sense that the national effort reflected in the magnitude of the four factors on the right-hand side must exceed population growth.[6] Clearly, an underdeveloped economy may choose not to abide by this criterion in the short run but no development effort can be termed successful if the CMEC is consistently violated over time. Such violation would imply that the pool of agricultural surplus population was being augmented faster by demographic pressures than it was being diminished by reallocation to the industrial sector. This would lead to an increase in the fraction of the total population in agriculture, which is in the opposite direction from industrialization.

The CMEC suggests that government policy must ensure that the combination of capital accumulation, innovational intensity, and labor-using bias is powerful enough to overcome population pressures. Some of these factors are more amenable to policy control than others. For example, the rate of capital accumulation can be enhanced through domestic austerity programs and through an increased volume of foreign aid (higher η_K); similarly, although our knowledge here is less precise, the government is usually in a position to stimulate the right kind of innovations (higher J and B_L); and, finally, population growth may be reduced by moral suasion or by planned parenthood programs (lower g). In the search for some guidelines to policy-making in this area it may be helpful to inquire into the likely absolute as well as comparative strengths of capital accumulation and innovational activity with respect to their contribution to total labor absorption. Such information can be obtained by an examination of the relevant historical experience of individual countries.

A first step in such an examination is to decompose any total observed labor absorption (in a specific country over a specific period of time) into the horizontal effect (i.e., the amount of labor absorption which can be laid at the doorstep of innovational activity) and the radial effect (i.e., labor absorption due to capital accumulation).Our labor-absorption equation (1.1) provides us with all the simple tools

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[6] See also the relevant discussion on this criterion, from the agricultural sector's point of view, in [14].

TABLE 1—JAPAN

	Critical Minimum Effort Analysis			Input Ratio Analysis		Decomposition Analysis		
Time	Total Population thous. (z) 1.	Industrial Labor Force thous. L (η_L) 2.	Agricultural Labor Force thous. L_a (η_{La}) 2a.	Capital Stock K mill. yen 3.	Input Ratio $K/L=q$ (η_q) 4.	$\Delta L/\Delta t$ thous. (η_L) 5.	$\Delta r/\Delta t$ thous. (η_r) 6.	$\Delta k/\Delta t$ thous. (η_k) 7.
1888	39,362	4,993	17,050	21,331.8	4,272.34			
1889	39,795	5,188	17,124	21,449.9	4,134.52	195	24.5	170.5
	(1.10)	(3.90)	(.43)		(−3.35)	(3.90)	(.49)	(3.41)
1890	40,164	5,385	17,198	21,580.7	4,007.56	197	28.6	168.4
	(.93)	(3.80)	(.43)		(−3.19)	(3.80)	(.55)	(3.25)
1891	40,444	5,582	17,243	21,738.1	3,894.32	197	32.7	164.3
	(.70)	(3.66)	(.26)		(−2.93)	(3.66)	(.61)	(3.05)
1892	40,772	5,781	17,304	21,897.0	3,787.75	199	37.3	161.7
	(.81)	(3.57)	(.35)		(−2.84)	(3.57)	(.67)	(2.90)
1893	41,127	5,980	17,336	22,053.4	3,687.86	199	41.7	157.3
	(.87)	(3.44)	(.18)		(−2.73)	(3.44)	(.72)	(2.72)
1894	41,453	6,183	17,368	22,216.5	3,593.16	203	47.3	155.7
	(.79)	(3.39)	(.18)		(−2.66)	(3.39)	(.79)	(2.60)
1895	41,883	6,384	17,385	22,417.5	3,511.51	201	51.7	149.3
	(1.04)	(3.25)	(.10)		(−2.35)	(3.25)	(.84)	(2.41)
1896	42,298	6,588	17,389	22,649.5	3,437.99	204	57.5	146.5
	(.99)	(3.20)	(.02)		(−2.16)	(3.20)	(.90)	(2.30)
1897	42,765	6,803	17,392	22,885.4	3,364.02	215	66.3	148.7
	(1.10)	(3.26)	(.02)		(−2.23)	(3.26)	(1.00)	(2.26)
1898	43,275	7,000	17,382	23,152.2	3,307.46	197	66.0	131.0
	(1.19)	(2.90)	(−.06)		(−1.72)	(2.90)	(.97)	(1.93)
1899	43,736	7,216	17,356	23,439.2	3,248.23	216	78.1	137.9
	(1.07)	(3.09)	(−.15)		(−1.85)	(3.09)	(1.12)	(1.97)
1900	44,231	7,437	17,331	23,697.7	3,186.46	221	86.3	134.7
	(1.13)	(3.06)	(−.14)		(−1.96)	(3.06)	(1.20)	(1.87)
1901	44,748	7,666	17,293	23,962.6	3,125.83	229	96.4	132.6
	(1.17)	(3.08)	(−.22)		(−1.96)	(3.08)	(1.30)	(1.78)
1902	45,401	7,883	17,239	24,242.5	3,075.29	217	97.9	119.1
	(1.46)	(2.83)	(−.31)		(−1.66)	(2.83)	(1.28)	(1.55)
1903	45,988	8,111	17,187	24,520.0	3,023.06	228	109.8	118.2
	(1.29)	(2.89)	(−.30)		(−1.75)	(2.89)	(1.39)	(1.50)
1904	46,499	8,331	17,105	24,821.9	2,979.46	240	123.0	117.0
	(1.11)	(2.96)	(−.48)		(−1.73)	(2.96)	(1.52)	(1.44)
1905	46,934	8,561	17,038	25,130.4	2,935.45	210	114.1	95.9
	(.94)	(2.52)	(−.39)		(−1.28)	(2.52)	(1.37)	(1.15)
1906	47,322	8,787	16,942	25,480.8	2,899.83	226	129.9	96.1
	(.83)	(2.64)	(−.56)		(−1.25)	(2.64)	(1.52)	(1.12)
1907	47,828	9,012	16,846	25,842.7	2,867.59	225	136.3	88.7
	(1.07)	(2.56)	(−.57)		(−1.14)	(2.56)	(1.55)	(1.01)
1908	48,407	9,234	16,737	26,245.4	2,842.26	222	141.4	80.6
	(1.21)	(2.46)	(−.65)		(−.91)	(2.46)	(1.57)	(.89)
1909	49,027	9,458	16,627	26,730.4	2,826.22	224	149.5	74.5
	(1.28)	(2.43)	(−.66)		(−.58)	(2.43)	(1.62)	(.81)
1910	49,685	9,680	16,489	27,246.9	2,814.76	222	155.0	67.0
	(1.34)	(2.35)	(−.83)		(−.41)	(2.35)	(1.64)	(.71)
1911	50,396	9,907	16,352	27,792.9	2,805.38	227	165.4	61.6
	(1.43)	(2.34)	(−.83)		(−.34)	(2.35)	(1.71)	(.64)
1912	51,123	10,134	16,213	28,341.4	2,796.66	227	172.3	54.7
	(1.44)	(2.29)	(−.85)		(−.32)	(2.29)	(1.74)	(.55)
1913	51,856	10,360	16,062	28,897.4	2,789.32	226	178.4	47.6
	(1.43)	(2.23)	(−.93)		(−.27)	(2.23)	(1.76)	(.47)
1914	52,574	10,590	15,881	29,427.0	2,778.75	230	188.4	41.6
	(1.38)	(2.22)	(−1.13)		(−.39)	(2.22)	(1.82)	(.40)
1915	53,310	10,811	15,716	30,001.4	2,775.08	221	187.5	33.5
	(1.40)	(2.09)	(−1.04)		(−.14)	(2.09)	(1.77)	(.32)
1916	53,975	11,036	15,521	30,458.1	2,768.04	225	197.3	27.7
	(1.25)	(2.08)	(−1.24)		(−.26)	(2.08)	(1.82)	(.26)
1917	54,588	11,263	15,236	31,189.5	2,769.20	227	205.6	21.4
	(1.15)	(2.05)	(−1.26)		(.06)	(2.05)	(1.86)	(.19)
1918	54,960	11,486	15,132	31,926.5	2,779.61	223	208.2	14.8
	(.86)	(1.98)	(−1.28)		(.38)	(1.98)	(1.85)	(.13)
1919	55,363	11,714	14,901	32,673.3	2,789.25	228	219.1	8.9
	(.73)	(1.99)	(−1.53)		(.35)	(1.99)	(1.91)	(.08)
1920	55,944	12,415	14,848	33,453.7	2,694.62	701	712.5	−11.5
	(1.05)	(5.98)	(−.36)		(−3.59)	(5.98)	(6.08)	(−.10)
1921	56,624	12,663	14,835	34,527.1	2,726.61	248	343.8	−95.8
	(1.22)	(2.00)	(−.09)		(1.21)	(2.00)	(2.77)	(−.77)
1922	57,357	12,910	14,823	35,630.1	2,759.88	247	363.9	−116.9
	(1.29)	(1.95)	(−.09)		(1.24)	(1.95)	(2.87)	(−.92)
1923	58,331	13,159	14,810	36,651.3	2,785.26	249	387.6	−138.6
	(1.70)	(1.93)	(−.09)		(.94)	(1.93)	(3.00)	(−1.07)
1924	58,864	13,409	14,797	37,677.5	2,809.87	250	409.1	−159.1
	(.91)	(1.90)	(−.09)		(.90)	(1.90)	(3.11)	(−1.21)

TABLE 1—(*continued*)

	Critical Minimum Effort Analysis			Input Ratio Analysis		Decomposition Analysis		
Time	Total Population thous. (g) 1.	Industrial Labor Force thous. L (η_L) 2.	Agricultural Labor Force thous. L_a (η_{La}) 2a.	Capital Stock K mill. yen 3.	Input Ratio $K/L=q$ (η_q) 4.	$\Delta L/\Delta t$ thous. (η_L) 5.	$\Delta r/\Delta t$ thous. (η_r) 6.	$\Delta h/\Delta t$ thous. (η_h) 7.
1925	59,757 (1.87)	13,657 (1.85)	14,785 (−.08)	38,781.6	2,839.69 (1.08)	248 (1.85)	424.6 (3.17)	−176.6 (−1.32)
1926	60,522 (1.28)	13,904 (1.81)	14,772 (−.09)	40,067.3	2,881.71 (1.51)	247 (1.81)	440.4 (3.22)	−193.4 (−1.42)
1927	61,317 (1.31)	14,153 (1.79)	14,760 (−.08)	41,423.9	2,926.86 (1.60)	249 (1.79)	460.7 (3.31)	−211.7 (−1.52)
1928	62,122 (1.31)	14,402 (1.76)	14,746 (−.09)	42,783.9	2,970.69 (1.52)	249 (1.76)	476.5 (3.37)	−227.5 (−1.61)
1929	62,938 (1.31)	14,650 (1.72)	14,734 (−.08)	44,093.9	3,009.82 (1.34)	248 (1.72)	489.2 (3.40)	−241.2 (−1.67)
1930	64,450 (2.40)	14,898 (1.69)	14,721 (−.09)	45,710.9	3,068.26 (1.98)	248 (1.69)	502.9 (3.43)	−245.9 (−1.74)

necessary to accomplish this. For, provided the rate of increase of industrial employment η_L and the rate of increase of the industrial capital stock η_K can be independently measured (as they usually can), the rate of labor absorption due to capital accumulation is η_K (the radial effect), and the rate of labor absorption due to innovation is the difference between total η_L and the above. That is,

$$\text{(2.2)} \qquad \begin{aligned} &\text{(a)}\ \eta_r = \eta_k \text{ (radial effect)} \\ &\text{(b)}\ \eta_h \equiv \eta_L - \eta_K = \frac{B_L + J}{\epsilon_{LL}} \qquad \text{(horizontal effect).} \end{aligned}$$

The countries selected for application of this simple decomposition technique are Japan (1888–1930) and India (1949–60). Japan is selected because of the general consensus that she was able to emerge successfully from a mid-nineteenth century position which closely resembled that of the contemporary dualistic underdeveloped economy. Contemporary India is chosen because of the importance of her development effort for large segments of the underdeveloped world.

In Table 1 we present time series data for Japan spanning the period from 1888 to 1930; in all columns the figures in parentheses represent rates of increase. In column 1 we present total population, in column 2 industrial labor force, and in column 2a agricultural labor force.[7] In column 3 we have computed capital stock figures for the industrial sector which, together with industrial labor force figures in column 2, permit us to derive input ratios for Japanese industry in column 4. To help us visualize the pronounced capital shallowing in the early period of Japan's development and the capital-deepening tendency which set in after World War I, we present Diagram 2, showing the growth path

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[7] Sources and methods of estimation are presented in the Statistical Appendix.

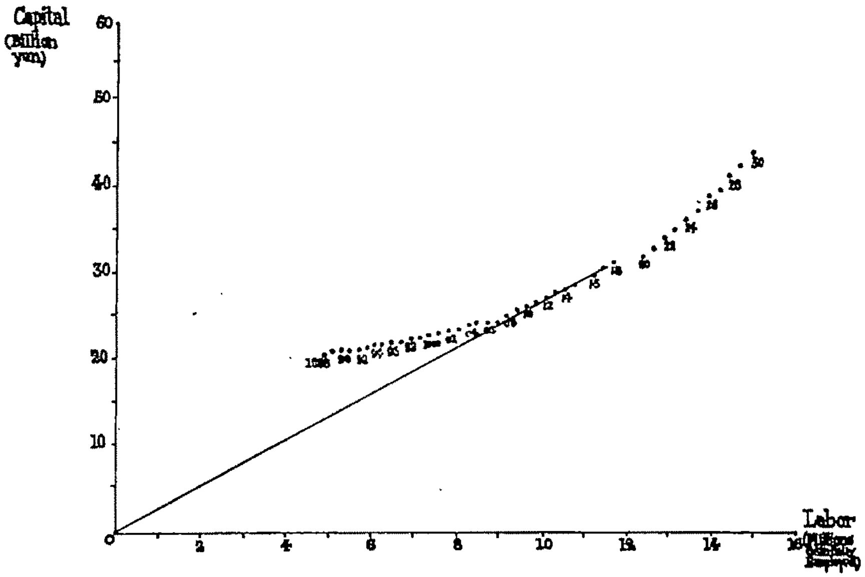

DIAGRAM 2. JAPAN: GROWTH PATH

in the contour map of Japan's industrial sector. Moreover, at the top of Diagram 3, the rates of growth of population (g), industrial labor force (η_L), and the agricultural labor force (η_{La}) in Table 1 are presented as a visual aid.

This indicates clearly that Japan's development effort does indeed meet the CMEC of this paper, namely that during her phase of unlimited supply of labor in the late nineteenth and early twentieth centuries the rate of absorption of the agricultural labor force by the industrial sector consistently exceeded the rate of growth of population. Japan's escape from the Malthusian trap is all the more impressive since the agricultural labor force not only decreased relatively but began to decline absolutely after 1897.

The results of decomposing the total amount of labor absorbed (column 5) into the radial effect η_r and the horizontal effect η_h are presented in columns 6 and 7, respectively. The time series for η_L, η_r, and η_h are also plotted at the bottom of Diagram 3. Notice that for any year the height of the η_L curve is the sum of the heights of the η_r and η_h curves.

We see, as our theory anticipated, that innovations can play an important role in the labor-absorption process. In the early years they accounted for the lion's share (around 80 per cent) of the total growth of the industrial labor force in Japan, with capital accumulation playing a relatively minor role (around 20 per cent). Over time, however, there is a definite tendency for the relative contributions of these two

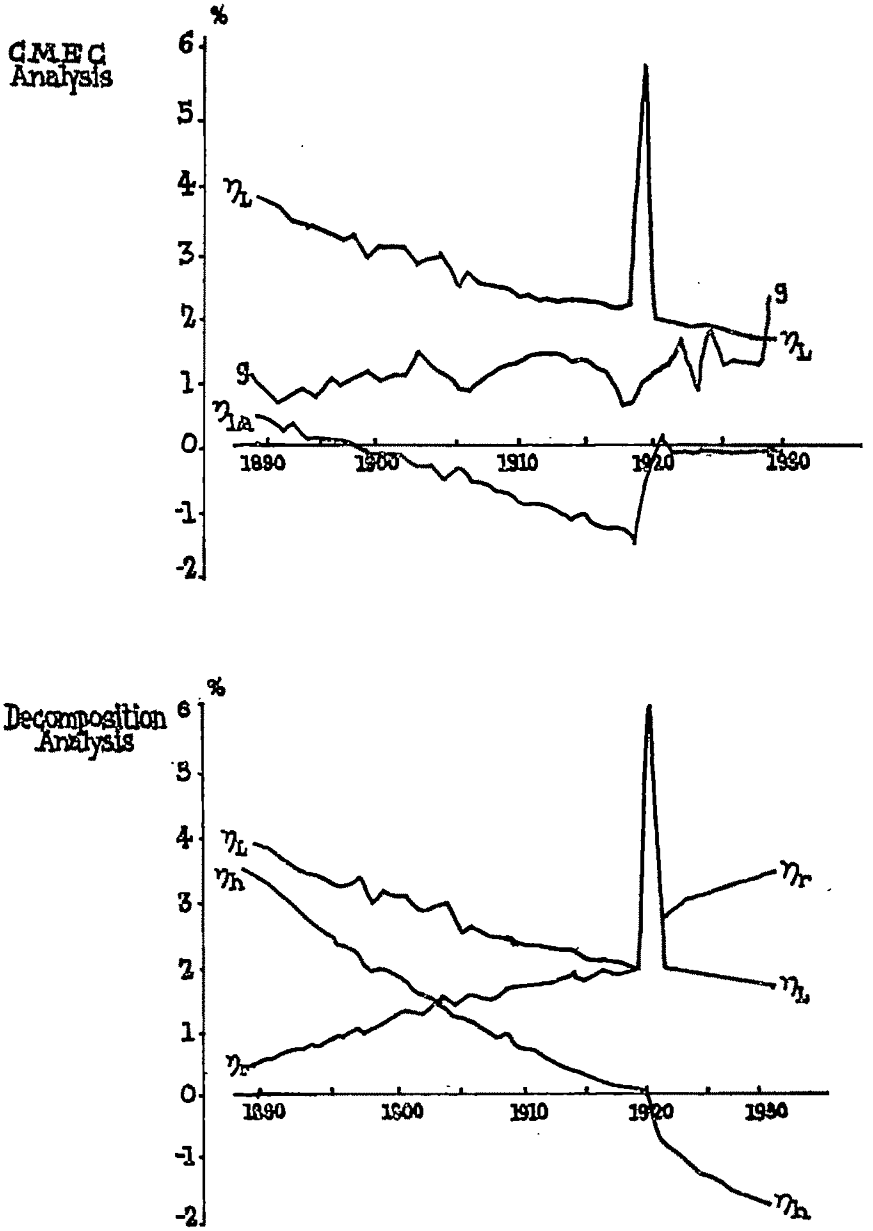

DIAGRAM 3. JAPAN

forces slowly to reverse themselves. The fuller significance of this rather striking reversal of roles will be explored in Section IV below.

Let us now apply the same technique to India over the period 1949–60 when that country undertook its first major organized effort at develop-

TABLE 2—INDIA

	Critical Minimum Effort Analysis		Input Ratio Analysis		Decomposition Analysis		
Time	Total Population thous. (g) 1.	Industrial Labor thous. L (η_L) 2.	Capital Stock K 1000 mil. Rs. 3.	Input Ratio $K/L=q$ (η_q) 4.	Δ_I/Δ_t thous. (η_L) 5.	$\Delta r/\Delta t$ thous. (η_r) 6.	$\Delta h/\Delta t$ thous. (η_h) 7.
1949	339,722	14,143	28.54	2,017.96			
1950	348,195 (2.494)	14,476 (2.4)	29.14 (2.1)	2,012.99 (−.3)	333 (2.4)	297.3 (2.1)	35.7 (.3)
1951	356,879 (2.494)	14,802 (2.3)	30.10 (3.3)	2,033.51 (1.0)	326 (2.3)	476.9 (3.3)	−150.9 (−1.0)
1952	365,776 (2.494)	15,062 (1.8)	30.66 (1.9)	2,035.59 (.1)	260 (1.8)	275.4 (1.9)	−15.4 (−.1)
1953	374,895 (2.494)	15,282 (1.5)	31.65 (3.2)	2,071.06 (1.7)	220 (1.5)	486.3 (3.2)	−266.3 (−1.7)
1954	384,241 (2.494)	15,546 (1.7)	32.67 (3.2)	2,101.51 (1.5)	264 (1.7)	455.0 (2.9)	−191.0 (−1.2)
1955	393,826 (2.494)	15,900 (2.3)	33.97 (4.0)	2,136.48 (1.7)	354 (2.3)	991.2 (6.4)	−637.2 (−4.1)
1956	401,574 (1.967)	16,721 (5.2)	37.97 (11.8)	2,270.80 (6.6)	821 (5.2)	3668.9 (23.1)	−2847.9 (−17.9)
1957	409,322 (1.929)	16,899 (1.1)	41.08 (8.2)	2,430.91 (7.1)	178 (1.1)	964.9 (5.8)	−7864 (−4.7)
1958	417,071 (1.823)	16,999 (.6)	45.17 (10.0)	2,657.22 (9.4)	100 (.6)	561.0 (3.3)	−461.0 (−2.7)
1959	424,819 (1.858)	17,183 (1.1)	49.09 (8.7)	2,856.89 (7.6)	184 (1.1)	1063.1 (6.3)	−879.1 (−5.2)
1960	432,567 (1.823)	17,255 (.4)	53.19 (8.4)	3,082.58 (8.0)	72 (.4)	425.2 (2.5)	−353.2 (−2.1)

ment. Table 2 presents all the relevant data compiled from Indian sources.[8] Columns 2 and 3 contain annual estimates of industrial labor force and capital stock which are also plotted in Diagram 4. Examination of this scatter reveals that almost from the outset India embarked on a policy of capital deepening in her industrial sector.

If we subject India to the same CMEC previously applied to Japan we find (see columns 1 and 2 of Table 2) that India does not seem to be escaping from the Malthusian trap. These data, summarized also at the top of Diagram 5, indicate that, in all but one of the years under scrutiny, the rate of population increase substantially exceeded the rate of labor absorption by the industrial sector. Thus the center of gravity in India has been shifting perversely away from the industrial and towards the agricultural sector over the short period under examination.

Applying our decomposition analysis once again, the results are

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[8] Again, sources and methods of estimation are presented in the Statistical Appendix.

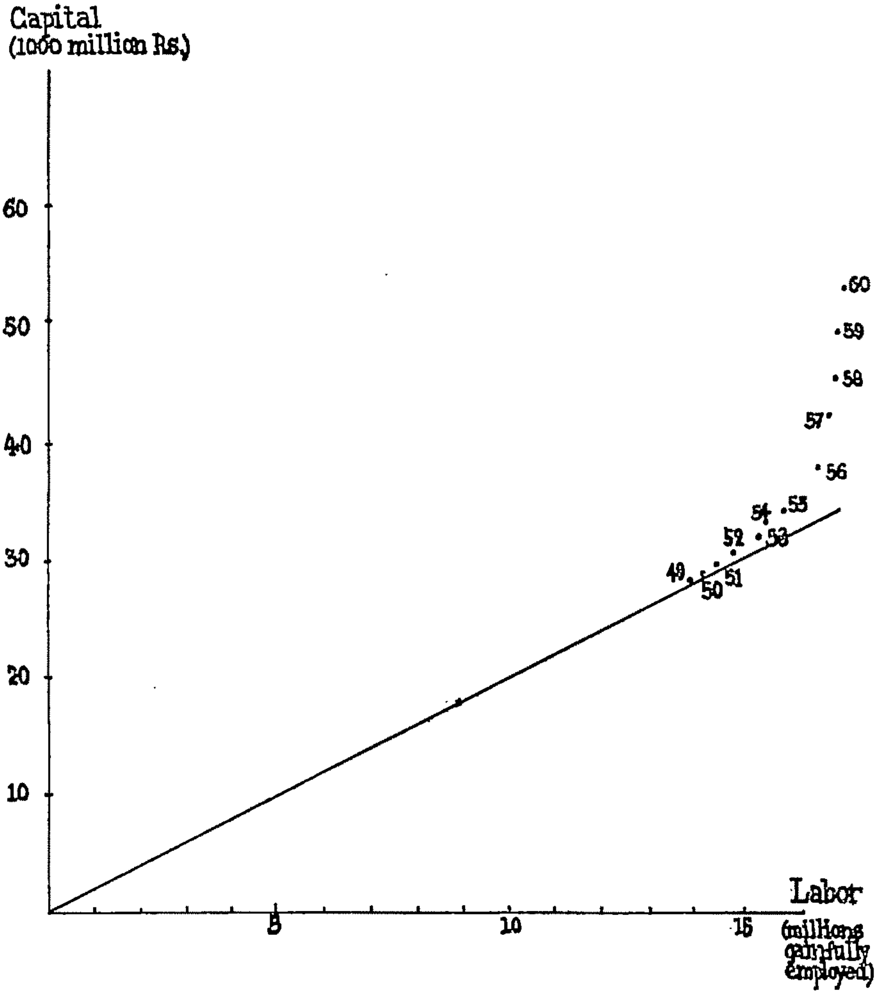

Diagram 4. India: Growth Path

presented at the bottom of Diagram 5. The striking fact is that while η_r fluctuates irregularly—with some downward tendency—η_h, again with the exception of one year, is substantially negative throughout. This means that, virtually from the outset, Indian innovational activity was of the very labor-saving variety, contributing negatively to total labor absorption by the industrial sector [see equation (1.2)].

The contrast is unmistakably clear. Continuous capital shallowing in Japanese industry between 1888 and the end of World War I is evidence that Japan made maximum use of her abundant factor, surplus agricultural labor, by adopting labor-using (or, at least, not very labor-saving) innovations. The continuously positive rate of labor absorption due to innovations, η_h, corroborates this finding. India, on the other hand,

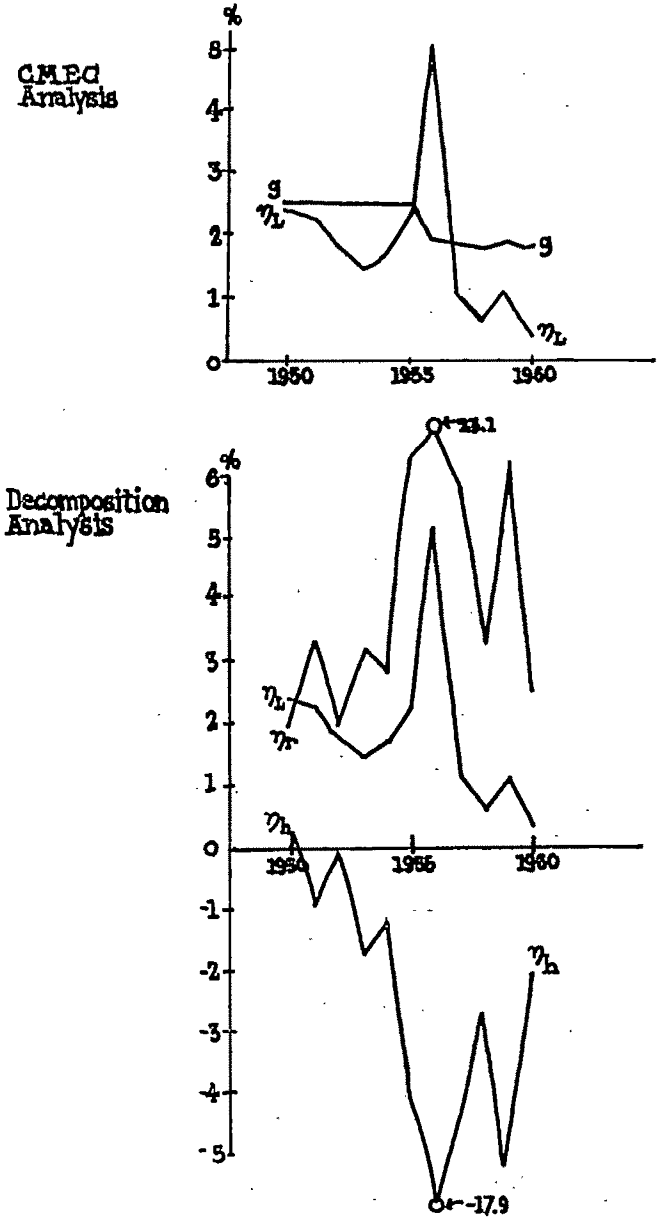

DIAGRAM 5. INDIA

apparently resorted to very labor-saving innovations from the very beginning of her development effort, thus yielding to the temptation of an increasingly capital-intensive industrial structure and neglecting the potentialities of a maximum utilization of her abundant, and rapidly growing, surplus agricultural labor force. When the CMEC is applied to both countries, we see that the emphasis on large-scale capital-intensive industry as against smaller-scale labor-intensive production functions does not make for optimum resource allocation through time and may, in fact, slow down economic progress.

Finally, it should be emphasized that the radial and horizontal effects in an economy's growth path are not necessarily independent phenomena—in spite of the fact that they can be separated by a statistical device. Not only do capital accumulation and innovation occur simultaneously through time, but they are likely to be behavioristically related. In the underdeveloped economy, for example, where capital is scarce and investment opportunities relatively plentiful, as soon as capital becomes available, it should not be difficult to find investment opportunities by exploiting the existing shelf of innovational ideas. In this case we may think of the horizontal effect as being induced by the radial effect.[9] Hence, a fuller interpretation of the experience of Japan requires us to note that the primary effect (on the rate of labor absorption) of capital accumulation can be significantly augmented by the effects of induced innovational activity.

III. *Output and Employment Criteria*

The main argument and preliminary policy conclusions of the last section were based on the acceptance of a rapid rate of labor absorption as the only criterion for "successful" development planning. Many planning commission officials, however, may take issue with this conclusion by arguing that the relevant criterion is the maximization of industrial output, and that they are willing to sacrifice the "longer-run" objective of maximizing industrial employment for the sake of this "more urgent" objective. In India, for example, the planners, while recognizing "rapid industrialization and a large expansion of employment opportunities" as basic twin objectives, finally conclude that "it is likely, however, that the Plan will not have a sufficient impact on the carry-over of unemployment of the earlier period" [4, pp. 24 and 27]. In Pakistan it was felt that, while "it is proposed to create the maximum possible employment, it will be difficult to absorb all available workers with the capital investment proposed for the Plan" [5, p. 6]. In other words, the common feeling is that there exists a conflict between maximum output and maximum employment and that the latter must be

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[9] This is in contrast to the mature economy with a relative abundance of capital (or savings) and a relative shortage of investment opportunities. In such economies it is more appropriate to think of a reverse causal ordering, i.e., "innovations induce capital accumulation" à la Schumpeter or Hansen.

(and, in fact consistently is) sacrificed.[10] However, as we intend to show in this section, this conflict is likely to be largely fictitious and its resolution, along the lines just noted, costly.

To analyze this important and rather controversial issue, let us notice first of all that, under the condition of "unlimited supply of labor," capital accumulation (η_K) and innovation activities (J, B_L) can determine not only labor absorption or industrial employment but also the rate of expansion of industrial output. In fact, we have [see Appendix (A1.3)]

$$\text{(3.1)} \quad \begin{aligned} &\text{(a) } \eta_Y = \eta_K + J(1 + \phi_L/\epsilon_{LL}) + B_L(\phi_L/\epsilon_{LL}) \\ &\qquad\qquad \text{(rate of increase of output)} \\ &\text{(b) } \eta_L = \eta_K + J/\epsilon_{LL} + B_L/\epsilon_{LL} \\ &\qquad\qquad \text{(rate of increase of employment)} \end{aligned}$$

where (3.1b) is a reproduction of our labor-absorption equation (1.1) and where, in (3.1a),

$$\eta_Y \left(\equiv \frac{dY}{dt} \Big/ Y\right)$$

is the rate of increase of industrial output and

$$\phi_L \left(\equiv \frac{\partial Y}{\partial L} L/Y\right)$$

is the labor elasticity of output.[11] Holding η_K constant in (3.1), these equations can be represented by two systems of parallel straight lines in the J—B_L space of Diagram 6:[12]

(i) a dotted system of equal-rate-of-growth-of-industrial-output contour lines (3.1a)
(ii) a solid system of equal-rate-of-growth-of-industrial-employment contour lines (3.1b).

Since the coefficients of J and B_L in (3.1) are positive, a country should direct its policy toward securing values for J and B_L as far to the "northeast" as possible—for high values of J and B_L will contribute

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[10] This discussion will remind the reader of the famous "investment allocation criteria" controversy which has been filling the literature in recent years (for a summary statement of the salient points see [1]). But the discussion has been couched in terms of partial equilibrium analysis for one-sector economies. We are attempting to carry out such analysis in a broadened framework incorporating both sectors of the dualistic economy.

[11] Under competitive assumptions in the input market, ϕ_L is the labor share of industrial output and hence is positive.

[12] At any moment of time, we may reasonably assume ϕ_L and ϵ_{LL} (which are not subject to policy influence) are positive. Notice, in Diagram 6, that the region corresponding to $-J$ is irrelevant as intensity J is always nonnegative.

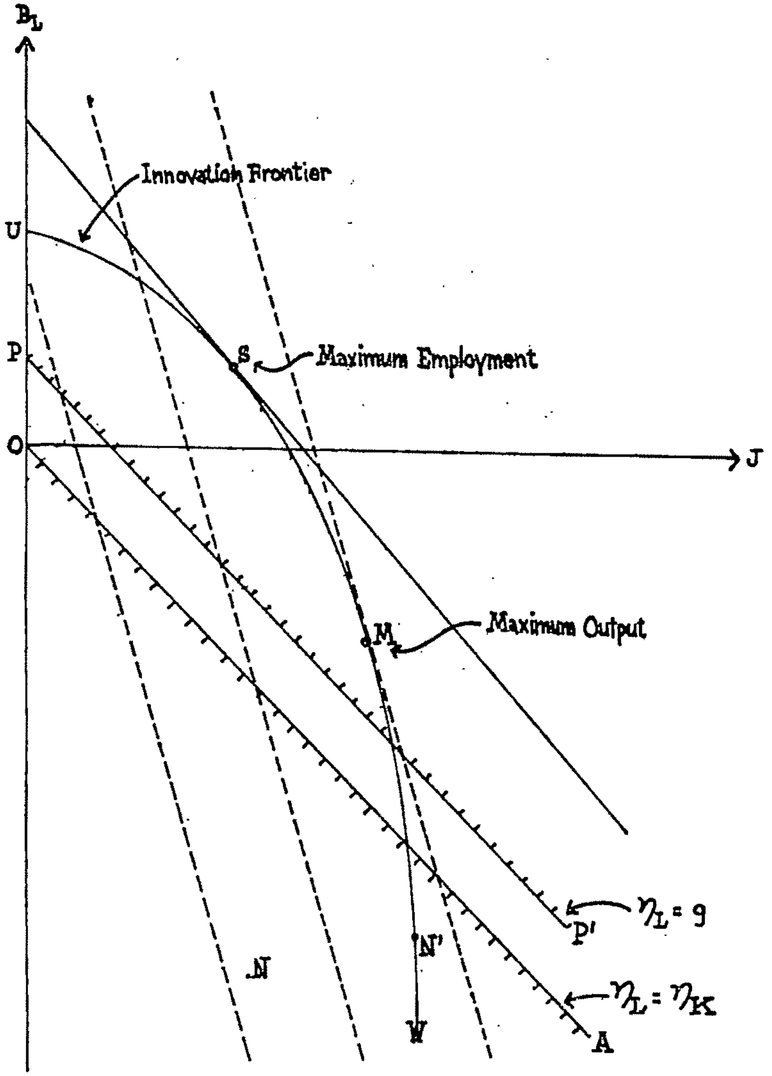

DIAGRAM 6. INNOVATION FRONTIER

both to greater employment and to greater output. Thus, it would seem that, for an underdeveloped economy with a shortage of capital and a surplus of labor, there should be no conflict between these two social objectives.

The planning commission officials, however, might continue to argue their case by pointing out that, with a limited investment fund to be allocated, the economy is unable to increase both J and B_L indefinitely. Indeed, this investment fund may be earmarked either for the importation of the latest "modern" techniques—developed in the mature economies under conditions of an entirely different factor endowment —or, alternatively, for the exploitation of domestically generated techniques. The former is likely to be characterized by high innovational intensity (J) and only modest labor absorption (B_L); the latter by a low J and a high B_L. Such choice among innovation potentialities may be represented by the negatively sloped innovation frontier line in Diagram 6, indicating that J and B_L are substitutable. Such an innovation frontier is drawn for a given rate of capital accumulation.[13]

Given the innovation frontier, the maximum employment criterion is indicated at point S (tangency with the solid system of contours) and the maximum output criterion at point M (tangency with the dotted system of contours). Since the slope of the dotted system

$$\left(=1+\frac{\epsilon_{LL}}{\phi_L}\text{ from (3.1a)}\right)$$

is steeper than that of the solid system [$=1$ from (3.1b)], point S must lie above point M on the innovation frontier. This indicates that, in order to maximize output, the maximum employment criterion must be sacrificed by choosing innovations with relatively higher intensity J, and a lower labor-using bias, B_L. This seems, at first glance, to lend some support to the above-cited typical position of the planning commission, but it needs to be subjected to more careful scrutiny.

A comparison of the performances of Japan and India may help to clarify the issue. Assume that population is growing at the constant rate g. When g is substituted in the growth equation (3.1b), a particular contour line PP' in the solid system is determined. Since this solid line represents a rate of labor absorption equal to the population growth rate, the entire region above it meets the CMEC ($\eta_L > g$) and the entire region below it does not ($\eta_L < g$). The contour line OA in the same solid system represents the boundary ($B_L + J = 0$) between very labor-saving and not very labor-saving innovations (see 1.2). Assuming that PP' lies above OA,[14] the (B_L, J) space can thus be partitioned into three regions with different economic interpretations. In the region above PP', we see that as a result of capital shallowing ($\eta_L > \eta_K$) the CMEC is satisfied. In the entire region below PP', the CMEC is not satisfied. This is because for most of the region, i.e., below OA, there is capital deepening. However, in rare cases, the CMEC may not be satisfied in spite of capital shallowing, i.e., between PP' and OA. Here population growth is so high that capital shallowing, while helpful, is not strong enough to make it possible to escape the Malthusian trap.

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[13] I.e., with a higher η_K the entire frontier moves outward in a northeasterly direction. Galenson and Leibenstein [3] argue that the investment allocation decision in one period will affect the size of reinvestment and hence the rate of growth of the capital stock. Using our terminology, this argument amounts to the notion that the choice between B_L and J in one period may affect η_K and hence the position of the frontier in the next period. Such a relationship, if proved to be quantitatively significant, can be made to supplement our analysis by rendering the system dynamically determined.

[14] On OA, with $B_L + J = 0$, $\eta_L = \eta_K$ (see 3.1b); on PP', $\eta_L = g$. If g is greater than η_K, PP' will lie above OA, as is indicated in Diagram 6. Other cases could be similarly analyzed by the same method.

As our empirical analysis in the previous section indicates, pre-World War I Japan may be represented, in almost any year, by a point above PP'. Assuming that Japan was already operating on its innovation frontier, we may thus locate her somewhere between the maximum employment criterion, S, and maximum output criterion, M. The precise location on the frontier between these two points must be determined with reference to the community's welfare function with respect to two legitimate competing social objectives.

On the other hand, if we construct a similar diagram for India, our empirical results indicate that her performance might be located at a point below OA in Diagram 6, as the CMEC is not satisfied in the presence of capital deepening. India's position may be represented by a point N' on the innovation frontier (or even by a point N within the frontier). In either case her performance may be considered nonoptimal since she can, with improved policies, obtain more of both social objectives by moving upward towards point M. It is in this context that we conclude that the maximum employment and the maximum output criteria are not usually in conflict and that the correct policy, having moved to the frontier, is to put heavier emphasis on technological change with a relatively higher B_L and lower J content.

Thus, the underdeveloped economy that desires to escape from its Malthusian trap simply cannot afford to disregard the whole range of labor-using innovational possibilities open to it. The experience of Japan in the nineteenth century certainly indicates that in the early stages of a country's development there are likely to exist many opportunities for adopting labor-using innovations requiring only modest amounts of capital and thus a minimal sacrifice of intensity. In Japan, such innovations included, among others, the natural survival of modernized cottage-style manufacturing as part of a large-scale industrial complex based on subcontracting arrangements; the speed-up of machines and the adoption of multiple shifts; and the wide-spread substitution of surplus labor in all sorts of activities peripheral to the machine process proper.[15] Most innovations of this type take the form of relatively unspectacular readjustments or rearrangements of the production process and demand only entrepreneurial attention or official policy reorientation rather than large doses of capital. What is involved is the adaptation of imported technology as well as the acceptance of entirely indigenous production functions. It is essential to resort to this reservoir of innovational ideas if the economy's abundant factor, surplus agricultural labor, is to be effectively utilized. Failure to do so, owing to a premature emphasis on so-called "modern" industries and a neglect of domestically generated production possibilities, may condemn such an economy to continued stagnation.[16]

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[15] The Japanese experience is analyzed in [13]. See also [6]. John Power, reviewing evidence on the potential growth contribution of small-scale enterprise [12], finds that "the short-run employment effect of favouring them . . . would be very substantial. Second, while labour productivity is low for these firms, it is not so low as to offset completely their favourable capital-labour ratios."

IV. *Growth in the Long Run*

We now shift the emphasis of our analysis from its short-run policy implications to the long-run growth process, assuming that an optimum short-run policy with respect to the allocation of investment funds is continuously maintained. Such long-run analysis must include both sectors of the dualistic economy, the industrial sector (which we have explicitly dealt with above), and the agricultural sector (which we have mentioned only tangentially). Moreover, it must focus on the disguised unemployed in agriculture who play a crucial role in linking the two sectors.

In Diagram 1b, which portrays the reallocation process, the horizontal portion of the supply curve of labor to the industrial sector comes to an end at point T, beyond which the supply price of labor begins to rise gradually. According to Lewis [9], this increase occurs because all the disguised unemployed have by then been reallocated from the agricultural sector, so that the "rural reserve army" is no longer able to cushion the impact of the demand for industrial workers on the level of industrial wages. We shall then assume the existence of such a turning point T, determined by the dynamics of the agricultural sector,[17] and investigate its significance for the long-run growth process in the dualistic economy.

An increase in the supply price of labor, *ceteris paribus*, will reduce the quantity of labor demanded by the industrial sector. Thus, as the turning point is reached, the process of labor reallocation is adversely affected. This may be more clearly stated by a slight modification of our labor-absorption equation (1.1) to accommodate wage increases. Such an equation [see Appendix (A1.1)] will take the form

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[16] It should, of course, be noted that not every country can be assured of escape from the Malthusian trap merely by pursuing optimal innovation policy. For some countries the entire innovation frontier may lie too far to the "southwest," perhaps because their rate of capital accumulation is too small. In our analysis above we assume the economy is not thus "doomed." Maximum use of labor-using innovations under conditions of an unlimited supply of labor is, however, likely to be at least necessary for success in the development effort.

[17] The determination of point T, or the length of the horizontal portion of the supply curve of labor, can be traced more precisely to such factors as the size of the initially disguised labor force, population growth, and the rate of productivity increase in the agricultural sector. For a full discussion of this issue—and its relation to the CMEC—see [14].

$$\eta_L = \eta_K + \frac{J + B_L}{\epsilon_{LL}} - \eta_w/\epsilon_{LL} \tag{4.1}$$

where

$$\eta_w \equiv \left(\frac{dw}{dt} \Big/ w\right)$$

is the time rate of increase of the real wage. Since $\epsilon_{LL} > 0$, we see that, as the real wage increases (i.e., as η_w becomes positive), the rate of labor absorption (η_L) declines. This equation applies after the turning point is reached and the supply price of labor begins to rise. It reduces (1.1) to a special case, i.e., when $\eta_w = 0$, before the turning point.

As our previous analysis indicates, a dualistic economy which, year after year, satisfies the CMEC, passes through a phase of capital shallowing in the industrial sector. Moreover, the experience of Japan shows that this tendency finally gives way to continuous capital deepening (i.e., after 1916–19, see Diagram 2).[18] In this final section, we examine, first, the inevitability of such a turning point marking the transition from capital shallowing to capital deepening; and, second, we analyze its significance for the long-run development prospects of the dualistic economy.

Remembering that the capital-labor ratio $q = K/L$, equation (4.1) immediately implies:

$$\eta_q = \eta_w/\epsilon_{LL} - (J + B_L)/\epsilon_{LL}, \tag{4.2}$$

which provides us with a framework for analyzing the change from capital shallowing to capital deepening.[19] There are, we believe, three basic forces that account for the inevitability of this turning point.

First, as the dualistic economy continually meets the CMEC, its domestic savings capacity—not to speak of its capacity to attract foreign capital—is likely to increase, leading to a higher rate of capital formation (η_K). Over time, moreover, the innovational activity embodied in a given rate of capital accumulation is likely to decline both with respect to its intensity (J) and with respect to the degree of its labor-using bias (B_L). This is because, initially, as Veblen [17] has pointed out, the less developed economy as a latecomer on the international scene has available for its use and adaptation the entire array of technological alternatives perfected by the advanced countries over the centuries. Rational policy implies that the most lucrative innovational ideas (in terms of their suitability for adoption or adaptation), from the viewpoint of both intensity and labor-using bias, are exploited first. Once the best innovational ideas have been used, it is reasonable to assume that the shelf cannot be constantly replenished by technological choices of the same caliber. In other words, innovational borrowing is likely to be subject to diminishing returns as a country exploits its status as a latecomer. This is especially true of labor-using innovations for which there exist physical limits to the possibilities of rearranging and reorganizing the productive processes. Thus, as η_K increases, the J and B_L embodied are likely to decline[20] leading, *ceteris paribus*, to an increasing value for η_q in (4.2).

---

[18] Moreover, the historical record of the mature economies invariably indicates continuous capital deepening over time and hence it is intuitively obvious that, sooner or later, if an economy is to "graduate" into this circle, capital shallowing must give way to capital deepening.

[19] As long as η_q is negative, there is capital shallowing; when η_q is positive, there is capital deepening.

Second, as we have already noted, if the economy is able to meet continuously the CMEC, its disguised unemployed labor force will sooner or later be exhausted, leading to a turning point in the supply curve of labor to the industrial sector. As the phase of unlimited supply of labor comes to an end, η_w becomes increasingly positive, leading once again, *ceteris paribus*, to an increasing value for η_q in (4.2).

Finally, and independent of the first two factors, with a gradual increase in the real wage and anticipations of further such increases, innovations will become increasingly labor saving.[21] The impact on η_q is evident.

Thus we see that, in the course of a successful development effort, a capital-shallowing phase in the economy's industrial sector will gradually give way to capital deepening. The turning point—or, perhaps more realistically, the turning range—occurs when diminishing returns to labor-saving innovations are telescoped with the cessation of the phase of unlimited labor supply.

In the case of Japan, according to the data presented in Diagram 2, such a turning point occurred around 1916–19, leading us to believe that her phase of unlimited supply of labor came to an end at about that time. We have, moreover, convincing independent evidence on this. Both Umemura [16] and Ohkawa [11] found Japanese industrial wages to be virtually stable before World War I, and to rise thereafter. As Kaldor put it, "in the case of Japan . . . real wages increased very little between 1878 and 1917, despite a one-and-a-half-fold increase in real income per head over the period . . . . In the period 1918–42, on the other hand, the rise in real wages fully kept pace with the growth in output per head" [8]. Such evidence serves to substantiate our empirical findings and thus our theory.

---

[20] In terms of our previous analysis in Diagram 6 this means that, through time, the outward shift of the innovation frontier with increases in η_K is subject to diminishing returns.

[21] We cannot, within the scope of this paper, deal with the precise mechanism by which such anticipations are translated into a search for innovations with labor-using bias. See, however, a recent article by Fellner [2].

Finally, the economic significance of the turning point as an important landmark in the life cycle of the developing economy may be briefly explored. First, after the turning point the rules governing the sources of capital accumulation for the economy as a whole are altered. For in the phase of unlimited supply of labor the disguised unemployed in the agricultural sector represent a "hidden" source of investment funds released in the course of labor transfer and absorption. Thereafter the transfer is no longer costless and the augmentation of the economy's savings fund must be accomplished in the traditional fashion. This point was first emphasized by Rosenstein-Rodan and Nurkse and then integrated into Lewis' pathbreaking model, in which it bears the major burden for the explanation of the "turning up" of the supply curve of labor [9] [10] [15].

Second, after the turning point the agricultural sector becomes completely commercialized. We know that even before that the industrial sector is commercialized in the sense that industrial workers are hired according to competitive equilibrium assumptions (until MPP_L equals the wage); but only afterwards is the entire economy captured by market forces as labor becomes scarce and is competitively bid for by both sectors.[22] For the agricultural sector, in particular, this may imply the displacement of family or community-centered relationships by impersonal or market-centered relationships. The emphasis on profit maximization in a fully commercialized agricultural sector is a *sine qua non* for further agricultural development and thus of considerable importance in the continuing evolution towards a fully mature economy.

Third, after the turning point the general level of real wages rises for the first time, as the disguised unemployed in the agricultural sector disappear and no longer play the role of a "reserve army." When this occurs there is a general rise in the standard of living, a major consequence of which is the impact on the pattern of consumption. According to Engle's Law, as incomes rise from a low base over time, consumption pressures will move from agricultural goods towards industrial goods.

---

[22] In [14] we have formally traced the "turning up" of the supply curve of labor to the fact that the agricultural employer is now willing to bid for the services of labor whose marginal product exceeds the institutional real wage for the first time.

This represents an increasingly powerful force after the turning point, causing a continuing reallocation of labor from the agricultural to the industrial sector. Thus, the center of gravity continues to shift towards the industrial sector until agriculture, having fulfilled its historic mission, becomes a mere appendage in the fully matured economy.[23]

Finally, an additional feature of the rise in the real wage at the turning point is the likelihood that the rate of population growth will decline —though perhaps not immediately—as the result of higher levels of income and increasing urbanization accompanying industrialization;[24] and, as an additional factor, endogenous to our model, we may assume that commercialization of the entire economy will also lower population growth as a more rational individualistic calculus replaces traditional views on family size.

Such turning-point phenomena should not, of course, be expected to occur all at once. For one thing, the phase of unlimited supply of labor will end only gradually; for another, even when there is a turning point in the labor supply curve, the various phenomena associated with it may occur only with time lags; as Marshall put it, *natura non facit saltum*. Nevertheless, the analysis above strongly suggests that the forces we have sketched will ultimately assert themselves, rendering the turning point a significant landmark in the life cycle of the developing economy.

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[23] To clarify this further, it should be recalled that, although the process of labor reallocation takes place both before and after the turning point, its meaning in the two cases is quite different. Before the turning point, the primary significance of labor reallocation is in the economy's attempt to meet the critical minimum effort criterion; after the turning point, further reallocation is caused primarily by a change in the composition of consumer demand as wages rise for the first time.

[24] This argument, of course, relies on the decline in the birth rate, which eventually dominates the population growth rate. The usual sharp declines in mortality are likely to occur in the course of the first developmental efforts, before the turning point.

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Mathematical Appendix

Let Y, K, L, and t stand for output, capital, labor, and time respectively. The production function in the industrial sector can then be written as

$$Y = f(K, L, t) \tag{A0.1}$$

which, for any fixed value of t, is assumed to satisfy the condition of constant returns to scale (CRTS) as well as the ordinary convexity condition of the production contour lines. These assumptions imply that the following elasticities are positive:

$$\begin{aligned}
&\text{(a)}\quad \phi_K \equiv f_K K/Y > 0; \qquad \phi_L \equiv f_L L/Y > 0 \\
&\qquad\qquad \text{(elasticity of } Y \text{ with respect to } K \text{ and } L) \\
&\text{(b)}\quad \epsilon_{LL} \equiv -f_{LL} L/f_L > 0; \qquad \epsilon_{LK} \equiv f_{LK} K/f_L > 0 \\
&\qquad\qquad \text{(elasticity of MPP}_{\text{L}} \text{ with respect to } L \text{ and } K) \\
&\text{(c)}\quad \epsilon_{KK} \equiv -f_{KK} K/f_K > 0; \qquad \epsilon_{KL} \equiv f_{KL} L/f_K > 0 \\
&\qquad\qquad \text{(elasticity of MPP}_{\text{K}} \text{ with respect to } K \text{ and } L).
\end{aligned} \tag{A0.2}$$

For example, $\epsilon_{LL}(\epsilon_{KK})$ is the index which measures the strength of the "law of diminishing returns" to labor (capital) and is positive. (Of course ϕ_L and ϕ_K are relative shares of labor and capital under competitive assumptions. However, as defined thus far, they are purely production concepts.) The assumption of CRTS further implies:

$$
\text{(A0.3)}\quad
\begin{array}{ll}
\left.\begin{array}{l}
\text{(a)}\ Y = f_K K + f_L L \\
\text{(b)}\ \phi_K + \phi_L = 1
\end{array}\right\} & \text{(Euler Theorem)} \\
\left.\begin{array}{l}
\text{(c)}\ Kf_{KK} + Lf_{LK} = KF_{KL} + Lf_{LL} = 0 \\
\text{(d)}\ K/L = -f_{LK}/f_{KK} = -f_{LL}/f_{KL}
\end{array}\right\} & \text{(follows from A0.3a)} \\
\text{(e)}\ \epsilon_{LK} = \epsilon_{LL}; \qquad \epsilon_{KL} = \epsilon_{KK} & \\
\text{(f)}\ \epsilon_{KK}/\epsilon_{LL} = \epsilon_{KK}/\epsilon_{LK} = \epsilon_{KL}/\epsilon_{LL} & \\
\qquad = \epsilon_{KL}/\epsilon_{LK} = \phi_L/\phi_K & \text{[follows from (A0.3cd)].}
\end{array}
$$

The above (A0.2) and (A0.3) are familiar "static" production concepts in the sense that "t" is not involved. For the analysis of innovation, we define the following "dynamic" production concepts:

$$
\text{(A0.4)}\quad
\begin{array}{ll}
\text{(a)}\ J \equiv f_t/Y > 0 & \text{(intensity of innovation)} \\
\text{(b)}\ H_L \equiv f_{Lt}/f_L & \text{(time rate of increase of MPP}_\text{L}\text{)} \\
\text{(c)}\ H_K \equiv f_{Kt}/f_K & \text{(time rate of increase of MPP}_\text{K}\text{).}
\end{array}
$$

Notice that J measures the fraction of the increase of output due entirely to innovation—holding K and L constant—and is positive, by definition. The Hicksian definition of the factor bias of innovation is:

$$
\text{(A0.5)}\quad
\begin{array}{ll}
\text{(a)}\ H_L \overset{>}{\underset{<}{=}} H_K & \text{(labor using, neutral, and capital using)} \\
\text{(b)}\ H_L < 0 & \text{(very labor saving)} \\
\text{(c)}\ H_K < 0 & \text{(very capital saving)}
\end{array}
$$

where "labor saving" ($H_L < H_K$) is used synonymously with "capital using," etc. To separate factor bias from intensity J, it is convenient to introduce two indices measuring the degree of bias of innovation at any time point t:

$$
\text{(A0.6)}\quad
\begin{array}{ll}
\text{(a)}\ B_L \equiv H_L - J & \text{(degree of labor-using bias)} \\
\text{(b)}\ B_K \equiv H_K - J & \text{(degree of capital-using bias).}
\end{array}
$$

To see the meaning of B_L and B_K, we have, by computing $\partial Y/\partial t$ from (A0.3a) and dividing by Y:

$$
\text{(A.07)}\qquad J = \phi_L H_L + \phi_K H_K = \phi_L(B_L + J) + \phi_K(B_K + J).
$$

Hence, the intensity of innovation J is the weighted average of H_L and H_K where the system of weights is ϕ_L, ϕ_K (A0.2a) and (A0.3b). This equation implies (using "iaoi" for "if and only if")

$$\text{(A0.8)}\quad \begin{aligned} &\text{(a)}\ B_L \overset{>}{\underset{<}{=}} 0 \text{ iaoi } H_L \overset{>}{\underset{<}{=}} J \text{ iaoi } H_K \overset{<}{\underset{>}{=}} J \text{ iaoi } B_K \overset{<}{\underset{>}{=}} 0 \text{ iaoi } B_L \overset{>}{\underset{<}{=}} 0 \\ &\qquad \overset{>}{\underset{<}{=}} B_K \text{ iaoi } H_L \overset{>}{\underset{<}{=}} J \overset{>}{\underset{<}{=}} H_K \\ &\text{(b)}\ H_L < 0 \text{ iaoi } B_L + J < 0 \qquad \text{(very labor saving)} \\ &\text{(c)}\ H_K < 0 \text{ iaoi } B_K + J < 0 \qquad \text{(very capital saving).} \end{aligned}$$

Thus, for example, if the innovation is labor using, B_L is positive and B_K is negative, i.e., they straddle O, with H_L and H_K straddling J. When the intensity of innovation J is given, B_L and B_K are two alternative ways of measuring factor bias and can be used interchangeably as the convenience of analysis demands. Their interdependence is seen from

$$\text{(A0.9)}\quad \begin{aligned} &\text{(a)}\ B_L\phi_L + B_K\phi_K = 0 \\ &\text{(b)}\ B_L/B_K = -\phi_K/\phi_L \end{aligned}$$

which follow readily from (A0.3b) and (A0.7). Equation (A0.9b), of course, does not apply when the innovation is neutral and $B_L = B_K = 0$.

The competitive assumption in the input market implies the equality of the real wage with the MPP_L, i.e., $w = f_L$. Differentiating with respect to "t" and using the notation η_x to denote $(dx/dt)/x$, we have

$$\frac{dw}{dt} = f_{LL}\frac{dL}{dt} + f_{LK}\frac{dK}{dt} + f_{Lt}$$

$$\eta_w = \frac{Lf_{LL}}{f_L}\eta_L + \frac{Kf_{LK}}{f_L}\eta_K + \frac{f_{Lt}}{f_L}$$

$$= -\epsilon_{LL}\eta_L + \epsilon_{LK}\eta_K + H_L \qquad \text{[by (A0.2b), (A0.4b)]}$$

$$\text{(A1.1)}\quad \frac{\eta_w}{\epsilon_{LL}} + \eta_L = \eta_K + \frac{B_L + J}{\epsilon_{LL}} \qquad \text{[by (A0.3e); (A0.6)]}$$

which is the labor-absorption equation (4.1) of Section IV and is reducible to equation (1.1) of Section I by letting $\eta_w = 0$. Differentiating the production function (A0.1) with respect to "t," we have:

$$\frac{dY}{dt} = f_K\frac{dK}{dt} + f_L\frac{dL}{dt} + f_t$$

$$\text{(A1.2)}\quad \eta_Y = \frac{f_K K}{Y}\eta_K + \frac{f_L L}{Y}\eta_L + \frac{f_t}{Y}$$

$$\eta_Y = \phi_K\eta_K + \phi_L\eta_L + J \qquad \text{[by (A0.2a), (A0.4a)]}$$

which traces the rate of growth of output to the intensity of innovation (J), the rates of growth of the factors of production (η_K and η_L) and the factor elasticities of output (ϕ_K, ϕ_L). This equation is generally valid in the sense that L and K can vary independently. If, as in our model in the text, η_L is dependent upon η_K, B_L and J, the rate of growth of output can be obtained by substituting η_L, computed from (A1.1), into (A1.2):

$$\eta_Y = \phi_K\eta_K + J + \phi_L\left(\eta_K + \frac{B_L + J - \eta_w}{\epsilon_{LL}}\right)$$

$$= \eta_K(\phi_K + \phi_L) + J(1 + \phi_L/\epsilon_{LL}) + B_L(\phi_L/\epsilon_{LL}) - \eta_w(\phi_L/\epsilon_{LL})$$

$$\text{(A1.3)} \quad \eta_Y = \eta_K + J(1 + \phi_L/\epsilon_{LL}) + B_L(\phi_L/\epsilon_{LL}) - \eta_w(\phi_L/\epsilon_{LL}).$$

When there is the condition of unlimited supply of labor (i.e., $\eta_w = 0$), equation (A1.3) reduces to (3.1a) in the text.

STATISTICAL APPENDIX

I. *Japan*

To obtain an industrial capital stock series for Japan, net real investment figures were added to or subtracted from a base figure for capital stock. Rosovsky's five-year moving averages for gross investment in nonresidential construction, durable equipment, and public investment were added[1] and deflated by Ohkawa's nonagricultural price index[2] to provide an estimate of real gross investment in fixed industrial capital. To obtain net investment figures, two alternative estimates of depreciation, 15 and 20 per cent of gross investment, were used,[3] and the resulting net fixed investment figures were added to or subtracted from the real capital stock figure for 1930 as calculated by Ohkawa.[4]

Our basic industrial labor force data are based on the number of gainfully employed and derived by subtracting the number of persons gainfully employed in agriculture, forestry, and fishing from the total gainfully employed population (total population figures are from Ohkawa's adjusted estimates.)[5] In fitting a smooth curve to the actual industrial capital and labor force figures, the turning point can be located either in 1915 (using 15 per cent for depreciation) or in 1917 (using 20 per cent for depreciation). Quadratic regressions gave the following equations:

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[1] Henry Rosovsky, *Capital Formation in Japan, 1868–1940.* New York 1961, pp. 109–10.

[2] Ohkawa Kazushi and Assoc., *The Growth Rate of the Japanese Economy Since 1878.* Tokyo 1957, p. 130.

[3] These were based on average capital consumption figures for 1934–36. Ohkawa (*op. cit.*, pp. 242 and 247) provides estimates of capital consumption which were divided by Rosovsky's gross investment for 1934–36 to obtain depreciation rates. Since these capital consumption figures fluctuated considerably over the three-year period, two alternative depreciation rates were employed in our calculation. In the body of the article all data are based on the 20 per cent assumption; however, both sets of results have been retained here as evidence that none of our basic conclusions would be affected by a change in this assumption.

[4] Ohkawa, *op. cit.*, p. 164. Although a 1935 figure was available, the 1930 data were used since they are likely to be less distorted and since the estimation methods are more fully exposed.

[5] Ohkawa, *Ibid.*, pp. 140, 145 and 246.

$$R^2 = .997$$[6]

$$K = 34{,}104.69 - 4.398134L + .0003506099L^2 \text{ (15 per cent)}$$

$$K = 34{,}775.62 - 4.044853L + .0003300030L^2 \text{ (20 per cent)}$$

with all coefficients having $|t| > 8$. Based on these regressions the turning points were located in 1911 (15 per cent) or 1913 (20 per cent).

Using only the pre-1913 figures, new regressions were calculated yielding:

$$R^2 = .999$$

$$K = 21{,}533.75 - 1.118466L + .0001714714L^2 \text{ (15 per cent)}$$

$$K = 22{,}945.75 - 1.114197L + .0001613461L^2 \text{ (20 per cent)}$$

with all coefficients having $|t| > 5.9$.

The turning points corresponding to these regressions are 1917 (15 per cent) and 1919 (20 per cent) as compared with 1915 and 1917 based on the raw data. For the post-turning-point period the following regressions were derived:

$$K = 7643700 - 10.82764L^2 + .0005954281L^2 \text{ (15 per cent)}$$

$$K = 7454200 - 10.17871L^2 + .0005599856L^2 \text{ (20 per cent)}$$

with all coefficients at $|t| > 2.7$; d_r and d_h, η_r and η_h were computed on the basis of the regression data. Consequently the data shown in Table 1 under "input analysis" are actuals yielding the turning point in 1917 and data listed under "decomposition analysis" are calculated from the regressions.

II. *India*

The basic data on Indian capital and labor were computed in the same manner as the Japanese data. Net yearly investment[7] was deflated by the price index for manufactures. The price index for the years 1949 to 1956[8] used 1938–39 quantity weights with 1953 = 100. The price index for 1953 to 1960[9] used April 1952–March 1953 quantity weights, again with 1953 = 100. The overlapping years were compared, and the differences in real investment were found to be slight. Thus the 1938–39 index was used for 1949–53 and the 1952–53 index for the years 1954–60. The total net investment figures were multiplied by the per cent of investment allocated to large- and small-scale industry in the first and second five-year plans.[10] This net investment was then added to a deflated capital stock figure for small- and large-scale industries[11] to obtain an end-of-year capital stock.[12]

---

[6] The high R^2's should not surprise us too much since the original investment data of Rosovsky are based on a moving average.

[7] UN, Economic Commission for Asia and the Far East, *Survey of Asia and the Far East, 1961*, (henceforth referred to as ECAFE) Table 8, p. 173.

[8] *Economic Bulletin for Asia and Far East*, Vol. 8, No. 2, p. 47.

[9] ECAFE; *op. cit.*, p. 202.

The industrial labor force was calculated by adding the employment figures for large-scale manufacturing and mining[13] and the employment in small-scale or cottage industries. Employment figures for large-scale industry were collected on the basis of the population covered by the Factory Acts which, after 1949, included all factories employing more than 10 workers with power or more than 20 without.[14] (The abrupt change in the employment figure for 1956 seems to be due to a change in the method of coverage.) The employment in mines for 1959 and 1960 was computed by using the average ratio of employment in coal mines to total mining employment in the years 1955–58.[15] Employment in the small-scale industrial sector was obtained for three years[16] and then calculated for the intervening years at the constant growth rate of 1.875 per cent for 1949–55 and .573 per cent for 1956–60. It should be noted that, while the Japanese industrial labor force statistics include all nonagricultural and fishing employment, the Indian employment data include only industrial workers and miners.

The figures for population were obtained from 1951 and 1961 Censuses[17] with logarithmic interpolation for the years 1949, 1950, and 1952–54. Thus

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[10] ECAFE; *Ibid.*, p. 84, Tables 3–10

	Pre-Plan (1950)	First Plan (1951–56)	Second Plan (1956–61)
Small Scale	4%	5%	4%
Large Scale	8%	8%	23%

[11] Uma Datta and Vinod Prakash, "An Estimate of Reproducible Tangible Wealth in India," *Papers on National Income and Allied Topics*, Vol. 1. New York 1960, pp. 248–57.

[12] These figures were used for end-of-year capital stock since the base-year capital figure for 1949–50 was assumed to be the average for twelve months, centered at the end of 1949. A similar procedure was followed for statistics from the plans, which usually covered parts of two years, and for employment statistics. Both Indian and Japanese base-year capital stock figures include inventories, but only Indian net investment includes them.

[13] 1949–55 General Statistical Organization of Gov. of India, *Monthly Abstract of Statistics*, May 1959, p. 105–06 and Feb. 1960, p. 68–69; 1956–1960 ECAFE, *op. cit.*, p. 170.

[14] Central Statistical Organization of Gov. of India, *Statistical Abstract 1956–1957*, p. 552.

[15] ECAFE, *op. cit.*, p. 170, Table 4; and for total mining employment, *Monthly Abstract of Statistics*, Feb. 1960, *op cit.*, p. 69, Table 1.

[16] (a) End of 1951—11.500 thousand employed (Kuchhal's *Industrial Economy of India*, Allahabad 1959). Data based on 1951 Census.

(b) End of 1955—12.619 thousand employed (Dhar and Lydall, *The Role of Small Enterprises*, p. 3; and p. 5 from *National Statistical Survey*, May–Nov. 1955, including industries 10 powered or 20 unpowered.

(c) End of 1960—it was planned that an additional 4.5 million workers would be employed under the second five-year plan. However, the actual figures are much lower. It was estimated (Kuchhal, *op. cit.*, p. 238) that only 6.5 million of the planned 8.0 million additional jobs would be available at the end of the second five-year plan. Using this ratio there would be 366,000 additional jobs available so the employment at the end of 1960 in small-scale industries was estimated to be 12.985 million.

[17] 1951—*Statistical Abstract of India 56–57*, *op. cit.*, p. 21; ECAFE, *op. cit.*, p. 168.

the coverage of the capital-labor ratios is not the same. In the case of India, it is assumed that the low K/L ratio in services and the high K/L ratio in transportation and utilities will offset each other and that the overall K/L in the nonsubsistence economy is equal to the calculated one.

Based on the industrial capital stock and labor data, the following regression for the Indian growth path was calculated:

$$K = 855.1250 - .1104050L + .000003692461L^2$$

with $|t| > 2$ and $R^2 = .994$.

Once again, d_r and d_h, η_r and η_h were computed on the basis of the regression data.

AN ECONOMETRIC ANALYSIS OF POPULATION GROWTH

IRMA ADELMAN*

There has been remarkably little theoretical or empirical analysis of the effects of economic development upon population change.[1] While the existence of an interaction between the economic and the demographic evolution of a society has long been acknowledged by authorities in both fields, only the impact of population growth on economic development has received any significant amount of attention in the economic literature.

The present paper attempts to illuminate the other side of the coin. Specifically, this manuscript constitutes an economic analysis of fertility and mortality patterns as they are affected by economic and social forces. First, age specific birth and death rates in various countries are correlated with several economic and sociocultural indicators. As a partial test of the validity of this approach, the derived relations are then used to estimate crude birth and death rates in 1953 in the various continents. Finally, a quantitative feeling for the relative impact of changes in economic and social variables upon the demographic features of a society is obtained by calculating the changes in the equilibrium age distribution and in the equilibrium rate of population growth which would result *(ceteris paribus)* from a permanent change in each of the socioeconomic variables.

I. *The Statistical Approach*

In principle, long-run relationships between demographic and socioeconomic factors can be investigated statistically from either of two points of view [4, pp. 629-630]. One possibility is to study one or more geographical units over time and determine the variations in fertility and mortality which are, on the average, associated with changes in

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* The author is associate professor of economics, Johns Hopkins University. This paper was written while the author was the recipient of a Social Science Research Council Faculty Research Grant. In addition, the author is indebted to the Stanford University Research Center in Economic Growth for financial assistance. She would like to thank J. J. Spengler, K. Davies, and R. A. Easterlin for their helpful suggestions during early stages of this work, and George Fishman, Mildred Mikelson, and George Guy for their computational assistance.

[1] Some notable exceptions to this statement are to be found in the work of G. S. Becker [24, pp. 209-31], R. A. Easterlin [4], E. E. Hagen [6], H. Leibenstein [9], B. Okun [12], and G. H. Orcutt, M. Greenberger, J. Korbel, and A. M. Rivlin [13].

the per capita income, degree of industrialization, education, and other relevant characteristics of each unit. For conclusions of general validity to be drawn from this approach, a similar analysis must be repeated for several countries over long periods of time.

Alternatively, a number of geographical units can be investigated at the same point in time. The influence of income, industrial development, and other socioeconomic indicators upon fertility and mortality can then be established quantitatively, in some average sense. One advantage of this approach is that the greater range of variation in characteristics among countries and the lesser degree of interaction among the explanatory variables permit a much more accurate determination of regression coefficients than does time-series analysis. However, there is a fundamental difficulty inherent in this cross-section technique: in order to draw any conclusions from the data it must be assumed that, regardless of any differences in environmental and historical conditions, each human population, from a demographic point of view, responds to a small number of more or less quantifiable socioeconomic variables as if it were drawn from a homogeneous environment. In view of the tremendous variations in values, outlook, and other sociocultural imponderables throughout the world, this condition is a very severe one; fortunately, it is open to empirical test. For, whenever specific cultural factors lead to systematic deviations in demographic behavior in different countries, low correlation coefficients, large standard errors, and significant differences in regression coefficients among individual subsamples will be observed.

In choosing between the techniques it must also be recognized that variations in the over-all (or crude) birth and death rates reflect not only changes in fertility and mortality conditions, but also differences in the age and sex distribution of the populations. In order to isolate the impact of socioeconomic forces upon fertility and mortality patterns, therefore, one must eliminate those variations in general birth and death rates which occur for purely demographic reasons; for it is the response of *age specific* birth and death rates to changes in economic and social factors that constitutes the focus of our study.

Since extended time series for age specific birth and death rates are unavailable even for the United States [4, p. 876], the time-series approach cannot effectively be used. The present study must therefore rely upon cross-country analysis to establish long-term effects of economic and sociocultural factors on age specific birth and death rates.

II. *Input Data*

The data upon which this study is based are primarily the result of a concerted effort by the United Nations to maximize the number of countries for which good demographic and other statistical information would exist. All countries for which roughly comparable demographic and economic data were available at some date in the period from 1947-57 were included in the study.

For each country in our sample the most recent year for which age specific demographic statistics could be found was determined, and the values of the independent (economic and sociocultural) variables were centered on or around that date. The sample used for the fertility study consists of 37 countries, whose annual per capita incomes range from $125 (Morocco) to $1900 (United States), with about half the incomes below $350. The geographic distribution of the observations is also wide, even though Africa and Asia are relatively underrepresented. The mortality study was performed on a similar sample of 34 nations. The list of countries included in the fertility and mortality samples, the data used for each study, and the sources and methods of computation are given in detail in tables available upon request from the author.

III. *Fertility Analysis*

A. *Determinants of Fertility*

Effects of economic conditions upon birth rates, at least in the short run, have often been observed by demographers. For example, Yule [15] found a weak-lagged positive correlation between the course of the business cycle in Great Britain during the latter half of the 19th century and the deviation of birth rates from their secular trend. Similar results were obtained by Ogburn and Thomas [11] for the United States for 1870-1920. From the years 1920-41, Kirk and Nortman [8], working with percentage deviations from the trend, found a correlation coefficient of + .77 between birth per thousand women of childbearing age and real per capita income. Their results were confirmed by Galbraith and Thomas [5], and, more recently, by Becker [24, pp. 209-231].

The long-run relationship between fertility and economic conditions is less clearly established. On the one hand, the normal pattern of fertility differentials among social and economic classes is that of negative association between socioeconomic status and fertility [20, pp. 86-88] [24, p. 5]. Moreover, casual empiricism suggests that, at least up to World War II, differences in fertility among countries were negatively correlated with their levels of economic development. On the other hand, the postwar baby boom experienced by the high-income Anglo-Saxon countries in sparsely populated areas argues in the opposite direction. In addition, Easterlin [4] has adduced strong evidence for the existence of a positive long-run relationship between births and economic conditions in the United States from 1890 to the present. Since the direction and extent of long-run association between childbearing and living standards is unclear, it would appear that one of the socioeconomic variables whose long-run effects upon fertility ought to be analyzed in this study is the level of real per capita national income (or some similar indicator).

A second set of factors commonly credited with responsibility for the continued decline in fertility which occurred prior to 1930 in northwestern Europe is the social and economic transformation brought about by the Industrial Revolution [17, pp. 76-80]. The resulting urbanization and industrialization process appears to have led to persistent changes in fundamental attitudes towards family limitation. Indeed, in all countries, the historical decline in fertility has been preceded by or accompanied by a great shift of the population from country to city. And both the urban-rural differential in fertility and the agricultural-industrial differential have persisted through time [24, pp. 77-87].

A logical indicator of urbanization is, of course, the percentage of the total population living in cities. Similarly, it is reasonable to choose as an indicator of industrialization the percentage of nonagricultural employment of the labor force. From preliminary tests on our sample data, however, it became apparent that there is a very high degree of correlation between these two indicators. Since, in addition, data on urban and rural populations are not internationally comparable because of national differences in the definitions of "urban" residents [23, pp. 1-2], it was decided to use the nonagricultural employment figures alone to represent the variety of socioeconomic forces related to the urbanization-industrialization complex.

Another major influence upon childbearing behavior is the mother's level of education [17, p. 89]. While the net direction of the effect of education upon fertility for college graduates is somewhat uncertain, at lower education levels a clear inverse correlation between the number of years of schooling of the wife and family size is well established from cross-sectional studies [24, pp. 155-70]. Unfortunately, there exist no international data on education for the female population alone; instead, an educational index for the over-all population is included in our regression analysis. This index was constructed by computing the unweighted arithmetic average of the literacy index and an index of newspaper circulation (tons per year) per capita, each based on U.S. = 100. These particular indicators of the educational level of a population were chosen primarily because of their ready availability. The averaging procedure was adopted in order to gain some discrimination among high-income countries, since the literacy index reported for most developed nations is 100.

A measure of population density is also included in the analysis. This was done because, ever since the days of Malthus, the concept of a stable optimum population size which maximizes the per capita income of a nation for a given set of techniques, resources, tastes, and institutions has played a central role in demographic theories.[2] For smaller than optimal populations the extent of the market is too limited to permit full benefits to be derived from division of labor and economies of scale. With too large a population diminishing returns set in, since fewer cooperating resources are available per worker. There exists, therefore, an intermediate population density which optimizes the average real income per capita.

Obviously, these more or less static considerations are not completely appropriate in a dynamic setting. However, in many underdeveloped regions, actual population numbers are so far out of balance with existing arable land resources, given their almost static techniques and their essentially fixed endowments of other cooperating factors, that the population-resource ratio may actually exert some practical influence upon reproductive behavior. Admittedly, population density is only a very crude index of the extent of population pressure upon available resources. In and of itself, a high (or low) ratio of people to land is not synonymous with over- (or under-) population. For it fails to take account of nonland resources and makes no allowance for differences in natural soil fertility, climate, crops and techniques, and the related institutional arrangements. A more refined measure of the population-land ratio, such as the number of people per square mile of cultivable or of arable land, would still be very far from the mark. Nonetheless, some indication of the influence of the population-resource ratio upon fertility may be obtained by including the average population density in the regression equations.

The decline in mortality among infants and young children has sometimes been advanced as another factor responsible for the decrease in fertility [17, pp. 81]. To the extent that families strive towards some optimal size, a greater rate of survival among the young may cause couples to limit the number of children born. In addition, the level of infant mortality may also serve as an index of the general social situation, insofar as the latter bears upon attitudes and practices related to childbearing. For these reasons infant mortality was incorporated into our preliminary regression analysis. Moreover, since infant mortality is highly correlated with per capita income, the partial regression coefficient of age specific birth rates upon infant mortality was not statistically significant and fluctuated in direction. This variable was therefore eliminated from the ultimate set of equations.

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[2] For a more detailed discussion of this point, see [9, pp. 171-91].

The final factor whose influence upon fertility was tested in our investigation is the percentage rate of growth of real per capita income. Leibenstein [9, pp. 173-75] argues that this variable has a direct effect upon the rate of population growth since, at a given level of per capita income, a higher rate of growth implies a higher percentage of investment goods and a lower percentage of consumer products. According to his theory, this shift in the composition of output leads to higher mortality and (perhaps) lower birth rates than would otherwise prevail. This variable, however, was ultimately dropped from the fertility study, as some of the earlier calculations indicated that it would not be statistically significant in the form of the regression equations chosen for our analysis.[3]

B. *The Regression Model*

From preliminary least-squares regression calculations using the indicators suggested by the discussion of the last section, it was found that the highest coefficients of determination and most consistent regression coefficients were obtained by using regression equations of the form:

$$(1) \quad \log_e b_i = a_{0i} + a_{1i} \log_e Y + a_{2i} \log_e I + a_{3i} \log_e E + a_{4i} \log_e P + u_i.$$

In these equations, b represents the number of live births per 1000 females in the ith age group, Y stands for the level of real national income per capita in 1953 U.S. dollars, I indicates the per cent of the labor force employed outside of agriculture, E is the index of education, P is the number of inhabitants per square mile, and u_i is a random disturbance term. Since, in (1), all the socioeconomic variables are expressed in logarithmic terms, the regression coefficients a_{ji} ($j = 1, 2, 3, 4$) measure the elasticities of the birth rate in the ith age group with respect to the jth variable.

In the computation of the regression coefficients from the cross-section data, it was recognized that there are significant differences in the statistical accuracy of the information available from each country. For this reason the data from each country were weighted; the weights were computed from information published in the 1956 *Demographic Yearbook* [19, pp. 9-23, 26-28]. Each weight is the product of three quantities:

1. The reciprocals of the Whipple Index. This index represents a measure of the inaccuracy of the age statistics [19, pp. 26-28].

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[3] The percentage rate of growth of real per capita income was sometimes statistically significant in some of the nonlogarithmic forms of the regression equations.

2. A measure of the birth registration procedure. This factor is 100 if births and deaths are recorded as they occur (continuous register) and 70 otherwise.

3. A measure of the completeness of the data. This factor is 100 if the data are based on a census covering more than 90 per cent of the population and is taken as 80 otherwise.

The resulting weights (available upon request from the author) vary from 28 for El Salvador and the Dominican Republic to 100 for Morocco, Algeria, and most of the European countries.

It was assumed in the calculations that the u_i, corrected by weighting for intercountry differentials in the quality of the statistics, are normally distributed around the regression surface defined by equation (1) with zero mean and constant variance. Therefore a_{ji} are unbiased estimates of the regression coefficients.

The validity of the cross-country approach was tested by computing separate regression equations—one for the developed lands and one for the undeveloped ones. No statistically significant differences in regression coefficients emerged, a result which indicates that the basic postulate of the cross-section technique—homogeneity of population response—is satisfied.

C. *The Regression Results*

The regression results derived with the aid of the statistical model described in the previous section are summarized in Table 1. The upper entry in the *i*th row and the *j*th column of this table is an unbiased estimate of the regression coefficient a_{ji}. The standard error of this estimate is listed in parentheses below the corresponding coefficient.

It is apparent from the last two columns of this table that our regression model accounts for roughly 50 to 70 per cent of the total variance in age specific birth rates among countries. The values of R^2 are all statistically significant, and the standard errors of estimate (column 6) are satisfactorily small. In addition, the signs of the individual regression coefficients are all internally consistent and in accord with a priori expectations.

A closer look at the table reveals that our results support the hypothesis that, *ceteris paribus*, age specific birth rates tend to vary directly with per capita income in the long run. Since the sign of the partial regression coefficient of births upon income is consistently positive, this conclusion appears to be valid even though the income coefficient is significantly different from zero for only the first two age groups. The dependence of births upon income, however, is not very strong even in the two lowest age groups; in fact, the calculated income elasticity never exceeds .55.

TABLE 1—AGE SPECIFIC BIRTH RATE REGRESSIONS

Age of Mother	ln Y	ln I	ln E	ln P	Constant	S	R^2	$\bar{R}^2$
15–19	.553 (.207)	−.209* (.366)	−1.229 (.315)	−.180 (.057)	6.896	.094	.497	.434
20–24	.202 (.086)	−.086* (.152)	−.533 (.125)	−.079 (.024)	6.782	.040	.527	.468
25–29	.075* (.065)	−.110* (.116)	−.332 (.100)	−.044 (.018)	6.765	.030	.480	.415
30–34	.061* (.074)	−.150* (.130)	−.380 (.112)	−.045 (.020)	6.892	.034	.513	.452
35–39	.089* (.092)	−.284 (.163)	−.490 (.141)	−.047 (.025)	7.213	.042	.530	.472
40–44	.084* (.130)	−.559 (.230)	−.540 (.198)	−.070 (.036)	7.622	.060	.507	.445
45–49	.268* (.197)	−1.182 (.350)	−1.472 (.301)	−.057 (.054)	10.816	.091	.700	.663

* Not significant at the 5 per cent level.

Symbols:

Y = per capita income, converted into U.S. dollars at purchasing parity exchange rate.
I = per cent of labor force employed outside of agriculture
E = an index of education
P = population density
Dependent variable is the natural log of the number of live births per 1000 females in age group specified.

These results of our regression analysis are consistent with the findings of Easterlin [4] for the United States from 1870 to 1960. Easterlin observed a positive correlation between long swings in white urban births and Kuznetz cycles in nonagricultural economic activity. He also found that the amplitude of the long waves in birth rates is much smaller than that of the corresponding long cycles in economic indicators.

Our findings are also consistent with the short-run results of Becker, whose estimates of average income elasticities for the short business cycles for first- and second-order births were .56 and .42, respectively [24, pp. 224-25].

There appears to be a systematic variation in income effects with age. The impact of income upon births, which tends to be strongest for women between 15 and 19, declines with age for a while and then rises again from age 35. This pattern of variation suggests the hypothesis that both desired family size and the timing of childbearing tend to react to economic factors. At the early ages, there is a strong tendency to adjust the family size to short-run variations in family income by temporarily postponing or accelerating childbearing in response to changes in economic conditions. As would-be mothers age, however, the strength of the timing effect diminishes, leading to a declining income elasticity to about age 34. The rising income elasticity at higher ages, on the other hand, represents primarily the increasing influence of the "permanent" effect of changes in per capita income.

As expected, the socioeconomic phenomena associated with the urbanization process tend to reduce birth rates in the long run. It is apparent from column 2 of Table 1 that the age specific elasticity of birth rates with respect to nonagricultural employment is negative throughout. It is not statistically significant up to age 35. The calculated values, however, decline to age 25 and then begin to rise; the elasticity is greater than unity for the 45-49 age group. The progressively stronger impact of industrialization upon age specific birth rates is thus quite marked. A reasonable interpretation of this pattern is that the reduction in crude birth rates which has historically accompanied the process of urbanization has been achieved, in part, by a shortening of that portion of the life span devoted to childbearing activities by an average woman. Further, the strongest effect appears among the older women.

The well-known negative correlation of birth rates with the level of education is also apparent in our results. The regression coefficient of birth rates with respect to the educational index is always negative and statistically significant. Furthermore, as indicated by the analogous age specific pattern of variation of the relevant elasticities, the impact of the level of education upon fertility is qualitatively similar to that of the degree of industrialization. Quantitatively, among all the variables, a 1 per cent change in the index of education appears to exert the largest absolute influence upon age specific birth rates.

In view of the rough degree of approximation to which the population density figure represents the theoretical concept of the population-resource ratio, the unequivocal significance of P in the regression analysis is somewhat surprising. To a certain extent, as indicated by the negative sign of the regression coefficient in column 4, overpopulation tends to generate its own antidote. However, the over-all elasticity of birth rates with respect to population density is rather small.

The regression equations for age specific birth rates for the subsample of developed countries taken by themselves are reproduced in Table 2 for comparison purposes. Since for this subsample the correlation coefficient between the logarithms of per capita income and of the degree of industrialization was quite high ($r = .81$), the latter variable had to be omitted from the regression equations. As stated above, the

TABLE 2—AGE SPECIFIC BIRTH RATE REGRESSIONS
DEVELOPED COUNTRIES

Age of Mother	ln Y	ln E	ln P	Constant	S	R^2
15–19	.703 (.224)	−.965 (.523)	−.127* (.077)	3.502	.099	.617
20–24	.179* (.112)	−.227* (.262)	−.809 (.038)	5.179	.050	.519
25–29	.175 (.090)	−.121* (.210)	−.050* (.030)	4.678	.040	.216
30–34	−.0051* (.094)	−.282* (.219)	−.045* (.032)	6.519	.042	.206
35–39	−.0015* (.104)	−.519 (.243)	−.042* (.136)	6.651	.046	.284
40–44	−.0392* (.148)	−.735 (.345)	−.060* (.051)	6.799	.065	.286
45–49	−.0923* (.214)	−1.212 (.501)	−.081* (.074)	6.897	.095	.327

* Not significant at the 5 per cent level.
Symbols:
Y = per capita income, converted into U.S. dollars at purchasing parity exchange rates
E = an index of education
P = population density
Dependent variable is the natural log of the number of live births per 1000 females in age group specified.

results of the regression for the developed subsample are consistent with those for the combined group of countries. Indeed, in no case is the difference in regression coefficients significant at the 5 per cent level. In developed countries, the influence of income levels upon births seems to decline more rapidly with age, and becomes negative from the age of 30 onward. The educational level exercises its primary impact at early ages and from 30 onwards, as before. And the impact of population density is statistically less significant for the developed subgroup than for all countries combined. In addition, R^2 declines more rapidly with age in the developed subsample than for the entire group of countries.

IV. *The Analysis of Mortality*

A. *Determinants of Mortality*

There is general agreement among demographers that the determinants of changes in mortality in both developed and underdeveloped areas are of two kinds: (1) those related to the standard of living of the population, and (2) those associated with public health programs for the control of epidemic and endemic diseases. The relative importance of these two classes of factors in both the short and the long run, however, and the extent to which they are interdependent are still unclear.

For example, in discussing recent trends and determinants of mortality in developed countries Stolnitz argues that, "To a significant degree, the great mortality movements of the West appear to have been initiated by medical advances, particularly as applied to governments. Improving economic conditions were important, but much more as permissive elements than as precipitating factors" [16, p. 34]. Similarly, with respect to underdeveloped areas, Bourgeois-Pichat and Chia-Liu Pau point out that, "In the short run the steep mortality decline during the postwar period in, for example, Ceylon, Formosa and Japan stressed that the drop was attributable more to the development of public health programs with a wide use of new drugs than to any possible rising level of living of the people" [16, p. 25].

Clearly, the immediate impact upon death rates in underdeveloped countries of such public health measures as the control and purification of the water supply, the disposal of sewage, the use of new drugs, large-scale innoculations against communicable diseases, and progress in environmental sanitation is bound to be dramatic. On the other hand, once the major benefits from these improvements have been reaped, it may well be that economic conditions play the primary role in determining the subsequent rate of progress in mortality. For it stands to reason that such factors as better nutrition, improved housing, healthier and more humane working conditions, and a somewhat more secure and less careworn mode of life, all of which accompany economic growth, must contribute to improvements in life expectancy. In addition, as pointed out by Spiegelman, "Fundamentally, health progress depends upon economic progress. By the rapid advance in their economies in the postwar period, the highly developed countries have produced wealth for the development of health programs. Also, more efficient technologies in industry are releasing the manpower needed for an extension of medical care and public health services. The intangible contribution of economic progress to lower mortality is derived from the advantage of a high standard of living—more abundant, better and more varied food—a more healthful work and home environment—and more time for healthful recreation" [16, p. 59].

In our study of mortality, the indicator of public health conditions used was the number of physicians per thousand inhabitants. It might be argued that the sum of real per capita expenditures on public health and social security would have been a preferable criterion. However, as pointed out by the United Nations [18, p. 489], the significance of this item in the total health picture varies from country to country depending upon the type of state organization and the scope of the government's share in economic activity. In socialist economies and in centralized states, for example, the national government is almost completely responsible for the health of the public. In private enterprise economies, on the other hand, the degree of public responsibility for health assumed by the government varies significantly from country to country and, in some, constitutes only a minor share of the total. The number of physicians per capita was therefore considered to be a more satisfactory indicator.

As before, economic conditions were represented in our mortality analysis by the level of real per capita income. The percentage of nonagricultural employment of the labor force was used as a measure of the degree of industrialization and urbanization. And the percentage rate of growth of real per capita income was introduced in order to investigate the empirical relevance of one of Leibenstein's population theories [9, pp. 173-75].

The same considerations which suggested the incorporation of population density into the birth rate analysis also led us to test the effects of this variable upon mortality. To our surprise, however, the partial regression coefficient of age specific death rates upon population density was never statistically significant in either the logarithmic or the nonlogarithmic forms of the equations calculated. This variable was therefore dropped.

The influence of the degree of education upon mortality patterns was also investigated by means of an index of education analogous to that used in the fertility study. This factor might be expected to affect especially postneonatal mortality for infants and of young children because survival probabilities at early ages are largely affected by the socially and educationally conditioned techniques which mothers employ in the feeding and care of babies. Indeed, our regression results indicate that, in the logarithmic form of the equations, the partial regression coefficient of death rates with respect to the educational index E was statistically significant up to the age of 25. Moreover, in accord with expectations, the effect of educational forces upon mortality after the first five years was found to diminish steadily with age. It was observed in the preliminary calculations that E and the health indicator H were never both statistically significant for the same regression surface, except in the case of infant mortality. Since H is the better indicator for most of the age groups, and since it was felt desirable to have a uniform set of explanatory variables for mortality at all ages, the number of physicians per capita (H) was used instead of the educational index (E) throughout the entire mortality calculation.

B. *The Regression Model*

The regression equations fitted by least squares to the data were of the form:

$$(2)\quad \log_e m_i = c_{0i} + c_{1i}\log_e Y + c_{2i}\frac{\Delta Y}{Y} + c_{3i}\log_e I + c_{4i}\log_e H + \delta_i.$$

In (2) m_i represents the number of deaths per 1000 people in the ith age group; Y and I have the same meanings as in the birth rate equations; $\Delta Y/Y$ is the percentage rate of growth of per capita real income; H is our health indicator—the number of physicians per 10,000 inhabitants; and δ_i is a random error term. The variable $\Delta Y/Y$ was not represented in logarithmic form since, for some countries, it takes on zero or negative values.

As before, in order to achieve a homoscedastic distribution of the δ_i, the least-square fits were computed with country weights derived in the same manner as in the birth rate analysis.

Nonlogarithmic forms of the regression equations with nonlinear terms in income were also fitted to the data. The nonlogarithmic alternatives resulted in slightly poorer fits than (2) up to the age of 40 and somewhat better fits afterwards. The decision to adopt the logarithmic version was made primarily for consistency with the birth rate study.

Disaggregation of the sample into developed and underdeveloped subgroups led to no significant differences in regression coefficients. This time, however, the values of R^2 for the underdeveloped countries were significantly higher than for the developed ones. This observation tends to bear out the contention of Ansley Coale that, "The course of mortality in *industrialized* areas can safely be discussed without reference to prospective variation in strictly economic forces" [24, p. 9].

C. *The Regression Results*

Table 3 presents a summary of the regression results derived with the statistical model just described. As before, the upper entry in the ith row and the jth column of this table is an estimate of the regression coefficient a_{ji}. The standard error of this estimate is listed in parentheses below the corresponding coefficient.

It is evident from the last 2 columns of the table that, up to the age of 50, medical and socioeconomic forces account for something like 50 to 85 per cent of the total variance among countries in age specific death rates. Up to the age of 70, the values of R^2 are all statistically significant. In addition, the signs of the individual regression coefficients are all internally consistent.

TABLE 3—AGE SPECIFIC DEATH RATE REGRESSIONS

Age Group	ln Y	$\Delta Y/Y$	ln I	ln H	Constant	S	R^2	$\bar{R}^2$
0–1	−.275 (.116)	+.025* (.030)	−.664 (.255)	−.149* (.129)	8.417	.0571	.686	.643
1–4	−.583 (.034)	−.035 (.008)	1.284 (.074)	−.311 (.037)	10.681	.0166	.857	.838
5–9	−.420 (.138)	−.033* (.035)	−.610 (.303)	−.336 (.153)	5.776	.0679	.721	.683
10–14	−.304 (.089)	−.065 (.023)	−.713 (.196)	−1.57* (.099)	4.916	.0437	.771	.740
15–19	−.433 (.092)	−.093 (.024)	−.476 (.203)	+.044* (.103)	5.014	.0455	.653	.606
20–24	−.372 (.105)	−.043 (.026)	−.362* (.231)	−.203 (.117)	4.808	.0519	.702	.661
25–29	−.341 (.103)	−.045 (.026)	−.637 (.225)	−.135* (.114)	5.738	.0504	.717	.678
30–34	−.274 (.100)	−.045 (.026)	−.615 (.220)	−.178* (.111)	5.470	.0493	.699	.658
35–39	−.240 (.098)	−.050 (.025)	−.514 (.214)	−.198 (.108)	5.175	.0481	.670	.625
40–44	−.137 (.098)	−.047 (.025)	−.528 (.214)	−.175* (.109)	4.878	.0484	.562	.502
45–49	−.207 (.097)	−.038* (.025)	−.185* (.211)	−.185 (.106)	4.252	.0472	.478	.372
50–54	−.065* (.090)	−.031* (.023)	−0.220* (.198)	−.198 (.100)	3.976	.0448	.444	.368
55–59	−.017* (.080)	−.016* (.020)	−.144* (.175)	−.194 (.088)	3.717	.0393	.358	.270
60–64	−.031* (.066)	−.015* (.016)	−.114* (.146)	−.170 (.072)	4.074	.0324	.410	.329
65–69	−.0008* (.085)	−.004* (.022)	−.060* (.187)	−.170 (.095)	4.082	.0422	.255	.153
70–74	−.048* (.066)	−.002* (.016)	+.008* (.146)	−.098 (.072)	4.409	.0323	.199	.089*

* Not significant at 5 per cent level.

Symbols:

Y = per capita income, converted into U.S. 1953 dollars at purchasing parity exchange rates

$\Delta Y/Y$ = the percentage rate of growth of per capita real income

I = the percentage of the labor force employed outside agriculture

H = health indicator, the number of physicians per 10,000 inhabitants

Dependent variable is the number of deaths per 1000 people in the appropriate age group.

One interesting feature of our calculations is the steadily diminishing importance of economic, social, and medical factors in explaining inter-country death rate differentials at higher ages. (From the age of 35 onwards, the values of R^2 decrease continuously.) Similar results were obtained by Moriyama and Guralnick [16, pp. 61-73] in a cross-sectional study of differences in mortality between occupational and social strata in the United States; they found that the gap in mortality rates between socioeconomic classes narrows with age, especially after 55. This observation may be related to some suggestions from recent medical research that the physiological quality of human organisms is largely determined at a very early stage in life. Economic and social conditions during early childhood, therefore, may well play a more significant role in determining differences in mortality rates after age 55 than do current variations in socioeconomic and medical factors.

A more detailed examination of our regression results leads to the following generalizations:

1. *Ceteris paribus,* there exists a negative long-run association between death rates and economic conditions. Column 1 of Table 3 indicates that the regression coefficient of deaths upon income is negative throughout and statistically significant up to the age of 50. The elasticity of death rates with respect to income is considerably below unity, however. These results are in accord with the conclusions of Moriyama and Guralnick in the study previously cited.

2. Urbanization and industrialization seem to play a significant direct role in the reduction of mortality (col. 3). Much of the apparent effect of urbanization may be due to differences between city and country in environmental sanitation (water supply, sewage, etc.), type of housing, and availability of professional (as opposed to traditional) medical care and the willingness of the population to utilize it. In addition, differences in occupational structure and in per capita income between urban and rural communities may also contribute to the negative correlation between mortality rates and the percentage of nonagricultural employment.

3. As expected, mortality is negatively associated with differentials in medical care (col. 4). Indeed, from the age of 50 onwards, variations in mortality are largely accounted for by differences in medical services. None of the other socioeconomic factors is statistically significant after that age.

4. Finally, our results also indicate the existence of a negative partial correlation between the rate of growth of real per capita income and death rates (col. 2). This finding is in direct conflict with that theory of Leibenstein which suggests that, *ceteris paribus,* a more rapid rate of economic growth will, on the average, be correlated with an *increase* in mortality [9, pp. 173-75].

The explanation of the latter phenomenon may be much more classical in nature than Leibenstein's argument would indicate. As pointed out by Adam Smith, "It is not the actual greatness of national wealth, but its continual increase, which occasions a rise in the wages of labour. It is not, accordingly, in the richest countries, but in the most thriving or in those which are growing rich the fastest that the wages of labour are highest" [14, p. 69]. Now, at a given level of income and within a given sociocultural context, the over-all death rate of the population might be expected to vary with the distribution of income between classes. Even when the over-all average standard of living is low, the per capita income of profit receivers is so high that mortality rates within the entrepreneurial classes are not particularly sensitive to changes in income. In contrast, at low levels of income, a higher wage rate reduces mortality among the laboring population through the better nutritional, health, and sanitation standards that accompany improved wages. As a result, an increase in the rate of growth of income, which augments the relative share of wages in the total product by increasing demand for labor, diminishes death rates. By the same token, by increasing the share of profit receivers, a decline in the rate of economic expansion leads to an increase in mortality.[4]

The flaw in Leibenstein's contention would appear to be that, even though a higher rate of growth of per capita income is associated with a decrease in over-all consumption, it is also associated with an increase in the amount of consumer products accruing to the lower income groups. This decreases mortality among the laboring classes without a corresponding increase in the mortality rate of upper income groups. The result is, of course, a net reduction in mortality for the community as a whole.

V. *Estimates for 1953*

To obtain a partial indication of the usefulness of the above regression approach to natality and mortality phenomena, the regression coefficients of Tables 1 and 3 were used to estimate the crude birth and death rates in 1953 in the six inhabited continents. The year 1953 was used as a bench-mark date because good estimates of per capita income were available for that year and because empirical demographic data for 1950-55 have been published by the United Nations. In addition, for this period, the socioeconomic regression estimates can also be compared with estimates of crude birth and death rates computed by the United Nations Population Division on the basis of purely demographic phenomenological models of population growth [20] [21] [22].

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[4] A similar argument is made in I. Adelman [1, pp. 110-11].

Before our regression analysis can be used for this purpose, however, the values of the independent variables in our equations have to be estimated. For North America, Oceania, and Europe, our estimates are the arithmetic averages of the single country data for 1953, weighted by population; but for Latin America, Asia, and Africa, statistics are notoriously scanty. Our estimates for these regions were therefore necessarily based upon fragmentary data, and are consequently much less reliable. Also, in the absence of better information, a uniform figure of 2 per cent was chosen for the rate of growth of real per capita income in order to avoid a suggestion of spurious accuracy. The values of the independent variables assumed for the 1953 estimates are given in Table 4.

TABLE 4—VALUES OF INDEPENDENT VARIABLES ASSUMED FOR 1953 ESTIMATES

Region	Population Density P^a	Per Capita Income Y	% Rate of Growth of Per-Cap. Income $\Delta Y^c/Y$	Physicians per 10,000 Population H	Educational Index E	% of Labor Force Outside Agriculture I
North America	7.8	1800^c	2.0	12.5^c	94^c	87^c
Latin America	8.1	265^d	2.0	2.0^f	30^f	40^f
Asia	51^b	75^d	2.0	1.0^f	20^f	30^f
Europe	59	770^c	2.0	11.0^c	95^c	78^c
Oceania	1.3	1120^c	2.0	12.7^c	100^c	84^c
Africa	6.5	105^d	2.0	0.5^e	20^f	35^f

Sources: a *Demographic Yearbook* 1956, p. 151. Population per km² in 1950.
b Corrected in the light of later estimates, back to 1950.
c Averages, weighted by population, of data for 1953 for countries in the region.
d Estimates taken from T. Kristensen and Associates, *The World Economic Balance* (North Holland, 1960), p. 250.
e Hypothetical figure.
f Estimate based on fragmentary data.

The resulting age specific birth and death rates are indicated in Tables 5 and 6 respectively. Since, from the age of 70 onwards, our death regressions were not statistically significant, the United Nations model mortality tables were used to estimate death rates for the uppermost age groups [22, pp. 72-73]. The pattern of birth and death rates estimated is consistent with later empirical data and a priori expectations. In our estimates, birth rates are lowest in Europe, higher and fairly similar in North America and Oceania, and higher still in Asia, Latin America, and Africa. Death rates in North America and in Oceania are somewhat lower than those in Europe. They are high in both Africa and Asia, and intermediate in Latin America. In accord with observation, Europe, in our estimates, appears to be in a low-birth-and-low-death-rate demographic state; North America and Oceania are of a low-death-rate and intermediate-birth-rate type; and Latin America is in a transition phase, with both mortality and natality intermediate between those of the Asian and African continents and the much lower birth and death rates of the more developed continents. In addition, the age specific pattern of variation of both birth and death rates in each continent is consistent with that found in the real world.

TABLE 5—AGE SPECIFIC BIRTH RATES ESTIMATED FOR 1953

Age of Mother	Asia	Africa	Latin America	North America	Oceania	Europe
15–19	66	111	106	64	63	28
20–24	234	291	274	206	209	149
25–29	256	283	259	188	193	163
30–34	206	226	199	129	133	113
35–39	145	158	134	73	74	63
40–44	66	72	57	23	25	20
45–49	28	28	17	2	2	2

TABLE 6—ESTIMATED AGE SPECIFIC DEATH RATES (1953)

Age Groups	North America	Africa	Latin America	Asia	Europe	Oceania
0–1	21.4	138	120.0[a]	152	29.6	24.9
1–4	.76	34.8	11.1	41.5	1.49	1.04
5–9	.36	6.17	2.42	6.19	.58	.45
10–14	.34	2.57	1.42	2.85	.49	.40
15–19	.65	2.98	1.99	3.82	.98	.81
20–24	.82	6.32	3.22	6.58	1.20	.99
25–29	.91	6.62	3.68	7.45	1.33	1.09
30–34	1.14	7.69	4.30	8.20	1.57	1.32
34–39	1.62	9.65	5.49	9.88	2.15	1.84
40–44	2.59	10.90	7.03	11.0	3.16	2.82
45–49	3.78	14.60	9.12	14.2	4.71	4.19
50–54	6.98	19.40	13.50	17.9	7.76	7.24
55–59	11.30	25.20	18.60	22.8	11.90	11.40
60–64	17.70	37.10	28.00	33.9	18.88	18.0
65–69	27.2	49.75	39.00	44.6	30.00	27.2
70–74	46.10	72.10	65.00	68.4	48.6	47.0
75–79[a]	74	118	99	108	77	75
80–84[a]	118	176	152	163	122	118
85[a]	232	277	255	265	234	232

[a] Estimated on the basis of U.N. model mortality tables, *Methods for Population Projection by Sex and Age, Manual III*; U.N. 1956, pp. 72–73.

Our estimates of the crude birth and death rates for each continent in 1953 are summarized in Table 7. These estimates are based upon population age distributions derived by interpolation from the actual data for age groups 0-14, 15-64, 65 and over listed in the 1956 *Demographic Yearbook.* The ratio of females to total population used for these estimates was calculated from individual country data on sex distribution by age given in the 1957 *Demographic Yearbook.* From Table 7 one can see that our birth rate estimates are close to the actual figures and compare very favorably with those of the United Nations. Our death rate estimates are less satisfactory. They are still considerably better than the U.N. estimates, but they tend to be low. Since crude death rates are quite sensitive to the details of the age distribution of the population, especially at ages above 45, a large part of the discrepancy between our calculations and empirical observations can be ascribed to inaccuracies and approximations in our assumed age structure. This hypothesis is supported by the fact that life expectancy at birth (see Table 8), which is independent of age structure, is consistent with observation.

One would expect, of course, that even better agreement with real world birth and death rates could be achieved if crude vital statistics were estimated from the regression equations country by country and averaged (using relative populations as weights) for each continent. Unfortunately, however, the scarcity of data, especially in the underdeveloped regions, precluded this approach.

TABLE 7—COMPARISON OF BIRTH AND DEATH RATE ESTIMATES 1950–55

Continent	Crude Birth Rate			Crude Death Rate			Natural Increase		
	Actual*	Our Estimate	U.N.** Estimate	Actual*	Our Estimate	U.N.** Estimate	Actual*	Our Estimate	U.N.** Estimate
	1951–55	1953	1950	1951–55	1953	1950	1951–55	1953	1950
Africa	45	43	47	25	20	33	20	23	14
North America	25	23	22	9	9	0	16[a]	14	13
Latin America	42	41	40	18	13	19	24[a]	28	21
Asia	39	39	46	22	19	33	17	20	13
Europe[b]	20	21	20	11	9	9	9[a]	12	11
Oceania	25	23	26	8	9	12	17	14	14

* Source: *Demographic Yearbook* 1956, Table A, p. 2.
Rates are estimated on annual averages for 1951–55.

[a] This rate excludes the effect of migration.

[b] Excluding USSR.

** Source: *The Future Growth of World Population*, U.N. 1958, Table 11, p. 32.

TABLE 8—COMPARISONS OF LIFE EXPECTANCY AND REPRODUCTION RATE ESTIMATES, U.N. AND OURS

Continent	Life Expectancy at Birth			Gross Reproduction Rate			NRR
	U.N. Estimate 1950	Our Estimate 1953[i]	Actual[g]	U.N. Estimate 1950	Our Estimate 1953[i]	Actual[g]	Our Estimate[1]
Africa	31.5[a]	46.5	49	3.0[a]	2.89	h	2.33
North America	68.2[b]	71.9	71	1.5[b]	1.70	1.74	1.66
Latin America	46.8[c]	56.7	50	2.75[c]	2.59	3.08	2.31
Asia	32.2[d]	45.2	h	2.92[d]	2.47	h	1.95
Europe	68.2[e]	69.9	66	1.29[e]	1.33	1.22	1.29
Oceania	68.2[f]	71.2	69	1.5[f]	1.73	1.59	1.69

[a] Source: *The Future Growth of World Population* (p. 12), Model AB for Middle Africa and Models CG and DH for Northern and Southern Africa.

[b] *Loc. cit.*, Model LMS.

[c] *Loc. cit.*, Model DH for Central, Tropical and Southern America and Model LMS for Temperate America.

[d] *Loc. cit.*, Model CG for Asia excluding Japan and Model LRVW for Japan.

[e] *Loc. cit.*, Model QRS for Northern and Central Europe and average QRS and LRS Models for Southern Europe.

[f] *Loc. cit.*, Model LMN.

[g] Estimated from available country data on or about 1953 in *Demographic Yearbook*. Averages weighted by population. Figures are rounded to nearest year.

[h] Sample available too unrepresentative to use.

[i] For this computation the regression death rates were converted into life table mortality rates with the aid of the Reed & Merrell Tables reproduced in [7, pp. 19–26].

[1] In this computation it is assumed that .495 of all births are females.

VI. *Equilibrium Distributions and Demographic Multipliers*

A. *Some General Considerations*

One important problem which our regression analysis enables us to attack is: how sensitive is the rate of population growth to changes in each of the socioeconomic variables? In attempting to answer this question, one cannot simply compare elasticities. For a given percentage change in one of the independent variables does not, in general, have the same a priori probability as the same percentage change in another independent variable. One must instead compute the effect on population growth of each of a set of equiprobable percentage changes in the independent variables in order to achieve a more or less objective intervariable comparison.

A more basic difficulty arises when one tries to determine the relevant whole-population elasticities (elasticities averaged over all age groups), as the averaging process depends of necessity on the age distribution assumed for the population. One can bypass this problem by rephrasing the question in a manner similar to that used in comparative statics analysis. Thus we shall not ask: what changes in the *actual* demographic features of a community will be brought about by a given percentage variation in a particular socioeconomic quantity? Instead, we shall compare the ***equilibrium*** configurations which correspond to the age specific birth and death rates of the society before and after the change.

This alternative approach leads us to the calculation of demographic multipliers, conceptually analogous to the more familiar multipliers of modern economics. These demographic multipliers indicate the differences in the equilibrium demographic features of a society before and after a change in the value of some particular social or economic variable. A demographic multiplier is derived by (1) calculating the equilibrium age distribution corresponding to a certain set of initial

TABLE 9—EQUILIBRIUM DEMOGRAPHIC FEATURES BY CONTINENT

Continent	Conditions	Age Distribution %			Dependency	Crude Birth Rate	Crude Death Rate[c]	Natural Increase[d]
		0–14	15–60	>60				
Africa	Actual '53[a]	42	52	6	.92	45	25	20
	Equilibrium	46	50	4	1.00	42	11	30
	Regression Est. 1953	—	—	—	—	43	20	23
Asia	Actual '53[a]	38	56	6	.78	39	22	17
	Equilibrium	41	53	5	.86	36	13	23
	Regression Est. 1953	—	—	—	—	39	19	20
Latin America	Actual '53[a]	40	55	5	.82	42	18	24
	Equilibrium	44	51	5	.94	38	8	30
	Regression Est. 1953	—	—	—	—	41	13	28
North America	Actual '53[a]	27	61	12	.64	25	9	16
	Equilibrium	33	57	10	.75	25	7	19
	Regression Est. 1953	—	—	—	—	22	9	14
Oceania	Actual '53[a]	30	59	11	.69	25	8	17
	Equilibrium	34	56	10	.78	26	7	19
	Regression Est. 1953	—	—	—	—	23	9	14
Europe[b]	Actual '53[a]	25	62	13	.61	20	11	9
	Equilibrium	27	59	15	.70	19	10	9
	Regression Est. 1953	—	—	—	—	21	9	12

[a] Source: *Demographic Yearbook* 1956, p. 8, Table B.

[b] Excluding USSR.

[c] Calculated by the method given in [25, p. 32].

[d] Excluding the effect of migration.

values of those socioeconomic variables which determine the age specific birth and death rates; (2) changing one of our independent variables, such as per capita income, by a certain percentage; (3) calculating the new age specific birth and death rates; (4) calculating the new equilibrium age distribution; and (5) comparing the demographic characteristics of the two equilibria.

Before these demographic multipliers can be calculated, therefore, we must derive the equilibrium configuration implied by a given pattern of age specific birth and death rates. The equilibrium age distribution of a society is that age distribution which would ultimately tend to establish itself under unchanging birth and death conditions [3, pp. 236-51].[5] It can be shown that, if the fertility of women at each age were to remain constant for a sufficient length of time, the age structure of the population would assume a definite pattern which is independent of the initial age distribution of the population. In addition, it can also be proved that, given a set of fertility and mortality rates, the equilibrium configuration is stable with respect to large displacements as well as to small. The nature of the implied equilibrium age distribution is therefore of demographic interest both intrinsically and as a means of obtaining a better representation of the inherent power for growth of a population. From an economic point of view, the equilibrium age distribution is also of interest, as such important features of an economy as its consumption pattern, the savings tendencies of the community, the attitudes of the society towards risk, the labor-force participation rate, and the average productivity of the employed are all age-dependent phenomena [24, pp. 287-522].

B. *Equilibrium Distributions and Multipliers*

Some of the significant demographic features of the calculated equilibrium distributions are given in Table 9 for the six continents. For each continent the predicted age configuration in equilibrium consists of a larger proportion of younger people than was observed in 1953, and, except possibly for Europe, it implies a reversal of the current tendencies towards an older population. Since the decrease in the aged is more than counterbalanced by the increase in the proportion of children, the economic burden upon the working population rises in equilibrium. In Table 9 this effect is measured by the dependency ratio, an indicator which is obtained by dividing the proportion of the population under 15 and over 59 by the working age population. These projected changes are obviously related to the dramatic difference in death rates between the actual and the predicted populations.

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[5] This equilibrium age distribution is identical with the "stable age distribution" first introduced by Lotka in 1911 [10]. The approximation technique suggested [3, pp. 240-42] was used for its calculation.

The demographic multiplier calculations summarized in Tables 10 and 11 were carried out for four continents, chosen to represent four different demographic situations. North America is an example of a high-birth-rate and low-death-rate society. Asia represents a community with both birth and death rates high. Europe is more or less typical of a low-death-rate and low-birth-rate region; and Latin America has an intermediate death rate and a high birth rate. The changes for which the multiplier calculations were carried out were of a magnitude which might be expected to result from a reasonably successful 5-year development program. Inasmuch as neither the literacy index nor the degree of nonagricultural employment in Europe and North America is likely to change very much, no educational or industrialization multipliers were calculated for these continents. In addition, it was not obvious what assumption could reasonably be made about changes in the number of physicians per capita and changes in population density. Therefore, no multipliers were calculated for these variables.

From Tables 10 and 11 it would appear that the demographic characteristics of a society are most sensitive to changes in the educational level of the population. Some of the demographic indicators, such as the rate of natural increase, also respond noticeably to changes in per capita income. The net effect of changes in industrialization per se or in the rate of increase of per capita income is, in general, quite small. On the other hand, the degree of industrialization has a significant influ-

TABLE 10—DEMOGRAPHIC MULTIPLIERS BY CONTINENT

Continent	Demographic Quantity	Equilibrium Value Based on 1953 Data	Effect of 25% Increase in			Effect of Change in $\Delta Y/Y$ from 2% to 4%
			Y	I	E	
North America	Birth Rate (%)	2.55	+.12	*		0
	Death Rate (%)	.69	−.04	*	*	0
	Rate of Natural Increase (%)	1.86	+.16	*	*	0
	Doubling Period (years)	37.61	−2.95	*	*	0
	Life expectancy (years)	71.88	+.34	*	*	+.31
Europe	Birth Rate (%)	1.92	+.08	*	*	0
	Death Rate (%)	1.03	−.05	*	*	−.01
	Rate of Natural Increase (%)	.89	+.14	*	*	+.01
	Doubling Period (years)	78.23	−10.59	*	*	−.87
	Life Expectancy (years)	69.94	+.43	*	*	+.35
Latin America	Birth Rate (%)	3.82	+.15	−.14	−.47	0
	Death Rate (%)	.78	−.05	−.05	+.09	−.02
	Rate of Natural Increase (%)	+3.03	+.21	−.08	−.55	+.02
	Doubling Period (years)	23.22	−1.48	+.62	+5.07	+.15
	Life Expectancy (years)	56.72	+1.08	+2.45	*	+.47
Asia	Birth Rate (%)	3.60	+.12	−.14	−.41	0
	Death Rate (%)	1.27	−.09	−.12	+.20	−.05
	Rate of Natural Increase (%)	+2.33	+.22	−.02	−.20	+.05
	Doubling Period (years)	30.09	−2.56	+.26	+8.13	−.62
	Life Expectancy (years)	45.17	+1.83	+4.04	*	+.97

* Not calculated.

TABLE 11— EQUILIBRIUM AGE STRUCTURE AND DEPENDENCY RATIOS BY CONTINENT[a]

Continent	Age Group	Equilibrium Value	Effect of 25% Increase in			Effect of Change in $\Delta Y/Y$ from 2% to 4%
			Y	I	E	
North America	0–14 (%)	33.14	+1.12	*	*	−.06
	15–35 (%)	31.65	+.19	*	*	−.04
	35–60 (%)	24.92	−.69	*	*	+.02
	>60 (%)	10.29	−.63	*	*	+.09
	Dependency ratio	.75	+.04	*	*	0
Europe	0–14 (%)	26.70	+.85	*	*	−.08
	15–35 (%)	29.96	+.29	*	*	−.06
	35–60 (%)	28.84	−.49	*	*	+.02
	>60 (%)	14.50	−.65	*	*	+.11
	Dependency ratio	.70	+.01	*	*	0
Latin America	0–14 (%)	43.79	+1.24	−.94	−3.86	−.08
	15–35 (%)	32.94	−.10	+.01	+.08	−.05
	35–60 (%)	18.51	−.73	+.63	+2.42	+.09
	>60 (%)	4.76	−.41	+.30	+1.36	+.04
	Dependency ratio	.94	+.03	−.02	−.09	0
Asia	0–14 (%)	41.33	+1.12	−.79	−3.46	−.05
	15–36 (%)	33.29	−.02	−.02	−.07	−.03
	35–60 (%)	19.99	−.68	+.55	+2.20	+.03
	>60 (%)	5.39	−.42	+.25	+1.32	+.05
	Dependency ratio	.86	+.04	−.003	−.06	0

[a] Percentages may not add up to 100 due to rounding.

ence upon life expectancy. Perhaps the major surprise in the results is the apparently paradoxical observation that the crude death rate increases as the educational level of the society is raised. The explanation of this effect is that, in the long run, the decreased birth rate accompanying an improvement in education leads to an older population, with its associated higher mortality rates.

VII *Conclusions*

The results of this study suggest that there is a systematic dependence of age specific birth and death rates upon some of the important socioeconomic variables. This relationship can be utilized to estimate the potential demographic changes which may be induced by national development programs. The demographic effects of changes in per capita income and in its rate of growth, however, do not appear to be very dramatic.

For economic development, the implications of this study are therefore rather encouraging. They indicate, for example, that the increase induced in the ratio of population growth by a 25 per cent improvement in per capita income would not raise the population by more than 1 per cent over a 5-year period. Furthermore, if the development program were accompanied by an effective educational effort, the rate of population expansion might actually decline.

All in all, it would seem that the influence of socioeconomic variables upon the demographic features of a society is very much smaller than the effect of population growth upon economic development.

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THE BROWN SHOE CASE AND THE NEW ANTIMERGER POLICY

By DAVID DALE MARTIN*

In June, 1962, the United States Supreme Court unanimously declared illegal the Brown Shoe Company's 1956 acquisition of the G. R. Kinney Company [3].[1] After eleven and a half years, the Supreme Court for the first time decided a case requiring a detailed analysis of the 1950 amendments of Section 7 of the Clayton Act.[2] Brown Shoe is undoubtedly one of the few truly landmark antitrust cases on close-knit combinations. The Court confirmed the fact that the Celler-Kefauver Act of 1950 constitutes a major substantive change in public policy toward the allowable degree of centralization of corporate control and not just a mere plugging of a loophole in the 1914 legislation.[3] In an opinion that liberally dispensed interpretations of the statute, the Court enunciated a policy that would have prevented the development of oligopoly structure in American industry had it been adopted in 1895, when the problem of regulating corporate mergers was temporarily relegated to the states in the E. C. Knight case [11, pp. 13-19].

A number of cases must reach the Supreme Court before enough specific criteria will have emerged for easy judgment of the legality of mergers. In this case, however, the Court greatly narrowed the area of uncertainty. The merger had vertical and horizontal characteristics, both of which the Court considered even though it might have disposed of the case on either of the two grounds alone. It also examined both the "line of commerce" and the "section of the country" questions, as

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[1] Mr. Chief Justice Warren delivered the opinion of the Court, in which Justices Brennan, Black, Douglas, and Stewart joined. Mr. Justice Frankfurter took no part in the decision and Mr. Justice White took no part in the consideration or decision of the case. Mr. Justice Harlan wrote a separate opinion dissenting on a jurisdictional question but concurring in the judgment of the Court. Mr. Justice Clark also wrote a concurring opinion.

[2] On the same day the Court denied review of the Circuit Court decision [5] upholding the Federal Trade Commission's interpretation of the statute in the Crown-Zellerbach case.

[3] For a detailed study of the origins and administration of Section 7 of the Clayton Act and its amendment, see [11].

well as the all-important issue of measuring a merger's probable competitive impact.[4] Thus, the high Court has finally spoken on many of the key merger questions that have risen and will continue to arise in the lower courts and the Federal Trade Commission.[5]

The purpose of this article is to examine from the economic standpoint the Court's treatment of the issues raised and to discuss some of the questions left unanswered. After a brief review of the findings of facts and conclusions of law by the lower court, I first consider the criteria used by the Supreme Court in defining the relevant lines of commerce and sections of the country. I then turn to the criteria used in judging the probable effect of the merger, first in its vertical and then in its horizontal aspects. The concluding section considers the significance of the new antimerger law on both the development of antitrust policy and the future of corporate concentration in the United States.

I. *The District Court Opinion*

In November, 1955, the government asked the district court for a permanent injunction of the contemplated merger and for a preliminary injunction pending the decision on the merits. The merger was effected on May 1, 1956, after Judge Hulen denied the latter motion to enjoin [15]. To facilitate eventual divestment, if it should be required, the judge did order the defendants to operate their businesses separately and to keep their assets separately identifiable.

Shoe manufacturing and distribution have not traditionally been characterized by oligopoly structure. In the district court, Brown's attorneys contended that the industry enjoyed healthy competition that would not be diminished by the merger because Kinney manufactured less than one-half of one per cent and retailed less than 2 per cent of the nation's shoes. Although Brown was the fourth largest U.S. shoe manufacturer in 1955, it produced only a little less than 4 per cent of the total shoe output of the industry [14, p. 728].

Of the total 1957 national production, the largest 24 companies together accounted for only about 35 per cent, the four largest companies accounting for about 23 per cent. International, the largest producer, was followed by Endicott-Johnson. Joined with Kinney, Brown displaced General Shoe as the third largest producer in 1957, whether size is measured by value of assets, physical volume of production, or net dollar volume of sales [14, p. 727]. Nearly a thousand other smaller firms were engaged in the manufacture of shoes [4, p. 10].

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[4] Section 7 of the Clayton Act as amended (15 U.S.C.A., 18) prohibits corporate acquisitions ". . . where in any line of commerce in any section of the country, the effect of such acquisition may be substantially to lessen competition, or to tend to create a monopoly."

[5] The du Pont-General Motors case (353 U.S. 586), decided by the Supreme Court in 1957, as brought under the old 1914 statute, since the action was initiated in 1914 before the 1950 amendment [6 and 12]. For a discussion in this journal of the competitive impact question in horizontal merger cases see [8, 9, and 10].

Shoe retailing is even more decentralized than shoe manufacturing. In 1954 shoes were sold through over 70,000 retail outlets in the United States, of which some 22,000 were classified as "shoe stores" by the Bureau of the Census. These did about half of the shoe business, and about half the sales of shoe stores—that is, about one-fourth of total shoe sales—was made by the chain operators. In 1956 the six firms with the largest number of retail "shoe store" outlets owned 18 per cent of such stores, and the top 13 firms owned 21 per cent [4, pp. 16-17]. Thus, in both shoe production and shoe retailing a few firms are large relative to the great number of small firms, but the degree of concentration is low compared with such industries as tobacco, petroleum, aluminum, shoe machinery, and steel, in which important Sherman Act cases have dealt with the basic question of market structure.

The question before the court, however, was not whether Brown had monopolized the shoe industry, but whether the acquisition violated the Clayton Act. Judge Weber, who tried the case after the death of Judge Hulen, singled out three basic issues for decision: the lines of commerce, the sections of the country, and the probable impact of the merger. Brown's attorneys contended that the merger would be shown not to lessen competition substantially or tend to create a monopoly if the lines of commerce and sections of the country were properly determined.

The defense, of course, gains in a merger case if it can show that the only relevant markets are defined in such a way that the merger has no impact on competition. This showing may be made by defining the markets in which the two firms operate either (1) so narrowly that the merging firms have no markets in common, or (2) so broadly that the merging firms are but two small firms among so many others that any impact is negligible. Brown's attorneys chose the first of these alternatives. Following the 1930 International Shoe Company case [7], Brown argued for a product breakdown not only on the basis of age and sex of the ultimate consumer of the shoes, but also for a further breakdown on the basis of different grades of materials, quality of workmanship, price, and type of use of shoes.[6] The government argued that the relevant line of commerce was "footwear" or, alternately, "men's, women's, and children's shoes," separately considered.

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[6] The International Shoe case was the only case before 1950 in which the Supreme Court interpreted the standard of legality of the original Section 7 of the Clayton Act. The Court ruled that the International Shoe Company's acquisition of the stock of the McElwain Company did not violate the law primarily on the grounds that "in respect of 95 per cent of the business [of each of the two companies] there was no competition in fact and no contest, or observed tendency to contest, in the market for the same purchasers; and it is manifest that, when this is eliminated, what remains is of such slight consequence as to deprive the finding that there was substantial competition between the two corporations, of any real support in the evidence. . . ." A most important change made in the

Judge Weber concluded from his analysis of the cases "that a 'line of commerce' cannot be determined by any process of logic and should be determined by the processes of observation" [14, p. 730]. He asked what the testimony revealed about "the practices in the industry, the characteristics and uses of the products, their interchangeability, price, quality and style." He concluded [14, p. 732]:

> There is this noticeable fact, however, that there is one group of classifications which is understood and recognized by the entire industry and the public—the classification into "men's", "women's" and "children's" shoes separately and independently. Brown and Kinney each manufacture and sell men's, women's and children's shoes. While there is a close question as to whether "shoes—as such" could be treated as a "line of commerce" and, while there is argument for a breakdown into quality, style, price and intended use, there can be no question or argument either legal, logical or evidentiary, but that it can be said that all "men's shoes," regardless of quality, style, price and intended use, have sufficient peculiar characteristics and uses to make them distinguishable and a "line of commerce." The same can be said separately as to all "women's shoes" and all "children's shoes." On this the government has sustained its burden.

On the related question of the proper geographical delimitation of the markets, the defendants agreed with the government that the nation as a whole is the relevant "section of the country" for shoe manufacturing. But for shoe retailing the defendants' attorneys argued that the relevant sections of the country should be defined by the flow of local commerce, varying from the central business district of large cities to the "standard metropolitan area" as defined by the Bureau of the Census for smaller communities. The acceptance of this position by the court would have placed suburban shoe stores in separate markets from downtown stores. The government argued first that the whole United States was the relevant market, and alternately, in case the judge would not accept this definition, that it should be defined as a city, or a city and the immediate surrounding area. As in the case of the product dimension of the market, the defense was arguing for a narrow definition to show that an insubstantial part of the business of the two firms was done in competition with each other.[7]

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statute by the 1950 amendment, however, was the removal of the test of lessening of competition *between* the acquiring and acquired firms and its replacement by the test in terms of lessening of competition in the market. See [11, pp. 297-99] and [10, p. 527].

[7] In many of the post-1950 cases under Section 7 of the Clayton Act, the defense attorneys have argued for a broader market definition than the government in order to show a lower degree of concentration of control.

Judge Weber was satisfied that in cities like St. Louis, where the trial was held and for which information was available, the downtown shoe stores were competing for business with the neighborhood shopping centers in outlying areas. He found that seven million pairs of Kinney shoes were sold in one year in the same cities in which twelve million pairs of Brown shoes were sold, and noted at least 141 cities with more than 10,000 population in which both firms had outlets. Brown's and Kinney's shares of the retail market in these cities were held to be substantial. Judge Weber concluded that the relevant sections of the country were cities of 10,000 or more inhabitants and the immediate surrounding areas in which both firms had outlets. He explained the limitation of the definition to cities of 10,000 or more population by the fact that all Kinney stores were located in cities of such size and the evidence presented in the case dealt with such cities.

Judge Weber's reasoning with respect to the probable impact of the merger on the degree of competition and monopoly in the markets as defined was based on a definite recognition of a difference between the Sherman Act and Clayton Act standards. He said [14, p. 737]:

> Certainly it is evident that Congress intended to encompass minute acquisitions which tend toward monopoly and to do so in their incipiency. . . . What difference can it make that Brown has only 5% of the shoe production and Kinney 0.5%, when Brown is the fourth largest firm in the United States and Kinney with only 1.2% of all retail shoe sales is the largest family shoe chain retailer. Their combination moves Brown to third place in the industry. Does it then make sense to say that this is imperceptible because the percentages are small? Or rather, doesn't it make sense to say, that regardless of percentages or size, the test is, what do the facts show as to the trends in the industry and the true economic impact of this particular merger, which takes place among an industry having a few large firms that control a sizeable segment of the total with the balance divided among hundreds of others having only minute segments.

Thus on the impact question Judge Weber was much more concerned with rather vaguely defined general circumstances in the industry than with the structure of the specific markets he had defined as the relevant ones.

As to the trend in the industry, Judge Weber found substantial evidence of a change in its organization, of which this merger was a part. In 1945 two of the large shoe manufacturers—International and Brown—had no retail outlets. By 1956 International had acquired 130 and Brown 845. By 1958 Brown had over 1800 outlets in operation, counting the 416 Kinney stores. Between 1945 and 1956 General Shoe increased its retail outlets from 80 to 526; Shoe Corporation of America increased its number from 301 to 842; Melville Shoe Company from 536 to 947; and Endicott-Johnson stepped up slightly from 488 to 540. Judge Weber noted that between 1950 and 1956 nine independent shoe firms became subsidiaries of large firms, the acquired companies having operated 1,114 of the nation's 22,000 shoe stores [14, p. 737].

The trend in the industry was toward vertical forward integration brought about primarily by acquisition. Judge Weber viewed this development as an incipient lessening of competition because of its effect on the opportunity of the remaining nonintegrated shoe producers to find buyers of their products [14, p. 738]. The limitation on the markets of independent manufacturers was accompanied by a definite downward trend in the number of shoe manufacturing plants—from 1,207 in 1950 to 1,048 in 1956. In 1956, 20 per cent of all plants were operated by only 10 firms, which acquired seven companies with 25 plants between 1950 and 1956.

Besides the effect of vertical integration on the remaining nonintegrated manufacturers, Judge Weber found injury to the remaining nonintegrated retailers. Independent retailers testified that they were having a harder time competing with company-controlled retail outlets benefiting from increased brand-name acceptability resulting from national advertising, availability of credit, inventory control, and price control. As a result, independent retailers were forced to concentrate their business in the higher quality and higher priced lines for which the market is smaller. Judge Weber concluded that the independent retailer was placed in this disadvantageous position "not by choice, but by necessity" [14, p. 738].

The district court ordered Brown to divest itself completely of all the stock, assets, or other interests held in Kinney and to file with the court within 90 days a plan for effecting the divestiture.

II. *Supreme Court Interpretation of the Statute*

The Brown Shoe Company appealed the lower court's decision directly to the Supreme Court on the basis of the Expediting Act of 1903.[8] The Court's opinion discussed in detail the legislative history of the 1950 amendments to the statute in order to lay the foundations for its interpretation of the law. Congress, the Court decided, not only intended to make asset acquisitions subject to the Clayton Act's provisions, but also intended to change the standard of illegality contained in Section 7 of the Clayton Act. The Court cited seven factors specifically considered in the legislative history to be relevant to a judgment on the legality of a particular merger [3, pp. 4567-70]:

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[8] The government accepted the appellant's claim that the Supreme Court had jurisdiction and itself moved that the district court's judgment be summarily affirmed. The Supreme Court deferred ruling on the government's motion and noted probable jurisdiction over the appeal. During oral argument of the case, however, Mr. Justice Harlan raised the question of whether the district court's action constituted a final judgment within the meaning of the Expediting Act in view of the fact that the lower court did not include a specific plan for the dissolution of the merger. The attorneys for both the government and the Brown Shoe Company filed briefs in which they argued that the Supreme Court did have jurisdiction. A majority of the Court decided that it did have jurisdiction and the Court's opinion contains a lengthy discussion of the Expediting Act. Justice Harlan dis-

1. Congress intended to apply the statute to vertical and conglomerate as well as horizontal acquisitions. This intention was evidenced by the removal from the statute of that part of the standard of illegality having to do with the effect of an acquisition on competition *between* the acquiring and the acquired firm and its replacement with language stating the criteria in terms of the effect of the acquisition "in any line of commerce in any section of the country."
2. Congress intended to erect a barrier to the "rising tide of economic concentration." A keystone in the erection of this barrier was the provision enabling the Federal Trade Commission and the courts to stop mergers when the trend to lessening of competition in a market was still incipient.
3. Congress rejected the application of Sherman Act standards in Section 7 cases.
4. Recognizing that stimulation to competition might result from a particular merger, Congress did not intend to prohibit all acquisitions or mergers. The Court found two examples in legislative history of mergers thought to be acceptable by supporters of the bill: mergers of small companies that make possible more effective competition with dominant firms, and acquisition of failing companies that can no longer compete effectively in the market.
5. Congress did not adopt or reject particular tests for defining either product or geographical limits of the relevant markets within which the probable impact is to be judged. Nor did Congress define the word "substantially" by which the probable lessening of competition is to be measured.
6. Although Congress provided no specific qualitative or quantitative tests with which to gauge whether a given merger's effect may be substantially to lessen competition or tend to create a monopoly, it did indicate that a merger must be viewed functionally in the context of its

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sented from the decision on the jurisdictional question but concurred in the decision affirming the lower court decision that the acquisition violated Section 7 of the Clayton Act. Justice Clark concurred both in the decision on the jurisdictional question and on the merits of the case, but he wrote a separate opinion in which he urged that the Expediting Act should be changed so as not to force the Supreme Court to hear such cases on direct appeal.

particular industry, taking into account market structure, changes in market structure, and the condition of entry. Adoption of the amendment in 1950 had been urged as a means of preventing the Congressionally perceived trend toward concentration from reaching those industries as yet unaffected.

7. Congress intended the word "may" to indicate a concern with probabilities rather than certainties.

Thus the net result was a change in the law to prohibit some mergers that would have been permitted under Sherman Act standards; yet per se rules were not provided by Congress. A rule of reason still must be used by the Supreme Court in judging each merger case. Gradually, a new body of law will emerge from a series of cases decided by the highest court. The Brown Shoe case was the first of the series, and it provides new law on a number of questions.

A. *The Relevant Market Question*

The Court's problem of defining the relevant market is complicated by the fact that the defendants gain by either an extremely broad or an extremely narrow market definition. Per se rules are clearly inappropriate for such a question. In each merger case the trial court or Federal Trade Commission hearing examiner, constrained both by the particular facts at hand and the purposes of the statute, must find reasonable market definitions. In Brown Shoe the Supreme Court laid down some guidelines on both the product and geographical aspects of the problem.

1. *Line of Commerce.* Until this case, the most important Supreme Court precedents on the line of commerce question were the du Pont-Cellophane and the du Pont-General Motors cases. Both of these were cases in which the defense chose to argue for a sufficiently *broad* definition to reduce its market share enough to show no undesirable anticompetitive effects. The broad definition was accepted and the defense won in the Cellophane case against a charge of monopolization in violation of the Sherman Act. A narrow definition was accepted in the G.M. case and the defense lost against a charge of violating the pre-1950 Clayton Act.[9] Neither case was a binding precedent in Brown Shoe, and

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[9] In the Cellophane case the Court explicitly considered the limitation of the monopoly power of du Pont stemming from the existence of substitute products. The base on which du Pont's market share was calculated was increased by the inclusion of data on other flexible wrapping materials and the share was accordingly reduced. In the du Pont-G.M. case the Supreme Court used "automobile finishes and fabrics" as the relevant line of commerce, ignoring the existence of alternative buyers of finishes and fabrics outside the automobile industry in evaluating the foreclosure effected by du Pont's control of G.M.'s purchases. The Court held automobile finishes and fabrics to have sufficient peculiar characteristics and uses to constitute a line of commerce. See [2, pp. 92-100] and [12, pp. 233-39].

neither dealt with the question of how *narrowly* a market can be defined.

Since the Court recognized a Congressional intent to strengthen the prohibitions against mergers, it was necessary to find guidelines for market definition that would serve to lessen the government's burden of proof. The Court found such guidelines while accepting both the Cellophane and G.M. precedents.

The cross-elasticity or reasonable interchangeability criterion was accepted as the proper determinant of the outer boundaries of a product market. The Court insisted that [3, p. 4571] "the boundaries of the relevant market must be drawn with sufficient breadth to include the competing products of each of the merging companies and to recognize competition where, in fact, competition exists." Presumably, the Court was also accepting reasonable interchangeability as the criterion for deciding the question of "outer boundaries" in cases in which the defense argues for extremely broad definitions. But this acceptance does not mean that the Court has adopted the Sherman Act guidelines. The government's burden has been significantly eased by the Supreme Court's acceptance of the argument that subcategories included within a broader line may also be considered relevant. The boundaries of such relevant lines within lines may be ascertained by ". . . examining such practical indicia as industry or public recognition of the submarket as a separate economic entity, the product's peculiar characteristics and uses, unique production facilities, distinct customers, distinct prices, sensitivity to price changes, and specialized vendors" [3, p. 4570].

The Court went on to say that a merger is proscribed if the probable impact is found in *any* one of such economically significant submarkets. In other words, a merger is unlawful if the government can find a subproduct line, supportable as relevant on any of a number of grounds, for which the unlawful probable impact can be proven. The defense cannot rely on a proof of no unlawful impact in other product lines; it must fight on whatever battlegrounds the government chooses. Congress has said "*any* line of commerce" and the Court has accepted "any" at face value so long as some reasonable ground exists for the definition used. Applying these principles to the Brown Shoe case, the Court was able to uphold Judge Weber's decision that the relevant lines for both the vertical and horizontal parts of the case were men's, women's, and children's shoes.[10] The Court found these three product lines (1) to be recognized by the public, (2) to be manufactured in separate plants, (3) to have peculiar characteristics making each one "generally noncompetitive" with the others, and (4) to be directed toward distinct classes of customers.

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[10] The government originally alleged that a probable effect of the merger would be a substantial lessening of competition not only in shoe retailing but also in manufacturing, in which both Brown and Kinney engaged. The lower court decided for the defense on this question and the government did not appeal.

Although Justice Harlan concurred in the Court's opinion that the acquisition violated the statute, he did not accept the majority's opinion on the proper manner of defining the relevant product line. Justice Harlan indicated that he has a much less flexible attitude on the question. He gave no sign of accepting the lines-within-lines argument. Using the ease with which manufacturing facilities can be switched from children's shoe production to women's or men's or vice versa, he found the relevant line to be the combination of all three. Justice Harlan, however, was concerned only with the vertical aspects of the case, which he considered sufficient for its disposition.

In his separate concurring opinion, Justice Clark also argued that the relevant line of commerce is shoes of all types. He, too, wished to dispose of the case on the basis of its vertical aspects alone. He reasoned that the line affected by the acquisition was the full line of shoes handled by Kinney stores—a line coinciding with that manufactured by Brown. Thus, both Clark and Harlan would have disposed of the case with no clue as to how they would answer the line of commerce question, or any other question, in a horizontal case.

2. *Section of the Country.* Using criteria essentially the same as those laid down in arriving at product limits, the Court, while accepting the lower court's findings, offered several important conclusions of law relating to the geographical delimitation of a market. The line-within-a-line concept was extended to include a section within a section of the country. Thus the Court recognized the existence of relevant markets within relevant markets. A relevant geographic market may be as extensive as the whole nation or as small as a single metropolitan area. The whole nation was used for the vertical part of the Brown Shoe case, and cities of over 10,000 and their environs in which both Brown and Kinney had stores were used for the horizontal part.

A particular firm may operate in many markets, but [3, p. 4574]:

> The fact that two merging firms have competed directly on the horizontal level in but a fraction of the geographic markets in which either has operated, does not, in itself, place their merger outside the scope of §7. That section speaks of "any . . . section of the country," and if anticompetitive effects of a merger are probable in "any" significant market, the merger—at least to that extent—is proscribed.

In a footnote, the Court stated that the fact that the unlawful impact is found in only a small portion of the markets in which the merging firms operate "would, of course, be properly considered in determining the equitable relief to be decreed." This statement may open the door to partial divestiture orders, thereby giving the government incentive to prove the unlawful effect in as many markets as possible, in spite of the fact that proof for any one market is sufficient to establish the violation. This question of partial divestment can be expected to arise in future cases.

A strong case can be made for ordering divestment of only those specific assets for which the probable effect is a substantial lessening of competition. If antitrust policy were implemented by the institution of cases to remedy all unlawful situations, such remedies would not be questionable. Antitrust policy, however, has always been based on deterrence. Society relies on the business community to avoid committing the unlawful acts. Such a deterrence policy requires that cases be brought both to clarify the law and to insure a reasonable probability that those who knowingly violate the law will be prosecuted. In the case of Section 7 of the Clayton Act no criminal penalties apply. A policy of deterrence requires, therefore, that the remedies used by courts or commissions in merger cases be sufficiently stringent to provide incentives to business firms to make only those acquisitions that are clearly lawful. If the Court interprets the law to require complete divestment of any going concern illegally acquired, then more selective acquisition policies by business would be necessary. If partial divestment becomes the rule, then business will have little to lose by making acquisitions and waiting for government action to be brought. The government, on the other hand, would have little incentive to restrict the case to the one or two best relevant markets but would feel impelled to demonstrate anticompetitive effects in enough markets to assure maximum divestiture. With a limited enforcement budget, bigger cases would result in fewer cases. In short, this interpretation of the statute could result in little deterrence. The purpose of the 1950 amendments would not be achieved, and the Clayton Act would serve merely to harass business without preserving and promoting competition.

Fortunately, the Court only refers to the propriety of *consideration* of the degree of overlap in arriving at a remedy. In the case at bar, less than half the cities in which one or the other of the two firms operated outlets were represented. The Court concluded, and the defendants had not even questioned the fact, that the number of included cities was large enough to invalidate the entire merger, if the unlawful impact were found.

B. *The Competitive Impact Question*

The statute prohibits an acquisition only [1] "where in any line of commerce in any section of the country, the effect of such acquisition may be substantially to lessen competition, or to tend to create a monopoly." As noted above, the Court explicitly recognized that the amended statute applies to vertical and conglomerate as well as to horizontal mergers. The criteria for evaluating the probable effect on competition in each relevant market cannot be identical, of course, for these three types of factual situations. In disposing of the Brown Shoe case, the Supreme Court only hinted at criteria for judging conglomerate mergers, but Chief Justice Warren and the other four justices joining in his opinion were willing to offer criteria on both the vertical and horizontal questions.

1. *Competitive impact vertically.* The Court's opinion defines "vertical arrangements" generally as "economic arrangements between companies standing in a supplier-customer relationship." The effect of a vertical arrangement that ties a customer to a supplier, whether by merger or otherwise, may be to foreclose the competitors of either the supplier or customer from a segment of the market otherwise open to them. Neither Section 3 nor Section 7 of the Clayton Act, both of which deal with such vertical arrangements, forbids all of them. The probable anticompetitive impact on a relevant market must be demonstrated.

The Court reasoned that since the primary evil in a vertical acquisition stems from foreclosure of a share of the market, the magnitude of the market share foreclosed is an important consideration. But rather than adopting some specific market share and thus establishing a precedent in quantitative terms, the Court decided to depart from the market share criterion and to look at other factors for the answer. It specified three: (1) the nature and purpose of the contract, (2) the trend toward concentration in the industry, and (3) the merger's probable effects upon the economic way of life sought to be preserved by Congress [3, p. 4573].

Since the Clayton Act is not a criminal statute, the relevance of evidence on the purpose of the acquisition stems not from a need to prove intent to violate the law, but from the aid that such evidence may give the courts "in predicting the probable future conduct of the parties and thus the probable effects of the merger" [3, p. 4571, note 48]. The nature and purpose of a merger must be evaluated in the same way as the nature and purpose of a contractual vertical arrangement in a Section 3 case. The Court reviewed the interpretation of Section 3, pointing out the necessity under that part of the Clayton Act of examining the interdependence between the magnitude of the market share foreclosed and the economic purpose of a contractual vertical arrangement. A tying contract is by nature inherently anticompetitive, since it forces a customer to buy something he doesn't want to buy from the seller in order to get something he does want to buy. It is therefore ordinarily unlawful even if the amount of commerce affected is very small. In the case of a limited term exclusive-dealing contract, the market share foreclosed could be much greater without the law being violated. Such a contract, under which a buyer agrees to buy all he requires of a particular type of product from the seller, is not inherently anticompetitive since the buyer may have freely chosen the product and its seller. It might escape censure if its purpose is to insure the purchaser of a sufficient supply of a vital product. Also, the Court said a requirements contract might be lawful if the share foreclosed is small and there is no trend toward concentration.

Similar considerations of purpose and nature are necessary in vertical acquisition cases. The Court found the Brown-Kinney merger to be neither an acquisition of a failing company nor the combination of two small firms attempting to compete better with large dominant companies. Kinney was the largest independent chain, so the potential foreclosure was as large as possible with a merger of an independent retailer and a manufacturer. As to the question whether the potential foreclosure would likely become actual, it was apparent that foreclosure was the purpose of the acquisition. The Court therefore considered the nature of this vertical acquisition to be inherently anticompetitive like a tying clause. Shoes that otherwise would not have been bought by an independent Kinney were forced into the Kinney stores. The opinion also noted the more permanent nature of a tie by acquisition.

It is not clear that the Court's findings on the nature and purpose of vertical integration in the Brown Shoe case would by themselves have provided sufficient grounds for upholding the lower court decision without the companion finding of a trend toward vertical integration in the industry. Justice Harlan was willing to rely on the nature and purpose alone. But the majority accepted Judge Weber's finding of a trend toward concentration and considered this trend to be an important factor, saying that [3, p. 4573] "remaining vigor [of competition] cannot immunize a merger if the trend in that industry is toward oligopoly."

The Court was convinced by the record that the shoe industry was in danger of a change in organization that would probably foreclose competition from "a substantial share" of the relevant markets without resulting in sufficient countervailing advantages—competitive, economic, or social. It did recognize the elimination of wholesalers and increased volume of purchases from the firm's manufacturing division as possible sources of lower costs and lower prices that benefit consumers. But such offsetting advantages of vertical integration were given little weight. The Court's reasoning on this question is not entirely clear, but it appears to have been influenced by three considerations. (1) Independent retailers may be hurt. The Court, however, was not willing to accept injury to competitors in and of itself as a substantial lessening of competition in the market. The statute, it said, protects competition rather than competitors. (2) The weighing of the disadvantages of vertical foreclosure against the advantages of efficiency had been done by Congress when it enacted the legislation. The Court said [3, p. 4576]:

> But we cannot fail to recognize Congress' desire to promote competition through the protection of viable, small, locally owned business. Congress appreciated that occasional higher costs and prices might result from the maintenance of fragmented industries and markets. It resolved these competing considerations in favor of decentralization. We must give effect to that decision.

(3) The Court also seems to have recognized that the efficiency advantages, if great enough, can always be achieved by internal expansion rather than by merger.

Although many economists might wish that the Court had given more weight to the possible cost-reducing effects of the vertical acquisition, its position is certainly not the result of a naive adherence to a small-business doctrine. It was based on careful consideration of the facts and on the intent of Congress in enacting the 1950 amendments.

In summary, the seven Supreme Court justices who participated in the decision agreed that it is the foreclosing nature of a vertical acquisition rather than the magnitude of the share of the market foreclosed that is of primary importance in judging such arrangements under Section 7 as well as under Section 3 of the Clayton Act. The government's case was strengthened, no doubt, by evidence of a trend toward concentration through acquisition. Offsetting economic or competitive advantages were considered relevant, but insufficient, by the five-man majority. Thus, vertical acquisitions are not per se unlawful, but the new law of mergers puts much less burden on the government than before. The criteria used in judging vertical arrangements brought about by a transfer of property rights in a going concern are now essentially the same as the criteria used when the connection between seller and buyer is achieved through a loose-knit contractual agreement.

2. *Competitive impact horizontally.* In formulating criteria for evaluating the probable impact in the relevant market of the horizontal aspects of an acquisition, the Court did not move quite so far toward bringing together the law on loose-knit contractual agreements and

close-knit combination through property transfers. In the case of a contractual agreement to centralize pricing decisions among firms on the same side of a market, the law has long considered the nature of the arrangement so obviously anticompetitive as to make it unlawful per se without regard to the reasonableness of the prices fixed. Although centralization horizontally by means of transfer of property confers the same, if not more, power to centralize decision-making, the antitrust laws have until now been interpreted by the Supreme Court to require a showing by the government of power to exclude entry and enhance the price of a product sufficiently to injure the public. The nature and purpose of a transfer of ownership of property was presumed to be inherently consistent with public policy, so such an arrangement had to confer a very large share of the relevant market to a single owner in order to be unlawful under the Sherman Act.

In this first case under the new law the Court has provided a more stringent prohibition of horizontal acquisitions by accepting much smaller market shares than have been considered sufficient to confer illegal monopoly power in past cases. With three products and over one hundred separate geographical markets, the number of relevant markets in which to consider market shares was very large. In spite of the failure of the government to produce testimony and precisely defined statistical information on each of the markets, the Supreme Court accepted the findings of the district court as adequately supported by evidence. Judge Weber had accepted evidence on market share based on census information for cities, but he defined the market as "cities and their environs."

The largest combined share of Brown and Kinney outlets was 55.7 per cent of the sales of women's shoes in Dodge City, Kansas, in 1955. The Court noted that the combined share exceeded 5 per cent in at least one of the three product lines in 118 of the cities in which both firms had outlets. On the significance of a 5 per cent market share the Court had this to say [3, p. 4576]:

> In an industry as fragmented as shoe retailing, the control of substantial shares of the trade in a city may have important effects on competition. If a merger achieving 5% control were now approved, we might be required to approve future merger efforts by Brown's competitors seeking similar market shares. The oligopoly Congress sought to avoid would then be furthered and it would be difficult to dissolve the combinations previously approved.

Thus the Court considered the magnitude of the market share of the merging firms in the light of the degree of fragmentation that exists in the industry. A merger resulting in a 5 per cent market share might be lawful under different circumstances. Particularly, the Court would appear reluctant to prohibit the use of a merger to achieve a market share less than that already permitted for a firm's competitors. Thus, if a line is to be drawn someplace, it can best be drawn so as to prevent relatively large firms from getting larger market shares through merger, irrespective of the magnitude of the share. It would seem that the Court has adopted a policy on market shares in horizontal mergers similar to that used with vertical tying arrangements. The shares of the relevant market united by the merger is relevant, but the standard of legality is to be multidimensional. No particular percentage is to be used either to condemn or condone a merger. The market shares united by the acquisition must be evaluated in the light of several other factors.

In this particular case of a horizontal merger of retail chains, the Court considered several characteristics of the firm controlling a specific share of a particular market to be important. A specific share in the hands of a firm also operating in many other markets need not be as large to be unlawful as the same share in the hands of a firm operating only in the particular market. If a firm can price in such a way as to achieve higher profits from some of its markets than from others, it is certainly justified in doing so from a profit-maximizing point of view. The effect may be a lessening of competition, however, since a firm with some profitable markets can last longer in a price war in the more competitive low-profit markets. The existence of such unbalanced market power may serve to inhibit competition and promote price leadership even though the largest firm's market share is relatively small compared with that which would be necessary for price leadership to develop if all firms operated only in one market. The Court recognized the relevance of the multimarket nature of Brown-Kinney, saying [3, p. 4576]:

> Furthermore, in this fragmented industry, even if the combination controls but a small share of a particular market, the fact that this share is held by a large national chain can adversely affect competition. Testimony in the record from numerous independent retailers, based on their actual experience in the market, demonstrates that a strong, national chain of stores can insulate selected outlets from the vagaries of competition in particular locations and that the large chains can set and alter styles in footwear to an extent that renders the independents unable to maintain competitive inventories.

Here the Court offered the beginning of a rationale for an attack on conglomerate mergers. The Brown case, of course, did not hinge on this point. But if the Court finds the multiproduct nature of a firm relevant to an evaluation of the effect of an increase in market share in a particular market, then perhaps it will also conclude that entry by merger into other markets affects the degree of market power in the firm's existing markets.

Finally, as in the case of the vertical-integration question, the Court also considered the trend toward concentration in the industry. The Court found this merger to be the "appropriate place at which to call a halt." This incipiency doctrine requires prohibition of mergers that in and of themselves do not appear to have the effect of public injury. The prohibition of the Brown-Kinney merger is a means of forestalling a large number of other potential mergers in the shoe industry. Thus the general pattern of control in many markets will be affected by the decision.

In short, the Court has greatly narrowed the range of uncertainty about the new law on horizontal mergers. The Sherman Act requirement of proof of the achievement of power to enhance price and exclude entry is clearly inapplicable, having been replaced by the incipiency doctrine. Rather than simply looking to the past, the Court will look primarily to the future, forecasting in broad terms the probable effect on the structure and performance of an industry if such mergers as the one at hand are condoned.

The relevant markets must be defined, but market definition is not likely to be the crux of many horizontal merger cases for three reasons. (1) Markets within markets were clearly recognized by the Court so that the quest for the one and only right market definition is no longer useful to either party to a case. (2) The Court definitely rejected the idea that the existence of only a small amount of overlap of the business of the merging firms, and thus little competition between them, justifies a merger. Defendants can no longer hope to gain by arguing for the market definition that minimizes the percentage of each firm's business done in competition with the other. (3) Market share statistics have themselves been relegated to a subsidiary role, and the definition of the base on which shares are computed is less crucial to the outcome of a case.

III. *Conclusions*

The Supreme Court had to be concerned with the effect of the Brown-Kinney merger on competition in the relevant markets. Far more important, however, is the effect of the Court's interpretation of the law on competition generally in the markets of our economy. With this decision and the opinions accompanying it, the changes in the law on corporate acquisitions made by Congress in 1950 finally have been given specific content. In essence the new policy is simple: both vertical and horizontal mergers are likely to be held illegal unless the companies can demonstrate clearly that the mergers are likely to increase competition and thus promote the public interest. Unless a company is able to so demonstrate, any vertical or horizontal acquisition in any significant market is particularly subject to challenge if a trend toward concentration exists.

Public policy on close-knit centralization of control—that is, centralization brought about by change of ownership of property—has thus gone full circle. In the early and middle nineteenth century U. S. policy was very similar to this new one. At that time, before the enactment of federal antitrust statutes the policy was embodied in the law of contract, property, inheritance, and corporations. The legal framework simply was not conducive to centralization of ownership of productive resources on a large scale. To combine property of many individuals under a single controlling decision-making unit required a corporate charter granted by special act of a legislature.

As competitive pressures increased with the lessening of transportation costs in the latter part of the nineteenth century, and as technological developments made larger scale production economical, the corporate form of business organization became increasingly useful. One effect, however, of what was obviously a desirable development from other standpoints, was the liberalization around 1890 of the general incorporation laws of several states [11, pp. 11-18]. This removal of the old limits placed by public policy on the degree of centralization was not counteracted by federal law until the Court in the Northern Securities case overruled E. C. Knight. The permissive policy that coincided with the great merger movement at the turn of the century was followed by a half-century of sanctioned oligopoly.

At least insofar as newly created combinations are concerned the law has now been changed radically. The new policy can be applied to all mergers brought about since 1950 and to intercorporate stockholding relationships created since 1914. But what of the existing big corporations operating in oligopolistic markets since the great merger movement at the turn of the century? Is it equitable to prevent the owners of shoe producing and distributing assets from creating the sort of industry structure already existing in so many other industries while doing nothing about those industries? As the implications of the Brown Shoe case decision are recognized by the American business community, we are likely to see strong pressures arising to change the present state of the law. Some will advocate repeal of Section 7 of the Clayton Act. Another alternative, of course, is to use the Sherman Act to reduce the long-standing centralization wherever its advantages are not clearly demonstrable. It is possible that such use of the Sherman Act would require not a change in the statute, but simply a recognition by the Court of the relationship between a highly concentrated market structure and public injury. The time may be ripe for a carefully chosen Sherman Act case designed to bring the law on existing combinations into line with the new antimerger policy.

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A THEORY OF FLUCTUATIONS IN RESIDENTIAL CONSTRUCTION STARTS

By SHERMAN J. MAISEL*

Housing production, which has earned the dubious distinction of ranking among the most cyclically volatile industries, is important in the economy because of its large size. Since shifts in housing starts have both an immediate impact on earnings and a delayed reaction on shelter standards, fiscal and monetary policies frequently include programs aimed directly at control of the rate of construction. This article focuses on housing fluctuations and presents a market model designed specifically to aid in their analysis.

Figure 1 illustrates the violent movements which have characterized housing starts. On an annual basis, movements of 30 to 40 per cent from peaks to troughs occurred three times between 1950 and 1962. Starts in the highest quarter exceeded those in the lowest by more than 50 per cent—a difference at annual rates of more than 600,000 dwelling units.[1]

Table 1 relates these starts for 1950-60 to the more basic market forces of demand and inventory changes. Final demand (12,478,000), which approximated 91 per cent of starts, is measured by net household formation plus net removals. For that period, household formations (9,645,000) equaled approximately 70 per cent of private starts; net removals (2,833,000), covering a wide variety of gains and losses to the stock that may offset or augment construction of new dwellings, equaled an additional 21 per cent.[2]

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* The author is professor of business administration, University of California, Berkeley. This is the final article in a series, supported by the Center for Research in Real Estate and Urban Economics, Berkeley, concerned with the determinants of housing demand. The idea for this article and its initial development occurred while the author was working with the Capital Markets Research Project at Harvard. Discussions with the project's director, James Duesenberry, and criticism by members of the SSRC-Econometric Project contributed greatly to its development.

[1] The starts data in this article are based on official Census estimates for the period since 1959. For 1950-58 the original data of the Bureau of Labor Statistics have been revised to agree with both the new Census concepts and the 1960 Census. The appendix contains both data and explanations of the revised series of starts, plus those for other series estimated especially for this study.

[2] For the decade of the 1920's, final demand balanced approximately 75 per cent of starts. For the 1930's it was about 110 per cent. The low demand and high vacancy factor for the 1920's was clearly important in aggravating the depression.

Houses not absorbed by final uses increase the inventory. Table 1 shows a rise in available vacancies and a small drop in the inventory of units under construction. Inventory increases for the decade of only 9 per cent of starts reflect the well-recognized fact that, over any extended period, basic demand normally accounts for a greater percentage of the total.

Figure 1 and Table 2 (Appendix), however, present the opposite picture. For the decade, quarterly changes in inventories accounted for

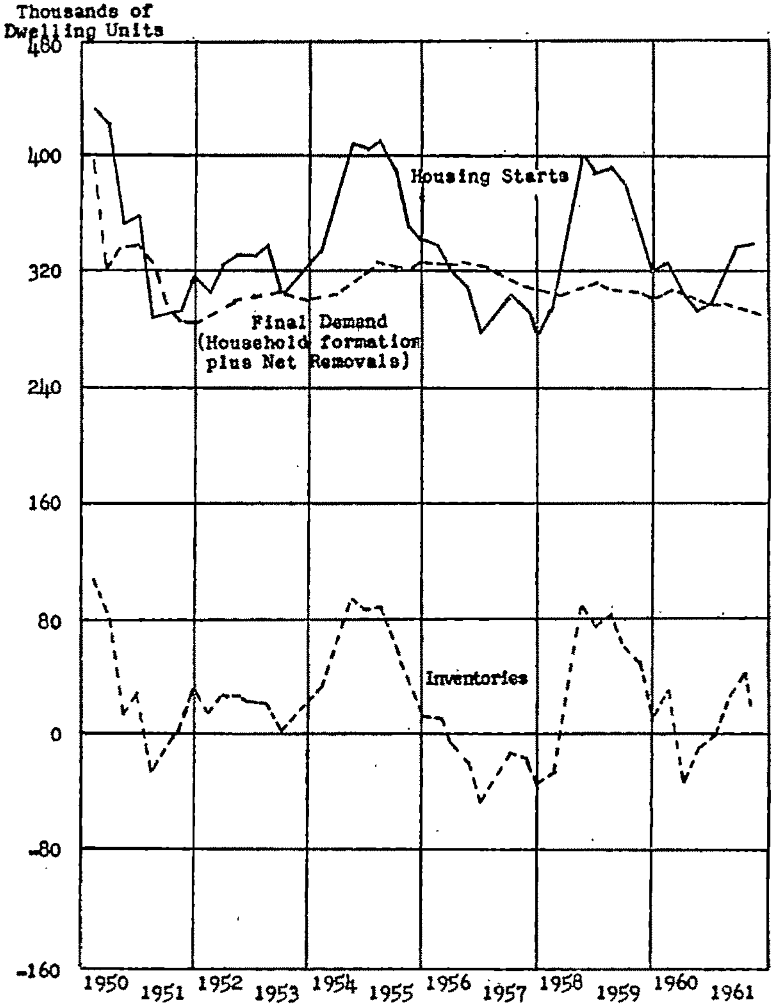

FIGURE 1. HOUSING STARTS, FINAL DEMAND, AND CHANGES IN HOUSING INVENTORIES IN THE UNITED STATES
QUARTERLY TOTALS, 1950–61

Source: See Appendix.

Table 1. Disposition of Total Estimated Starts of Private Housing Units in the United States. April 1, 1950 to March 31, 1960

Net Additions to Households:			9,645,000
Net Removals:			
Losses from available stock:			
Mergers from existing units	900,000		
Demolitions	2,050,000		
Other losses	1,947,000		
Vacancies held off market	692,000		
Subtotal:		5,589,000	
Increase in seasonal units:		337,000	
Offsetting gains, (negative removals):			
Public housing completions	425,000		
Trailers, institutional, group quarters etc.	962,000		
From conversions of other structures, splits, etc.	1,700,000		
Decrease in dilapidated vacancies	6,000		
Subtotal:		−3,093,000	
Total Net Removals			2,833,000
Increase in Available Vacant Units			1,245,000
Change in Inventory under Construction			−37,000
Total Private Housing Starts:			13,686,000

Source: Adapted from *U. S. Census of Housing 1960 Components of Inventory Change,* HD(4), Part 1A-1, Table C; *U. S. Census of Housing 1950,* Vol. 1, Part I; and 1960 *Advance Reports,* HC(A2)-1. Adaptation necessary because of change in definitions and sampling variability compared to decennial census.

85 per cent of the variance in starts, while the complementary movements in basic demand were related to less than 15 per cent. In shorter periods such as a quarter or a year, the market forces are reversed. The primary correlation is that between starts and inventory shifts.

The theory of the market developed in this article explains this sharp divergence. The short run instabilities that characterize housing starts have numerous causes deeply imbedded in the construction process. These forces primarily create fluctuations in vacancies and inventories under construction. The movements shown in Figure 1 are of a type similar to the inventory cycles experienced in manufacturing. In contrast, movements in underlying demand form a more stable equilibrium around which inventories fluctuate. While shifts in demand could cause sharp movements in starts, the forces necessary for such reactions—particularly heavy income declines—have not occurred in the postwar market.

Section I describes the logic of this theory and its main variables, contrasting the impact of these variables on inventories and on final demand. Section II presents the statistical testing of the model. Section III analyzes the fitted equation.

I. *A Model of the Housing Market*

Figure 2 represents the underlying framework of the market for housing and new construction. Around this skeleton, this section constructs a basic model, discusses the necessary interrelationships among the market's components, illustrates the variables, and selects some of the more obvious relationships for analysis and statistical testing.

The figure pictures a typical flow-feedback system. Because of lags, acceleration, partial adjustments, and amplification, such a system tends to have an inherent instability [2] [9] [15].

The major sector (I) represents the existing stock of dwellings which, by definition, equals the number of households plus vacant units. Sector II shows dwelling units completed in the period and their disposition. Completions, by definition, are identically equal to net household formation plus the change in vacancies and net removals. As net removals drop out of the picture, the other items flow into sector I, modifying the number in each of its segments.

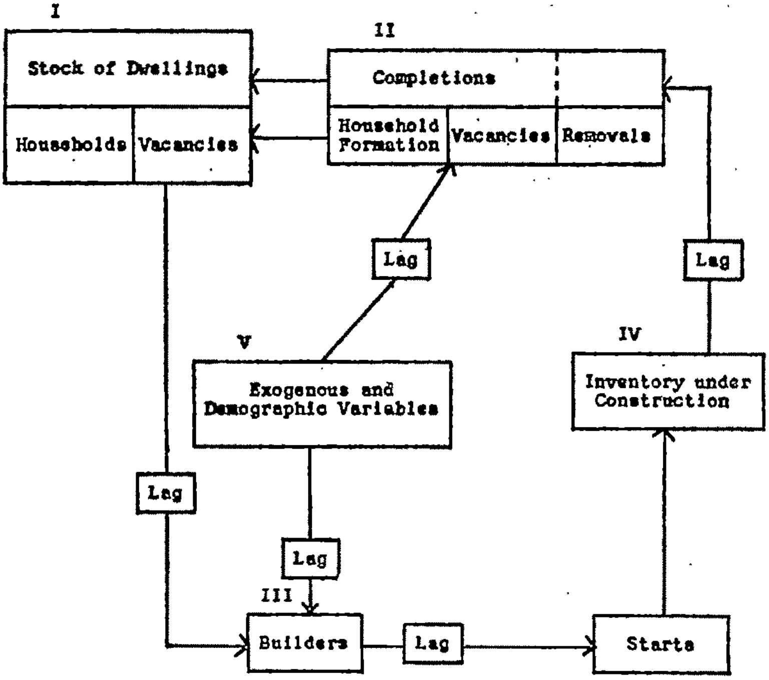

Figure 2. Net Flows of Construction in Housing Market

Let us examine the system in equilibrium. Builders (III) start exactly enough units to maintain a constant level (IV) of inventory under construction. The outflow of completed units from this inventory exactly equals basic demand (household formation plus removals and perhaps a normal increase in vacancies). The completions minus removals enlarge the stock of dwellings.

Now assume a shift in an exogenous variable (V), such as income, credit, or relative prices. Such shifts either may affect builders directly or may change the rate of household formation or removals. Changes in the stock become known to builders through data on vacancies, prices, rents, and rapidity of turnover. If the information is favorable, builders raise starts, enlarging the inventory under construction and, eventually, boosting the rate of completions. Unless by chance demand rises at the same rate as completions, vacancies will increase. As this fact also filters through to the builder, he will contract his starts.

The model clarifies the types of variables and other influences underlying fluctuations in starts. First come forces outside the housing market which can change the rate of household formation, increase or decrease the number of removals, or cause builders to alter their estimates of future demand and therefore their starts. Internally, as vacancy rates alter, the relationships between rents, prices, and costs of ownership change also. Builders and promoters find their profits affected either directly by price fluctuations or indirectly by the holding costs of vacant units. The lag between starts and completions may also lead to an inventory cycle.

A. *Movements of Final Demand*

This section lists the variables determining the level of final demand, and expresses and discusses their relationships. In contrast to the numerous forces which may directly influence housing starts, the number of variables about which significant statements can be made is limited. Each general relationship contains the term "disturbance" to account for both omitted and random factors.

Net Household Formation (ΔHH) depends upon population (Pop), disposable income (Y_d), unemployment (U), credit (Cr), prices (Pr), time (T), and other forces or disturbances (u):

$$\Delta HH = P\,(Pop, Y_d, U, Cr, Pr, T, u). \tag{1}$$

Population (Pop) includes the number, age, and sex distribution of the people in the country. In normal times, this variable dominates net household formation [3] [20] [26]. From 1890 to 1947, over 95 per cent of household formation could be attributed directly to this population factor. In periods such as the 1920's, G. H. Orcutt showed only minor annual movements in the ratio of actual to population-predicted marriage rates [26, p. 84].

The Great Depression and World War II so altered other social and economic variables that their influence tended to dominate those of normal population growth. In 1930-34 the rate of household formation to population dropped sharply as marriages were postponed and existing families doubled up in single dwellings. Between 1947 and 1960, the rate of household formation nearly doubled that expected from demographic forces.

Time (T) represents the social factors that gradually increase the number of households among populations of a given size. Until the war these social pressures were mild but steady, increasing household formation by just under 18,000 a year. In the postwar period a sharp drop in the average age at marriage, accompanied by an apparent unwillingness of families or individuals to share dwellings, boosted household formation by over 400,000 a year. Social changes seem to have diminished recently, with the result that additions to demand from this cause are likely to be far smaller than in the past decade.

Income (Y_d) and unemployment (U) are major indicators of economic well-being and appear significantly related to standardized marriage rates, undoubling, and other household formation [3] [20] [26]. Orcutt found a high correlation between changes in real disposable income per capita and annual marriages for the period 1920-56. Since 1950, a still better explanation can be devised by utilizing a quarterly index of unemployment of young males. Recent recessions have had more serious impacts on groups critical for household formation than on disposable income. Most marriages occur among those under 25, the group that has been among the hardest hit by recessions.

Household formation of non-marrieds also forms a significant segment of current housing demand. It fluctuates as much or more than marriages, and perhaps for the same reasons. Unfortunately, no suitable short-run data exist for movements of non-married households [20, p. 273]. The series in the Appendix is interpolated from changes in the rate of actual to expected marriages.

Credit (Cr) and prices (Pr) of constructing, owning, and leasing houses are frequently listed as important influences on household formation. It is assumed that couples marry and that individuals or couples undouble as a result of lower rents or greater ease in purchasing houses. Section III points out the need to separate credit and price effects on starts and holdings of vacant houses from their influences on household formation. That discussion indicates that both analytically and according to statistical tests the impact of these variables on the rate of marriages appears low [20].

B. *Net Removals from Available Housing—Census Starts Concept*

In addition to units required to shelter additional households, the economy must produce houses to replace those removed from the available stock. Final demand for new construction consists of net household formation plus the extremely varied types of forces operating on the housing stock which we define as "net removals."

Under the heading of net removals, Table 1 shows nine types of changes which add or subtract dwellings from the available stock in addition to those cited in Census Series, "Private Housing Starts in the United States" [32]. In the past decade, 283,000 new dwellings were required annually to replace net losses. This is in sharp contrast to the situation during the depression when these nine sources furnished so many additional units that no new construction was needed.

Because net removals can have such an important influence on starts, the possible causes of their fluctuation must be analyzed. Ideally, each of the components lumped under net removals should be treated separately, but unfortunately the required data do not exist. Table 1 is an almost unique attempt to estimate values for some of the subcategories. However, only the over-all decade total can be considered accurate. The subcategories may contain offsetting errors [17] [22].

A general relationship can be expressed as: net removals (Rem) depend upon governmental action (G), income (Y_d), vacancies (V), stock of housing (Hou), time (T), and disturbances (u):

$$Rem = R\ (G, Y_d, V, Hou, T, u). \tag{2}$$

Governmental action (G) has been one of the most important determinants of net removals. Losses in the stock result from the demolition of war housing, clearance for highways and urban renewal, and code enforcements. In the private market, demand from these sources is offset by additions of public housing. Contrary to expectations, administrative data on past and proposed governmental action are at present minimal. This segment remains an important source of uncertainty.

Income (Y_d) is unproven as a determinant of the removal rate for substandard units. We do know, however, that in prosperous times more dwellings are demolished to make way for other uses. Prosperity probably also cuts down on conversions. Some have suggested that higher income should also bring an increased production of seasonal units, but this market remains small, with additions of seasonal dwellings reported by the Census at only 33,700 per year.

These total income-related influences seem to have brought about much larger net losses since 1950 than in previous periods. Furthermore, the rate of removals appears to have risen gradually.

Vacancies (V) of less desirable units might lead to their more rapid removal, but this has not happened as yet. Because the initial cost of housing is so high, it is usually more efficient to repair and remodel existing units, even those in poor condition, than to tear them down and start anew. Only in exceptional cases has the market reduced the value of dwellings in the stock sufficiently to make their removal without a federal subsidy worth while.

Stock of Housing (Hou) would appear to be directly related to certain losses, such as those due to fires, floods, and other disasters. Some theories of fluctuation have been based on the notion that replacements occur as a function of depreciation. However, the probability of a replacement cycle is considered remote because housing's life span remains more an economic than a physical function.

Time (T) is included as a variable because most forces influencing removals work steadily and not erratically. Except through income, there are no obvious reasons for the main components to alter sharply in the short run.

Because there was little information on when removals took place within the decade, we experimented with five possible distributions. Since changes appeared to have occurred regularly, the relationship among these distributions was attributed to time rather than to income or other economic forces. The assumed distributions ranged from a constant amount in each quarter through exponential increases as a function of time.

Our final estimate for this series assumes a constant minimum plus losses equal to .1 per cent of the stock at the end of each previous quarter, to reflect forces which would grow directly with the number of dwellings. Such a distribution is logical only if economic forces either cause no short-run fluctuations or cause conflicts that offset each other's impact. But a disregard for economic forces is basically unsatisfactory, since removals did fall sharply in the depression. Income fluctuations for the past decade have been either too small to influence removals or one of the series in our statistical model, such as interest, carries whatever weight might attach to any cyclically related removals.

C. *Movements of Housing Inventories*

Housing inventories consist of units under construction and those held vacant for sale or rent. Figure 1 shows how large and erratic the movements in these inventories have been. In certain quarters the rate of change for each of these series ranged as high as 400,000 units on an annual basis. Fortunately technical factors make extreme movements of both types simultaneously improbable, so that the instability of all inventories has not been much greater than for either of its parts.

Inventories of goods in process are required because of the lag between starts and completions. The number of uncompleted units is determined by the length of the production process and by the feedback system relating starts to completions and final sales.

A normal stock of vacant units is needed for new sales and population mobility. An insufficient supply, as in the postwar period, creates a difficult and nearly intolerable situation for those requiring different accommodations or wishing to migrate. While individual owners desire no vacancies, the aggregate need is met through an equilibrium price or rent which makes it worth while for each to hold his share of vacancies in turn. Many analysts assume that equilibrium vacancies should equal five per cent of the stock. From 1950 to 1962 a linear trend showed an increase of 106,000 units in available vacancies annually. We consider this as a reflection of the rate of increase in the equilibrium level.

Movements of vacancies around this equilibrium have been important. Desired and actual vacancies need not be equal. Because of builders' and sellers' errors of optimism and pessimism, additions to the housing stock diverge from final demand. The resulting shortages or overages influence future starts. The reaction of builders to these vacancies depends on the market's organization. Vacancies may be of various types, spread over many local markets. Cross-elasticities are weak; the gestation period is long. As a result, macro disequilibria may last for long periods before they are adjusted by alterations in the number of starts.

Four major housing submarkets exist: (a) newly constructed single-family houses not yet sold or occupied, (b) previously occupied units being offered for resale, (c) new rental units, and (d) previously occupied units offered for rent.[8]

The level of vacancies in one sector may fall temporarily as that in another rises. Builders of new houses attract a surge of buyers by easier credit or price changes. New rental units entice tenants from older ones with lures of status or amenities. Then adjustments occur through the price mechanism and the type of new construction started. The feedback to starts will vary from short delays for vacancies in new sale houses to several years for existing rental units.

Although information on both total vacancies and its components is increasing rapidly, it is still inadequate [33]. There is difficulty in reconciling the decennial census data with current reports. Since 1955, vacancy rates for all sales-type units have varied between .5 and 1 per cent of all units [33], or from approximately 250,000 to 650,000 dwellings. For-sale vacancies have increased as much as 10 per cent of starts in critical years.

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[8] On April 1, 1960 there were 1,975,000 available vacant units with 525,000 for sale and 1,450,000 for rent. (An additional 2,326,000 vacant units were listed as not available.) Never-sold new units may have been anywhere from 5 to 40 per cent of the for-sale type, while new rental units probably were close to 20 per cent.

New sales-type vacancies are the smallest and, under normal circumstances, have the most rapid impact on new starts.[4] Because of carrying costs, any lengthening of the period of sale rapidly erodes the builder's profits. Furthermore, the volume of unsold new units is controlled by limited builders' capital and by the unwillingness of lending agencies to finance additional starts when the builder has a backlog of unsold units.

Vacancies for sale are also found among existing units. Again owners attempt to keep these at a minimum. Most vacancies result primarily from emergencies, death, foreclosures, or migration. Real estate men advise against vacating a unit prior to its sale because an owner's expenses continue for the empty unit, placing him at a disadvantage.

While vacancies of existing units slow sales and lower prices of competing units, their impact on starts is less immediate than that of new vacancies. The spatial separation of markets may also lead to higher total vacancies. Any particular locality may experience a local economic disaster, with accompanying high vacancies. If basic demand were to drop sharply, as in the early 1930's, the period of adjustment for sales-type units could lengthen.

The probability of sizable movements from equilibrium is greater for rental than for sales-type units. The quarterly Census Surveys indicate that rental vacancies nearly doubled between 1955 and 1961. Moreover, even though rental vacancies rose nearly as fast as new construction, starts continued to expand. In 1961, apartment starts surpassed their previous peak—in 1925—while a new postwar peak for rental vacancies was also established. Total rental vacancies equaled a four- to five-year production level for multi-family units. In recent years, about a quarter to a third of vacant rental units appear to be located in buildings less than three years old [33]. This would be equivalent to a 30 to 40 per cent vacancy rate for new structures.

In 1960 rental vacancies were nearly three times as large as vacancies for sales-type units. There are several explanations for these higher rental vacancy rates: (1) Since payments would fall on his occupied units also, even the best-informed owner may maximize his returns by allowing vacancies to increase rather than by cutting rents [27]. (2) In a market so complex and diverse as rentals, reliable information is scarce and this tends to breed inaction, hence an increase in vacancies. (3) During the first two years of operation, delays in renting are normal. The total gestation period on new construction covers a three- to four-year period—thus a long period elapses before the market experiences a feedback to new starts [35].

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[4] U.S. Bureau of the Census, *Construction Reports: Housing Sales,* Census HHFA: C25-1 (a new series) states that on April 30, 1962 there were only 54,800 completed and unsold new sales units. Similarly, U.S. Bureau of the Census, *Current Housing Reports,* Series H-111 No. 29, shows around 40,000 new vacancies. This would mean that these units were only a two- to three-weeks' level of single family starts, or abou ta month's inventory of units for sale. Local market information indicates that vacancies of this type have probably fluctuated from a one- to three-months' level of starts.

These forces causing vacancies to fluctuate about equilibrium can be expressed in an equation similar to those for final demand. Changes in vacancies about their trend (ΔV) depend upon costs (C), expectations (Ex), credit (Cr), rents (R), prices (Pr), the existing disequilibrium of vacancies (V), and disturbances (u):

$$\Delta V = V\ (C, Ex, Cr, R, Pr, V, u). \tag{3}$$

Costs (C) exert an important influence on starts. To a large extent, the promoter's costs are externally determined by the amounts he must pay for supplies and labor. If his margin decreases, he must curtail production. Normally, his investment in overhead is minimal, so there is little pressure to maintain output with a prospectively unsatisfactory markup.

Expectations (Ex): Because of the long interval between the start of production and the final sale, expectations play a significant part in forming a builder's decisions. Articles in business magazines and trade papers, talks with other builders and lenders, with customers and tenants—all of these affect his actions. Expectations may move in long or short waves. Clearly, the long delay in building acutely needed rental units in the postwar years can be attributed to the memories of the lean 1930's [35].

Credit (Cr) is a major variable influencing the margin by which starts exceed or fall short of final demand. Tightening of credit affects the terms and prices of construction loans, as well as the discounts that builders of single-family units must pay for financing. Movements in terms have been sufficient to wipe out all potential profits [14]. For promoters of apartments, availability of credit rather than price has been a critical factor, because most often their main concern is to minimize required equities [29, p. 25]. Relative changes in credit availability and terms may also bring about vacancy disequilibrium if they cause households to move from one sector of the market to another, and their vacating of old units has only a lagged impact on starts.

Our model is based on the hypothesis that the cost and availability of credit cause only temporary reactions on starts. This contrasts with the more common view that credit's impact on final demand causes fluctuations.

Rents (R) and prices (Pr) measure the income from building or owning houses. In addition, they reflect the reaction to changed costs, credit, and vacancies. Higher prices and faster sales may increase profits immediately. In other cases, because of weak cross-elasticities, such market movements may influence builders' profits only after considerable lags. Rents indicate possible profits from new apartment units. They also measure the competition between rental units and new sales-type units. Movements either in costs or in rents and prices can clearly advance or retard starts.

Vacancies (V) above the equilibrium are both a cause and effect of the variations in starts. As noted, high vacancies cause the rate of increase in inventories to fall. Speed of reaction depends on the type and location of the empty units.

In addition to changes in vacancies, fluctuations in inventories under construction are extremely important. Annual differences in this series have been as high as 275,000 (see the Appendix) and have accounted for more than a third of the variance in starts.

D. Changes in Inventories under Construction

Changes in inventories under construction (ΔI) depend upon the difference between starts (St) in a period and completions (*compl*), which are in turn a direct function of previous starts (St_{t-1}):

$$\Delta I = I\ (St,\ compl\ [St_{t-1}],\ u). \tag{4}$$

When final demand changes, starts must increase by still more in order to raise the inventory level to that required for higher output. This means that current starts are related to previous inventory levels and starts. A simplifying assumption, making completions an exact function of starts six months earlier, allows this inventory cycle to be expressed as a difference equation among starts of separate periods. It is simple to evaluate the form of this equation directly from the statistical estimates, while recognizing that the logical structure might vary somewhat [19].

The homogeneous form of the inventory equation using the parameters of equation (7) (see next section) is:

$$\text{Starts}_3 - .318\ \text{starts}_2 + .236\ \text{starts}_0 = 0.$$

This equation has one negative and two complex conjugate roots, all smaller than one. As a result, the inventory levels will oscillate. The period of the underlying cycle is between two and three years. If the system received no new shocks, the cycle would eventually disappear.

Movements in the inventory under construction serve to magnify any shocks arising in the remainder of the system. In mathematical form, the total system is described by a difference equation approaching a moving equilibrium [2] [10]. The form and magnitude of the fluctuations will depend on the coefficients of both the homogeneous equation and the equilibrium which it approaches; or, in other words, on the shifts in basic demand, builders' action, and the feedback-vacancy relations previously discussed.

II. *The Statistical Testing of Housing Market Theory*

The previous section developed a model of the housing market which can be tested statistically. In analyzing results, however, one must constantly hold in mind the inadequacy of the data and their weak conformance to the general criteria necessary for normal statistical techniques. That the parameters found statistically will vary from those of the true world is not only possible but likely. In fact, if the model's only test of reliability depended upon the correlation within the period, it would not be worthwhile to comment on the results. Because of the weakness of the data, I have added four other tests to the traditional one. While these tests are not independent, the fact that all give similar results lends somewhat more credence to the final equation. In addition, the resulting relationships have been checked against bits and pieces of data from cross-sections, from single areas, and from previous periods.

The preceding section discussed market movements in the form of the following four relationships:

(1) $\Delta HH = H\ (Pop, Y_d, U, Cr, Pr, T, u)$
(2) $Rem = R\ (Hou, V, Y_d, G, T, u)$
(3) $\Delta V = V\ (C, Ex, Cr, R, Pr, V, u)$
(4) $\Delta I = I\ (St, compl, u)$

We have also made it clear that a necessary identity exists. With changes in vacancies as a residual, the level of starts in any period is identical to the sum of the four dependent variables in the equations previously discussed:

$$St = \Delta HH + Rem + \Delta V + \Delta I. \tag{5}$$

One approach to statistical testing is to fit statistical parameters to equations (1) through (4), to add the results, and to compare this total with actual starts. It appeared, however, that fitting four separate equations would be demanding too much of the data because of missing variables and inexact estimates. Somewhat conflicting series for households, removals, and changes in vacancies occur. We desired to test the sensitivity of the model to the separate estimates. This was accomplished by utilizing a reduced form of equation (5). The results were then tested in equations (1) through (4). While the separate equations were significant, the statistics would be stretched beyond their true limits if each of the four separate equations were offered as a valid model. Instead, only the single reduced form is discussed.

Equation (6) is the final statistically derived equation. Since no adequate series for expectations and prices which appear in (3) could be found, we assume they are represented in (6) by the rent index. Because household changes and removals are included directly, the variables which influence them do not appear in this equation either.[5]

The estimates in (6) were fitted by least squares to the data for the inter-Census period 1950-60. The lags were found empirically and have no special theoretical value. In fact, from the previous section it is clear that, if four separate series for vacancies existed, each would have a separate lag.

The resulting equation, with its standard error and correlation and Durbin-Watson coefficients, is:

$$(6)\qquad St_0 = -278.0 - \underset{(9.47)}{27.92}\,\frac{1}{3}\sum_{2}^{4} i_{-j} - \underset{(.0393)}{.1391}\,V_{-1} + \underset{(.1652)}{.3191}\,St_{-1}$$

$$-\underset{(.0810)}{.2958}\,St_{-3} + \underset{(.935)}{2.212}\left(\frac{R}{C}\right)_{-1} + \underset{(2.006)}{3.637}\,Rem_0 + \underset{(.3539)}{.5677}\,\Delta HH_0$$

Quarterly $R^2 = .878$ $\bar{S}_u = 16.50$ $d = 2.09$ annual $R^2 = .99$

Where:

St = Census type starts of private dwelling units
i = Treasury bill rate new issues
V = Deviation of vacancies at start of quarter from straight trend
R = Rent component of the BLS Consumers Price Index
C = Residential cost component of the GNP implicit price index
Rem = Estimate of new removals
ΔHH = Net household formation in quarter

(For data see Appendix.)

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[5] For forecasts, of course, their future values must be estimated. In a more general model, their estimates are based on the theory discussed in Section II. Household formation is predicted from population, time, and unemployment. Removals are based on the stock of housing and time.

Clearly this model meets the traditional statistical tests, including that of a "*d*" or Durbin-Watson coefficient, with no indication of severe autocorrelation.

Because the data include estimated series with high possible errors, a second test measured the sensitivity of the model to extreme combinations of different forms of the independent variables. As examples, we used three household series which varied from estimates of no change to the large annual movements reported in the Census Current Population Reports. Similarly for removals, five series covered a range from no change through rates of growth decreasing exponentially and those increasing exponentially. We also tried five different interest rates; a direct estimate of government action through FNMA; and three forms of vacancy estimates.

For each set, shifts occurred in several of the main parameters in addition to those of the altered variables. Correlation coefficients tended to be somewhat, but not much, lower. In almost all cases, however, the general form of the model remained the same; the signs agreed; and the same variables appeared to be significant. The agreement of the equations with the underlying theory did not appear to depend on the particular procedures used to estimate the variables.

A third test was to compare the annual predictions from the quarterly models with actual annual data. As shown, the annual R^2 exceeded .99. This higher R^2 was to be expected since the residuals of the annual data are not independent of the quarterly residuals, but the correlation is satisfactorily high.

A fourth test utilized (6) in predicting the annual values for each of the two years (1960-61) which lay outside the original period used in fitting. Predictions were of two types—one period change and process estimates [6]. In the first type, starts were estimated from the values of the independent variables as they appeared at the end of 1961. This assumes correct knowledge of the variables in the projection period. Projected starts were 1,199,000 for 1960 and 1,336,000 for 1961, compared to actuals of 1,252,000 and 1,304,000. The predictions correctly show two turning points. They also show a sharp drop for 1960 in contrast to almost every available forecast for that period. That year experienced a 17 per cent drop in starts from 1959, which was contrary to the surface trends. As a result, almost all traditional forecasting procedures went far off the mark.

Estimates using actual values of the independent variables give a proper statistical test, but they do not show the model's ability to forecast prior to the availability of such data.

In order to test the model's forecasting ability, the independent variables must be estimated from knowledge available at the time of forecast. The test utilized projections of no change for interest rates, rents, and costs; a basic demographic series for households; and a time trend for removals. The values of lagged starts and vacancies were simulated by operating the model with the naively estimated exogenous variables a quarter at a time. The 1960 and 1961 forecasts from this simulation were 1,184,000 and 1,245,000 respectively. Again the turning points were correctly predicted. Each forecast was about five per cent too low. Given the difficulty of forecasting in this period, these predictions appear good; in fact, better than most.

A final test recomputed the equation to include 10 additional quarters in 1960, 1961, and 1962, which became available after the preliminary work was completed. This recomputation increases the degrees of freedom by more than 25 per cent. It also gives more weight to the official series for starts which begins in 1959. The results of the recomputation are:

$$
\begin{aligned}
(7)\qquad St_0 = -\,172.9 &- \underset{(6.73)}{20.25}\,\frac{1}{3}\sum_{2}^{4} i_{-j} - \underset{(.0367)}{.1441}\,V_{-1} + \underset{(.1420)}{.3177}\,St_{-1} \\
&- \underset{(.0780)}{.2357}\,St_{-8} + \underset{(.905)}{2.673}\left(\frac{R}{C}\right)_{-1} + \underset{(1.500)}{2.456}\,Rem_0 + \underset{(.3330)}{.5908}\,\Delta HH_0 \\
&R^2 = .85 \qquad \overline{S}_u = 17.01 \qquad d = 2.09
\end{aligned}
$$

Clearly, the significance of the variables did not alter. No basic shift in the structure is indicated. The same variables fit the longer period equally well. The observed differences appear logical in the light of our other market information.[6]

III. *The Behavior of Housing Starts in the Postwar Period*

Figure 3 contrasts the actual deviations of housing starts from their means with the deviations estimated by (7). The figure also shows the contribution of the individual variables to the estimates.

The fitted model agrees with both theoretical considerations and other knowledge in assigning a low importance to household formation and net removals as a cause of postwar housing fluctuations. With the exception of a movement in 1950-52 related to the Korean War and

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[6] As a final test we used the model to forecast new private housing starts for 1963. The model, on the basis of data at the end of 1962, predicts that housing starts in 1963 will be 1,296,000, or a decrease of 11 per cent from the 1962 level.

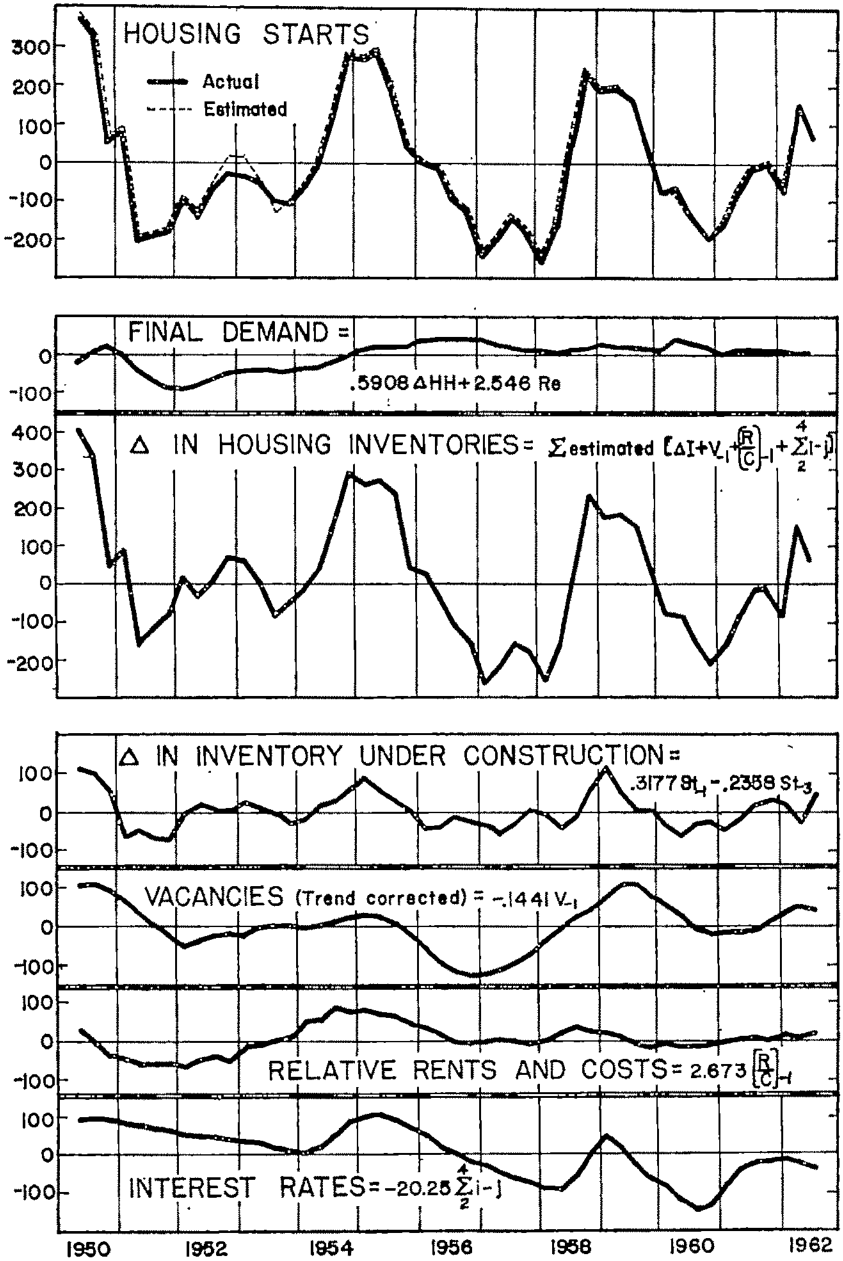

FIGURE 3. RELATIONSHIPS BETWEEN HOUSING STARTS AND THE INDEPENDENT VARIABLES
(Thousands of starts—seasonally adjusted annual rates)

reflected in starts, final demand forms a relatively stable equilibrium around which the critical disturbances take place. This theory clashes sharply with those expressed in many recent articles that attribute movements in housing starts primarily to the effect of changes in interest rates, maturities, and loan-to-value ratios on consumers' demand for space[7] [1] [5] [11] [12] [24] [30].

While starts are influenced by credit, as is evident from Figure 3, the channel of causation appears to be through inventories rather than final demand. Both analysis and policy will differ significantly, depending upon whether credit causes a temporary disequilibrium of inventories or a basic change in the number of housing units demanded by consumers.

It is difficult to find any logical channel of causation between credit and household formation.[8] Few newly formed households purchase homes. In 1960 census components of change show that only 7.5 per cent of households formed in 1958-59 owned their own homes. Since these households include a large number of reconstituted ones (i.e., households formed by widowed, divorced, separated persons, etc.) whose members probably owned homes under their previous marital status, it is likely that only one or two per cent of new households purchase dwellings during their first two years of existence. Almost certainly, only a small fraction of these, and therefore an insignificant part of the total, would delay marriage or refrain from forming their own households because of credit changes.

For financing to affect the formation of non-owner households, it must first alter rents; then, higher rents must deter people from marrying or undoubling. There is no reason to expect credit to have short-run impact on rents. Furthermore, rents, averaging about 15 per cent of income, are only part of the cost of establishing a household [23]. Given the high psychic costs of delayed marriages or merged households, it appears that reactions to changed prices or rents are likely either to be small or to influence long-run rather than cyclical changes in household formation.

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[7] Break [5, p. 225] correctly summarizes the views of most of those who have been interested in housing fluctuations as a potential area for the use of monetary policy. "Changes in the interest rates, maturities, and loan-to-value ratios prevailing on new home mortgages have an important effect on the demand for housing. This is a proposition that not only commands general acceptance among experts, but also forms the basis of the loan insurance and guaranty programs of the Federal Housing Administration (FHA) and Veterans Administration (VA)."

The approach of this paper, in contrast, builds upon the theories found in much of the business cycle literature [3] [7] [8] [13] [18]. The fact that two different approaches have led to such sharply divergent analyses might make an interesting case study for an historian of the development of economic theories.

[8] There is, of course, almost certainly a relationship between credit and the average expenditure of households on housing. A failure to distinguish between demand for a certain number of houses and the average value of each leads to some of the errors in analysis.

Given the normal situation of a large stock of households growing at a slow rate, an acceleration could make even small percentage increases in total households important for new starts; but such movements do not seem to have occurred in recent years.

Figure 3 shows little relationship between credit and household formation (it actually is negative). This lack of any positive correlation between household change and credit or house prices has generally been found in previous studies as well.

The pictured impact of the other fitted variables reflects both their own weights and the weights of the wider group of variables discussed in Section I. Equation (7) and Figure 3 measure the impact of each variable on both types of inventory movements. The movement of inventories under construction in Figure 3 has a roughly similar path, but less amplitude, than the data in the Appendix. This results from the fact that the fitted variable, Δ in inventory under construction, reflects only the endogenous inventory cycle.

In the three cycles since 1950, actual disequilibrium movements consisted of almost equal changes in inventories of units under construction and available vacancies. The larger actual movements in inventories under construction as estimated in Table 2 result from shocks transmitted through the remaining variables. The amplitudes of the endogenous movements are increased by the other forces particularly in 1954, 1958, and 1960.

The level of vacancies appears to play a role in each cycle. Below-normal vacancies gave impetus to additional building in 1950 and accounted for most of the rise in starts in 1957, 1958, and 1961. On the other hand, the expansion of starts in 1954 and 1955 caused a backlog of empty units which were an important cause of decreased building in 1956-57. A similar reaction occurred in 1959. Throughout the period, vacancies appear to have had far more influence on starts than most observers noted.

The data used to represent the ratio of rents to costs are not as sensitive an index as one would hope for because of the notorious inadequacies in the underlying data. Still the relatively low level of rents resulting from rent control seems to have had a depressing effect on starts through most of 1952, causing demand to be deferred during this period. In 1953 and 1954, rents shot up much faster than costs, thus adding to the expansion.

The high level of starts in 1954 and 1955 led to a rapid rise in costs, which in turn helped to make building or speculating on starts less profitable. Since 1956, except for a small impetus given to the boom of 1958, the influence of these variables has been slight.

The final variable consists of lagged short-term interest rates. According to equation (7), credit, vacancies, and the inventory cycle account for approximately equal parts of the total variance in starts. As explained earlier, our hypothesis states that changes in the cost and availability of credit cause important disturbances in the rate of inventory accumulation. Builders and promoters increase starts because their potential profits grow as discounts fall or the amount of equity required is decreased. In addition, with more favorable terms, they may attract existing households from other parts of the market. As long as their inventories remain low, their profits grow. However final demand does not expand with starts. When the increased inventory is mainly in units under construction, builders feel only slight pressure to change their action. Eventually, however, as the number of empty completed units grows, pressure mounts to cut back starts. Timing depends on both the period of production and the lags between starts and particular types of vacancies.

Because of intricacies in the mortgage market, as, for example, discounting, many seemingly obvious credit variables, such as FHA yields (even corrected for discounts), conventional yields, or the spread between bond yields and FHA rates, are poor measures of the cost of credit. This fact, as well as the probability that changes in availability may be even more significant than costs, explains the earlier finding that short-run interest rates give a considerably better fit in equations (6) and (7) than any of the long-run mortgage credit series.

Because credit is an important government policy variable, significant changes in past relationships may occur which must be considered in analysis and forecasting. Clearly, if the Federal Reserve succeeds in breaking past relationships between the Treasury bill rate and the availability and cost of credit, equation (7) will be affected. Even more important may be the changing policies of the Federal National Mortgage Association. In this study, FNMA intervention in the credit market appeared significant on theoretical grounds, and nearly so on a statistical basis. However, since FNMA action was closely related in time to interest rate movements, it did little to increase our statistical explanation of past movements. Even though this collinearity did result in insignificant coefficients for FNMA policy, it seems logical to assign to direct governmental intervention of this type a good deal of weight, even if only subjectively.

Finally, Figure 3 shows significant longer-run movements. In 1950 there was still a backlog of final demand. Builders were more likely to receive premiums than to pay discounts for their financing; vacancies were below a desirable level; households were still doubled up; rent control was in the process of being dismantled; the high demand allowed only minimum removals. Between 1950 and 1956, most of these aftereffects of the war worked themselves out of the system. Since 1957, the number of starts has averaged about 1,366,000 units per year, with a high quarter rate of 1,604,000 and a low of 1,162,000. With this production, available vacancies have accumulated at the rate of about 60,000 per year, slightly below the assumed equilibrium.

There is no obvious reason for the average level of starts to vary much in the near future. Only if sudden economic changes or governmental policy should drastically alter the present levels of basic demand or if vacancies begin to change rapidly, would the number of units constructed over the next five years vary sharply from the previous five.

The probabilities of continued instability also appear to remain unaltered. Basic factors of lags, acceleration, and poor information have not changed, and the system continues to be plagued by unstable forces. The possibility of serious outside shocks remains strong. Only a major increase in knowledge, combined with government policies specifically designed to compensate for, rather than accentuate, normal market instability would appear to offer much hope for improvement.

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TABLE 2—(*Continued*)

1958	1st	278	235	−14	1,747	74
	2nd	299	232	− 5	1,731	74
	3rd	354	234	74	1,719	74
	4th	401	235	116	1,690	75
1959	1st	389	237	53	1,664	75
	2nd	391	234	− 5	1,688	75
	3rd	382	233	12	1,775	75
	4th	356	231	−19	1,837	76
1960	1st	323	226	−73	1,905	76
	2nd	327	236	−33	1,999	76
	3rd	307	232	−16	2,047	77
	4th	292	227	−40	2,061	77
1961	1st	299	219	−15	2,089	77
	2nd	320	221	+41	2,104	77
	3rd	338	220	+59	2,085	78
	4th	340	219	+26	2,066	78
1962	1st	321	217	−13	2,083	78
	2nd	378	214	+68	2,122	78
	3rd	356	213	−21	2,140	79

Source: Appendix.

3. Changes in the Private Inventory Under Construction (seasonally adjusted).

This series is derived directly from the starts series on the assumption that the time from start to completion is two quarters for single-family units and four quarters for multifamily. The actual amount in this inventory would vary if the construction period changed.

4. Units Vacant Available for Sale and Rent (seasonally adjusted).

Additions to vacancies reported by the 1950 Census were cumulated quarterly. Completions in a quarter were estimated from lagged starts according to series 3. Net change in vacancies is the number of completions minus the number of households formed (2) and units removed (5).

5. Series on Net Removals. Net removals in each quarter were assumed to equal the constant 21,100 plus 0.1 per cent of the available stock at the end of the previous quarter.

6. The following are standard series: Rate on New Issues U. S. Government three months bills: *Federal Reserve Bulletin.*

7. Residential Rent Component of the Consumer Price Index (1947-49 = 100), U. S. Housing and Home Finance Agency, *Housing Statistics,* October 1961, p. 95.

8. Cost of Residential Construction—GNP Implicit Price Deflator Residential Construction (1954 = 100), Table 6, *Survey of Current Business* (July 1961).

THE OPTIMUM FOREIGN EXCHANGE MARKET

By JEROME L. STEIN*

During the past few years, the system of international payments has been examined critically by economists and bankers. Most critics contend that improvements could be made, and some have offered detailed proposals for the revision of the system.

The criteria and techniques of analysis by which economists evaluate the efficiency of alternative forms of market organization were not used by these critics. Nowhere did they prove that the implementation of their proposals would raise an unambiguous measure of social welfare. The developments in welfare economics during the past thirty years seem to have made little impact upon either the critics or the defenders of the existing system of international payments. The contribution of this paper is the demonstration of how social welfare criteria can be used to evaluate the desirability of alternative foreign exchange markets. It is shown that the optimum exchange market depends upon the structure of the economy.

A cardinal utility function is constructed where the independent variables are the real absorptions over a set of n periods. The problem is to find the time path of absorptions which will maximize social utility, subject to a constraint. This is an Irving Fisher problem applied to international finance. The constraint is that the country should neither accumulate nor decumulate monetary gold over the entire set of n periods. That is, the sum of the money value of production should equal the sum of the money value of absorption over the n periods.

Optimality requires that the marginal utility of real absorption per dollar change in the international balance be the same in each period. From this necessary condition an optimum price line is deduced. This line relates the optimum price of foreign exchange to the real value of current production that would prevail if the exchange rate were fixed at its long-run equilibrium value. We then evaluate the efficiency of a stabilized and a free exchange market in terms of deviations of the actual prices of foreign exchange from the ideal prices.

In a Keynesian economy where there are idle resources, a higher level of real absorption could be achieved by increasing aggregate demand. Currency depreciation tends to increase aggregate demand and hence real absorption. Over a business cycle, optimality requires that: (1) the price of foreign exchange should rise during contractions and thereby stimulate production and raise real absorption, and (2) the price of foreign exchange should fall during expansions and thereby curtail aggregate demand and real absorption. Absorption will be smoothed in an optimal way if currency depreciation occurs during contractions and currency appreciation occurs during expansions.

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A free exchange market tends to be more efficient than a stabilized exchange market if the balance of payments tends towards deficits in contractions and towards surpluses in expansions. Then, the price of foreign exchange will rise during contractions and stimulate production and absorption and fall during expansions and reduce production and real absorption. On the other hand, suppose that the structure of the economy is such that the balance of payments tends towards surplus in contractions and towards deficit in expansions. Then a market stabilized at an equilibrium rate of exchange will be more efficient than a free exchange market. The latter would produce currency appreciation during contractions and currency depreciation during expansions. Fluctuations in real absorption would be aggravated in a free exchange market. In a Keynesian economy with idle resources, the optimum exchange market depends upon the relation between the balance of payments and the state of the business cycle.

We also examine a full employment economy. Again it can be shown that the optimum exchange market depends upon the structure of the economy. In an export economy which is always at full employment but has varying levels of productivity, a market stabilized at the equilibrium rate is more efficient than a free exchange market. The reason is that in a free market the terms of trade would improve during the export season when produced income is above the mean. Since absorption depends upon the terms of trade and real output, a positive covariance between the terms of trade and output aggravates fluctuations in absorption.

I. *A Social Utility Maximization Model*

A. *A General Formulation*

The economy is viewed as a set of homogeneous families with respect to the utility enjoyed. A smooth cardinal utility function is assumed to exist for this economy defined on a set of (real) absorptions. The sum of (real) sales to consumers, net private domestic investment, and sales to the government constitutes (real) absorption. In this formulation, utility is assumed to be produced by the rate of growth of wealth as well as by the rate of consumption. The inclusion of sales to the government and domestic investment in the social utility function reflects the utility derived from the services of the government sector and that derived from the increased capacity to produce income in the future.

The time horizon of the economy consists of n discrete time periods. If U is the utility function and a_t is (real) absorption at time t, then

$$(A1) \qquad u = U\,(a_1, \ldots, a_t, \ldots a_n),\ U'_t > 0,\ U''_t < 0$$

where u is a real number. Positive, but diminishing, marginal utility of absorption is assumed to prevail in each period.

Absorption enjoyed by the economy during a given period is determined by the level of real output (y_t) and by the price of a unit of foreign exchange (p_t). The marginal propensity to absorb out of current production,[1] $\partial a/\partial y$, is assumed to be greater than zero but less than unity.

Given the real value of current production y, real absorption is positively related to the terms of trade. Variations in the terms of trade T could occur in several ways. The terms of trade is the ratio of the domestic prices of home-produced goods (q_H) to the domestic prices of imported goods (q_F). The denominator of the terms of trade is the foreign price of imported goods (q'_F) multiplied by the price of a unit of foreign currency (p). That is,

$$T = (q_H/q'_F)\ (1/p).$$

Insofar as a country's exports are usually more specialized than its imports, a negative relation is believed to exist between p and T [1, p. 268]. Or we could consider a world where domestic prices are given in all countries, and T is inversely related to p.

An improvement in the terms of trade raises real income. Since the marginal propensity to absorb out of real income is positive, absorption will be positively related to the terms of trade. As a result of the assumed negative relation between p and the terms of trade, given y, a negative relation will prevail between absorption and the price of foreign exchange. The absorption function is:

$$(A2) \qquad a_t = A\ (y_t, p_t), \qquad 1 > A_1 > 0, A_2 < 0.$$

Interdependence exists between the independent variables in (A2) above. Variations in the price of foreign exchange (p) induce changes in the real value of current production (y). The lines of interdependence between p and y have been neatly subsumed under the following categories by Fritz Machlup [3, pp. 260, 265-68].

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[1] The marginal propensity to absorb is the sum of the marginal propensity to consume and the marginal propensity to invest.

1. *The idle resources effect.* When the price of foreign exchange is raised, the prices of export goods and of import-competing goods are raised relative to wage rates. Thus the output of exports is induced and consumers are induced to switch from imports to home-produced goods. Moreover, as Laursen and Metzler [2, p. 286] have shown, the total money consumption out of a given money income will rise with the price of foreign exchange. As a result of the switching from imports to home goods and the rise in the total money expenditures by consumers, aggregate demand for domestically produced goods increases relative to aggregate supply. If there are idle resources, there will be an induced increase in the real value of current production. The magnitude of the change in y induced by the rise in p, which increases the total demand for domestically produced goods, depends upon the elasticity of aggregate supply and the cross-elasticity of demand between the output of domestically produced goods and the prices of foreign goods in terms of domestic currency.

2. *The resource reallocation effect.* When the exchange rate is overvalued, an excess demand exists for foreign exchange. Often, this excess demand is frustrated by tariffs, import quotas, exchange restrictions, or subsidies to exporters. The net effect of such governmental actions is to violate the marginal conditions for a maximum real value of current production. A misallocation of resources is produced, in the attempt to balance the supply of and demand for foreign exchange. When there is an excess demand for foreign exchange, the maximum price that an importer is willing to pay for a unit of foreign exchange exceeds the minimum price at which an exporter will sell. The value of the marginal product of imports, in the production of domestic goods, is equal to the maximum price of a unit of foreign exchange that an importer is willing to pay. On the other hand, the minimum selling price of a unit of foreign exchange by an exporter is equal to the marginal cost of the export goods.[2] When there is an excess demand for foreign exchange, the value of the marginal product of imports exceeds the marginal cost of the exports required to purchase these imports with foreign exchange. For this reason, there is a misallocation of resources.

By raising p, when there is an excess demand for foreign exchange, the governmental restrictions or favors can be eliminated. As a result of the improved allocation of resources, i.e., the satisfaction of the marginal conditions for a production maximum, y can be raised. This is the *resource reallocation effect* of a change in p upon y.

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[2] These conditions will be satisfied in the short run, but not necessarily during the market period.

We may summarize the influences upon the real value of current production with

$$y = f\,(y^*, p). \tag{A3}$$

The function $\partial f/\partial p$ is precisely the resultant of the two effects cited above. When there is a positive elasticity of aggregate supply, f_2 will be positive. If there is full employment and no misallocation of resources, f_2 will be zero, for variations in the price of foreign exchange cannot affect the value of current production. On the other hand, if there is a severe misallocation of resources at full employment as a result of government restrictions to eliminate a basic deficit, f_2 will be positive when governmental restrictions are lifted concomitant with the devaluation. For these reasons, we claim that as a rule f_2 will be positive: an increase in the price of foreign exchange will tend to raise the real value of current production.

Variable y^* refers to the level of real output that would result if the price of foreign exchange, measured from some arbitrary origin,[3] were zero. For simplicity, y^* will be taken as an exogenous variable. Our object is to distinguish between two sources which affect the real value of current production. The y^* variable reflects the value of y that would occur if the exchange rate were fixed at a given level. Domestic supply and demand conditions and foreign supply and demand conditions, given p, are all subsumed under y^*. In the usual Keynesian analysis, y^* is the level of output at which aggregate demand is equal to aggregate supply, given p. In a free exchange market, y can also be affected by variations in the exchange rate, which is the second source of disturbances in y. Later, the interdependence between y^* and p is considered. But for the present, assume that y^* is completely exogenous to the model.

Schematically, our model contains the following system of relationships. Utility (u) depends upon

$$\begin{tikzcd}
u & a \arrow[l] \arrow[r, leftrightarrow] \arrow[d, leftrightarrow] & p \arrow[dl, leftrightarrow] \\
 & y & y^* \arrow[l]
\end{tikzcd}$$

absorption (a). Real output (y) and the price of foreign exchange (p) determine absorption. In turn, real output is affected by the price of foreign exchange and other conditions y^*.

Finally, the model is constrained with the requirement that, over the n periods, the balance of payments must balance. For expositional simplicity, assume that all claims against (or liabilities to) foreigners consist of accumulations (or decumulations) of monetary gold which yields no interest. The balance on current account[4] is identically equal to the money value of output less the money value of absorption. If q represents (a vector of) prices, y represents (the vector of) real outputs, and a represents (the vector of) real absorptions, then the money value of the balance[5] in period t is $q_t(y-a)_t$. Since the balance of payments surplus or deficit is not invested in interest-earning assets, balance of payments equilibrium over the entire period requires that the sum of the money value of production $\sum qy$ equal the sum of the money value of absorption $\sum qa$. That is:

---

[3] The arbitrary origin could be the mean value of p that would produce equilibrium in the balance of payments over the entire set of n periods. See equation (4.1) for the determination of this value.

$$\text{(A4)} \qquad \sum_{t=1}^{n} q_t(y-a)_t = 0.$$

This constraint requires that the economy should be neither a net creditor nor a net debtor over the entire set of n periods, i.e., it should neither accumulate nor decumulate monetary gold over the period as a whole.

B. *The Maximization Conditions*

The independent variables in this model are $(y_1^*, \cdots y_n^*)$, i.e., the levels of real output that would result if the price of foreign exchange, measured from some arbitrary origin, were set at zero. We seek the set of foreign exchange prices $(p_1, \cdots, p_n)$ which will maximize social utility in our country subject to the constraints. The effects of our policies upon the rest of the world are assumed to be negligible. These prices are the *ideal* prices of foreign exchange. In the subsequent parts of this paper, the *actual* prices of foreign exchange produced in free and stabilized foreign exchange markets are compared with the *ideal* prices.

To determine $(p_1, \cdots, p_n)$, the vector of ideal prices, define the Lagrangian function W, where

$$\text{(1)} \qquad W = U(a_1 \cdots a_n) - \lambda \sum_{t=1}^{n} q_t(y-a)_t.$$

Substitute (A2) and (A3) into (1), and express y and a in terms of y^* and p. Thus,

$$\text{(2)} \qquad a = A(y, p) = A[f(y^*, p), p].$$

---

[4] In this model, no long-term capital movements occur. If there were long-term capital movements equal to L, then the constraint (A4) would be that the sum of the current account balances plus L be equal to zero.

[5] The identity, GNP ≡ absorption + balance, holds in money terms whereas the variables in the model are in real terms. Consequently, price vector q_t is introduced in (A4). As Machlup [3, pp. 269–70] has shown, the concept of a "real" balance is not useful. Only money balances have economic significance.

This can be simplified as $a = G(y^*, p)$; and the money balance is

$$q(y - a) = q[f(y^*, p) - G(y^*, p)]. \tag{3}$$

The function W to be maximized is

$$W = U[G(\overset{*}{y_1}, p_1) \cdots G(\overset{*}{y_n}, p_n)] - \lambda \sum q_t[f(\overset{*}{y_t}, p_t) - G(\overset{*}{y_t}, p_t)]. \tag{4}$$

The necessary conditions for an interior maximum are:[6]

$$\frac{U_t' G_2}{q_t(f_2 - G_2)} = \lambda \qquad \text{for } t = 1, 2, \cdots, n. \tag{5}$$

U_t' refers to the marginal utility of absorption with respect to the tth period's absorption. G_2 refers to the *total* change in absorption induced by a change in the price of foreign exchange during period t. Variable f_2 is the effect of p upon y.

$$G_2 = \overset{\text{first term}}{\frac{\partial a_t}{\partial y_t}\frac{dy_t}{dp_t}} + \overset{\text{second term}}{\frac{\partial a_t}{\partial p_t}}. \tag{6}$$

The first term indicates the effect of a change in the price of foreign exchange upon the real value of currently produced output and the marginal propensity to absorb out of the real value of currently produced output. This term expresses the indirect effect of p upon a. On the other hand, the second term is the direct effect of p upon a.

The denominator in (5) is the change in the money value of the current account balance, since f_2 is dy/dp and G_2 is da/dp.

Our maximization conditions (5) state that:

$$\frac{\text{the change in utility (per unit change in } p)}{\text{the change in the balance (per unit change in } p)} = \frac{U'G_2}{q(f_2 - G_2)} = \lambda$$

should be the same in each of the n time periods. Suppose that the price of foreign exchange rose and produced a current account surplus of \$X in period one. Given idle resources the real value of current production would rise. This rise would induce an increase in absorption and in total utility. Since the money balance over the entire n periods must be zero, the price of foreign exchange would have to fall in a subsequent period to produce a deficit of \$X. As a result of the idle resources effect, the real value of current production would decline. The induced decline in absorption would decrease total utility. For the prices of foreign exchange to be optimal, the rise in total utility during one period should be equal to the decline in utility during the other period. Otherwise, total utility could be raised without violating the requirement that the balance of payments be in equilibrium over the entire set of n periods.

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[6] In Section II the optimum y^* is considered.

C. *A Specific Formulation and the Ideal Price Line*

An exact specification of the functions in equations (A1), (A2), and (A3) permits us to solve explicitly for the ideal prices of foreign exchange. Consider a two-period model (the present and the future) of the following sort:

$$u = \ln a_1 a_2 \tag{A1.1}$$

$$a = c_1 y - c_2 p \qquad c_1,\ c_2 > 0 \tag{A2.1}$$

$$y = y^* + e_1 p \qquad e_1 \geqq 0 \tag{A3.1}$$

where p is measured from some arbitrary number,

$$\sum_{t=1}^{2} q_t(y - a)_t = 0. \tag{A4}$$

Maximize the Lagrangian function W, where

$$W = \ln a_1 a_2 - \lambda \sum_{t=1}^{2} q_t(y - a)_t \tag{1.1}$$

with respect to p_1 and p_2. For simplicity, let the over-all price index in each period be the same,[7] although the terms of trade can and do indeed vary between periods.

The ideal or optimum prices of foreign exchange are found from the necessary conditions for an interior maximum.

$$\frac{\partial W}{\partial p_1} = \frac{\partial W}{\partial p_2} \tag{2.1}$$

and from the constraint

$$q_1(y_1 - a_1) + q_2(y_2 - a_2) = 0. \tag{A4}$$

From (2.1) we obtain

$$p_1 - p_2 = \frac{-c_1}{(c_1 e_1 - c_2)} (y_1^* - y_2^*) \tag{3.1}$$

and from (A4) we obtain

---

[7] Insofar as q, y, and a are vectors, the stability of the over-all index requires that $q_1 \cdot v = q_2 \cdot v$, where v is the vector of weights given to the various prices.

$$(4.1) \qquad p_1 + p_2 = \frac{-(1 - c_1)}{e_1 + c_2 - e_1 c_1} (\overset{*}{y}_1 + \overset{*}{y}_2).$$

Solving for p_1, and substituting (Y) the mean value of y^* for y_2^*,[8] we obtain *the ideal price line:*

$$(5.1) \qquad p_1 = \frac{-c_1}{c_1 e_1 - c_2} \overset{*}{y}_1 + \frac{c_2}{(c_1 e_1 - c_2)[e_1(1 - c_1) + c_2]} Y.$$

The slope of the ideal price line can be negative or positive, depending upon the magnitude of the idle resources effect relative to the magnitude of the effect of a change in the terms of trade upon absorption, when the real value of current output is given. When the price of foreign exchange is raised there are two countervailing effects. On the one hand the depreciation is an inflationary device which tends to raise the real value of output and hence the real value of absorption. This effect is

$$c_1 e_1 = \frac{\partial A}{\partial y} \frac{dy}{dp} \cdot$$

On the other hand, suppose that y were fixed at a full employment level. When the price of foreign exchange is raised, there is an adverse movement of the terms of trade. This adverse movement lowers real income and hence reduces absorption by $c_2 = \partial A / \partial p$. Call this the terms of trade effect upon absorption.

The term $(c_1 e_1 - c_2)$ in (5.1), which is the same as G_2 in (6), represents the total effect of a change in p upon absorption. In a Keynesian world of idle resources G_2 is positive. Currency depreciation will expand production and the real value of absorption. The greater the elasticity of aggregate supply, the more likely it is that G_2 will be positive. At the other extreme, consider a full employment world with no misallocation of resources; i.e., y is fixed. Consequently $e_1 = 0$ and G_2 is negative. Currency depreciation, which deteriorates the terms of trade, will reduce real absorption.

Simplify (5.1), the ideal price line, to

$$(6.1) \qquad p_1 = - H_1 \overset{*}{y}_1 + H_2 Y.$$

In a full employment world H_1 and H_2 are negative, giving a positive slope to the ideal price line. When the idle resources effect is dominant (a Keynesian world), H_1 and H_2 are positive, giving a negative slope to the ideal price line. For simplicity, assume that Y, the mean value of y^*, is zero. Then the ideal price line is *RR* when the idle resources

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[8] $Y = \frac{1}{2}(y_1^* + y_2^*)$.

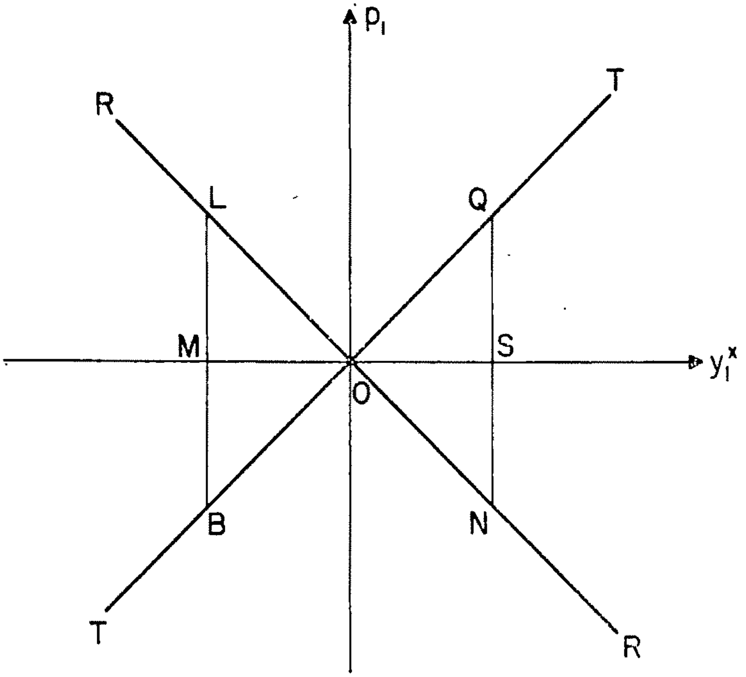

FIGURE 1. IDEAL PRICE LINES.
RR exists when there are idle resources.
TT exists when there is full employment.

(Keynesian) effect is dominant, and *TT* when the terms of trade (full employment) effect is dominant (Figure 1).

The logic of the ideal price line can be given a literary explanation. This line gives the pair (p_1, y_1^*) which maximizes utility and equilibrates the balance of payments. Suppose that y_1^* rises to *OS* (Figure 1), given a mean $Y=0$. If the price of foreign exchange (measured from an arbitrary origin) remained at $p_1=0$, then absorption would increase in period one relative to period two. Given declining marginal utility schedules, the marginal utility of absorption in period one would be below that prevailing in period two. Optimality requires that absorption be smoothed out over the various periods. If the idle resources effect is dominant, absorption in period one can be reduced through a decline in the price of foreign exchange[9] to *SN*. Aggregate demand would decline as the demands for exports and for home-produced goods decline. This decline would induce a contraction of the real value of current production and induce a decline in real absorption. As a result of the decline in the price of foreign exchange, the effect of a rise in y_1^* is offset in terms of real absorption.

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[9] Currency appreciation is a deflationary device in an economy with idle resources.

On the other hand, suppose that we are in a world of full employment where the terms of trade effect is dominant. When y_1^* rises to OS, real absorption tends to rise in period one relative to period two. Optimality requires that absorption be reduced in period one and raised in period two. A rise in the price of foreign exchange to SQ (Figure 1) will deteriorate the terms of trade and reduce real absorption, out of a given y.

II. *The Efficiency of Alternative Exchange Markets*

A. *Conflict and Compatible Economies*

A dichotomy has been drawn between the Keynesian case, where the idle resources effect is dominant, and the full employment case, where the terms of trade effect upon absorption is dominant. In the former case, the ideal price line is negatively sloped with respect to the (p_1, y_1^*) axes; and, in the latter case, the ideal price line is positively sloped.

A further dichotomy must be made between types of economies for purposes of specifying the optimum foreign exchange market. In a "conflict economy" there is a conflict between the goals of internal and external stability. When income and employment are rising, an excess supply of foreign exchange develops at the given exchange rate. When income and employment are declining, an excess demand for foreign exchange develops at the given exchange rate. The "export economy" described by Henry C. Wallich [5] is one example of a conflict economy. In an export economy, the value of exports (or of exports plus imports) is a greater fraction of GNP than is the value of gross private domestic investment. The major exogenous disturbance in this type of economy is the value of exports, which is the effective multiplicand in the income-generating process. Consequently, the balance on current account and the value of y^* are positively related. When exports rise, income increases and a current account surplus develops. As a result of the accumulation of foreign exchange reserves, which constitute the reserves of the banking system, interest rates tend to decline during the expansion. Nevertheless, the short-term private capital outflow does not offset the basic balance surplus.

A conflict between internal and external stability could exist in a non-export economy, where fluctuations in gross private domestic investment produce income fluctuations. Suppose that a rise in the marginal efficiency of investment (1) led to an increase in the rate of gross private domestic investment, and (2) induced a short-term private capital inflow into a booming stock market. The balance on current account can be expected to deteriorate as imports rise with income. Nevertheless, the short-term private capital inflow could exceed the decline in the current account balance. Let us define a conflict economy as one where the covariance between y and the excess supply of foreign exchange is positive, given an exchange rate. Alternatively, a conflict economy could be defined as one where the covariance between y^* and the excess supply of foreign exchange is positive—since y^* is the level of y that would prevail if p were zero.

The dual to a conflict economy is a "compatible economy," where there is no conflict between the measures required to produce internal and external stability. During the expansion phase of the business cycle, there develops an excess demand for foreign exchange; and during the contraction phase, there develops an excess supply of foreign exchange (given an exchange rate). For example, the business cycle could be produced by variations in gross private domestic investment; and the balance on current account could conform inversely with the cycle. Although interest rates may conform positively with the cycle, the inflow of short-term private capital may be insufficient to offset the decline in the current account balance during the expansion; and the outflow of short-term private capital may be insufficient to offset the rise in the current account balance that occurs during the contraction. Consequently, exchange depreciation tends to occur in the upswing, and exchange appreciation tends to occur in the downswing of the cycle, in a free exchange market.

Define a compatible economy as one where the covariance between y^* and the excess supply of foreign exchange is negative.

In a Keynesian economy (where the idle resources effect is dominant), absorption is positively related to the price of foreign exchange. This relation occurs because currency depreciation increases aggregate demand. The optimum foreign exchange market will be the one that tends to raise the price of foreign exchange in recessions, and to lower the price during expansions, by the "correct" amounts.

Define the inefficiency (I) of a foreign exchange market as the sum of the absolute values of the deviations between the actual and the ideal price $p = -H_1 y^* + H_2 Y$. That is:

$$I = \sum_{t=1}^{n} \left| p_t + H_1 y_t^* - H_2 Y \right| .$$

It is proved that, in the Keynesian case: (1) a free exchange market[10] with perfect foresight and no opportunity costs of investing in foreign exchange is formally identical to a market stabilized[11] at the equilibrium rate; (2) inefficiency tends to be smaller with a free exchange market than with a stabilized market, in a conflict economy where there is a reasonable degree of uncertainty; and (3) inefficiency is smaller in a stabilized market than in a free exchange market, in a compatible economy, if the rate is stabilized at an equilibrium rate of exchange.

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[10] A free exchange market will be defined as one where the government does not attempt to stabilize the exchange rate through purchases or sales of foreign exchange, regulations upon the private sector, or through adjustments in the level of aggregate demand.

[11] A stabilized exchange market exists (1) when the government buys and sells foreign exchange in such quantities as to keep the price of foreign exchange in the neighborhood of a fixed price, or (2) when at least two governments stabilize the price of a freely traded commodity in terms of domestic currency, and imports and exports of this commodity are free from governmental restrictions. Whether stabilization is direct or indirect is immaterial here.

B. *The Keynesian Case: The Idle Resources Effect Is Dominant*

Variations in y^* (i.e., the real value of current production that would occur if p measured from an arbitrary origin were zero) occur in a Keynesian economy as part of the business cycle. When y^* declines from O to M (Figures 2a and 2b), the economy contracts towards the trough. When y^* rises from O to S, the economy expands towards the peak.

Under the best of conditions, a stabilized market produces inefficiency if y^* is not constant. Suppose that the value of y^* fluctuated between M and S with a mean y^* equal to $Y=0$. Then $\bar{p}=0$ would

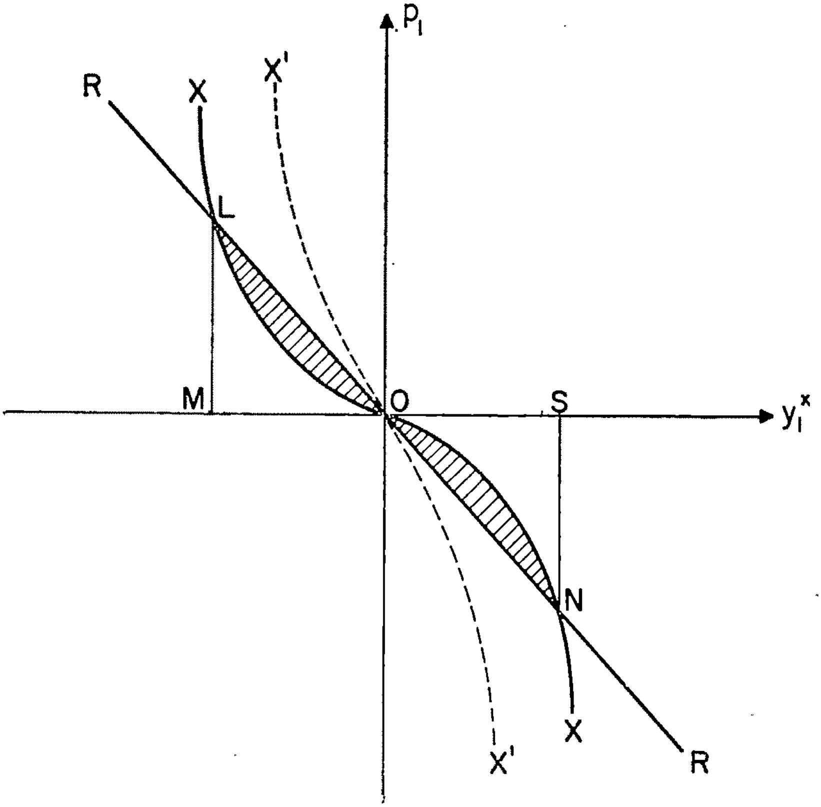

Figure 2a. Keynesian Case: Conflict Economy.

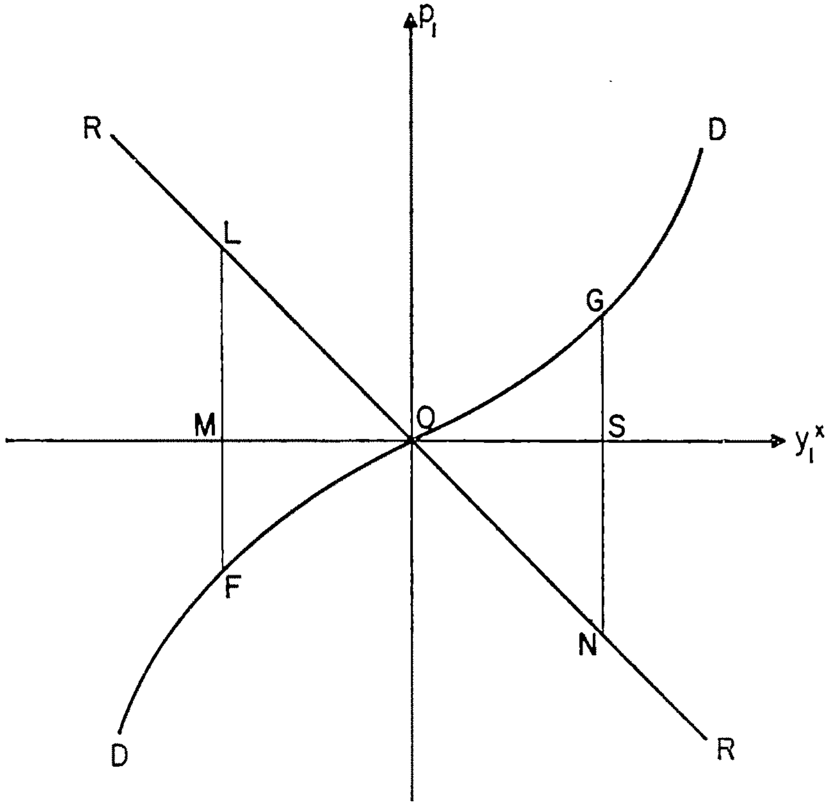

FIGURE 2b. KEYNESIAN CASE: COMPATIBLE ECONOMY.

produce balance of payments equilibrium over the entire set of n periods.[12] If the exchange authorities had perfect foresight and desired a stabilized rate that would produce no net change in the gold reserves over the entire n periods, they would select $\bar{p}=0$ (where $\bar{p}$ is measured from an arbitrary origin). Then the deviation of the actual price from the ideal price is equal to the absolute value of the distance of the ideal price line *RR* from the horizontal axis. This inefficiency is equal to areas *LOM*+*NOS* in Figure 2a.

A stabilized market is inefficient because the price of foreign exchange (1) failed to rise during the contractions, and thereby failed to stimulate production and absorption; and (2) failed to decline during the expansions, and thereby did not depress production and absorption.

How would the exchange rate fluctuate in a free exchange market? If there were perfect certainty, and no opportunity costs involved in the purchase or sale of foreign exchange, then the free market price would be constant at the price that equilibrated the demand and supply of foreign exchange over the entire n periods. Let this price $p=0$. If the price tended to fall below $p=0$ during the export season, speculators would buy unlimited quantities of foreign exchange and force the price up to $p=0$. By assumption, there are no opportunity costs involved in holding foreign exchange (e.g., foreign exchange can earn as much as capital tied up in the home industry), and there is perfect foresight. Similarly, if the price of foreign exchange tended to rise above $p=0$ during the import season, the speculators would borrow to go short (sell) unlimited quantities of foreign exchange to bring the price back to $p=0$. For this reason, a free market under certainty (i.e., perfect foresight) with no opportunity costs involved in holding foreign exchange is formally equivalent to a stabilized market at the equilibrium price.

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[12] Equation (4.1) gives the mean price $\frac{1}{2}(p_1+p_2)=\bar{p}$ which equilibrates the balance of payments when $\bar{Y}=\frac{1}{2}(y_1^*+y_2^*)=0$. It is seen that $\bar{p}=0$.

To contrast the effects of free and stabilized markets, one must therefore assume that there is not perfect foresight (a realistic assumption). The free market prices that would prevail in a conflict economy, with uncertainty, are given by curve XX in Figure 2a. This assertion can be supported as follows.

Let y^* vary between M and S with a mean of zero. When y^* exceeds the mean value, in a conflict economy, there will tend to be an excess supply of foreign exchange at $p=0$. Variations in y^*, in such an economy, could result from variations in the value of exports. Hence, the price of foreign exchange would tend to fall below its mean; and p would be negative. Due to a principle of increasing risk, the price would fall at an increasing rate for increases in y^* or the value of foreign exchange offered for sale. Similarly, when y^* is below its mean, there would tend to be a deficit at a price $p=0$; and the price of foreign exchange would rise. Again, due to a principle of increasing risk, the price would rise at an increasing rate for decreases in y^*. As a result, the free market would produce prices given by the XX curve. The risks involved in taking positions in the foreign exchange market determine the shape of the XX curve. Curve $X'X'$ is associated with more risk than is curve XX.[13]

Inefficiency is defined as the sum of the absolute values of the deviations between the actual prices (given by the XX curve) and the ideal price line RR. Inefficiency in a free market, in a conflict economy with a reasonable degree of uncertainty, is the shaded area in Figure 2a. If the price were stabilized at $p=0$, or if the free market had perfect foresight, inefficiency would be equal to $LOM+NOS$. This proves the assertion that a free market is more efficient than a stabilized market in a Keynesian-conflict economy where there is a reasonable degree of uncertainty. A free market is superior, in such an economy, because (1) the currency depreciates (p rises) during contractions and thereby increases aggregate demand and (2) the currency appreciates (p falls) during expansions and thereby reduces aggregate demand. As a result, absorption and output are smoothed over the cycle.

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[13] See [4] for an analysis of how professional risk bearers operate in the foreign exchange market.

Conceivably, the curve $X'X'$ (Figure 2a) might describe the path of free market prices. The inefficiency in a market with such a curve could exceed the inefficiency produced in a stabilized market. One can virtually exclude this case from the realm of possibility. A free market would be less efficient than a stabilized market, in a Keynesian-conflict case, if the fluctuations in absorption were greater in the former market. For this to occur, the depreciation that would have to result when y^* was below the mean must be sufficient to produce a y and an a considerably above the mean, and the appreciation that would have to result when y^* was above the mean must be sufficient to produce a y and an a considerably below the mean. This possibility is most unlikely to occur.

In a compatible economy, there tends to be a negative relation between the excess supply of foreign exchange (given p) and the value of current production. What will be the set of exchange rates produced by a free exchange market? When there is uncertainty, the price of foreign exchange will be positively related to the level of y^*. When y^* is above the mean, there tends to be a shortage of foreign exchange (at $p=0$) and the price rises above the mean. Similarly, when y^* is below the mean, there tends to be a surplus of foreign exchange (at $p=0$) and the price falls below the mean. A principle of increasing risk is assumed, whereby speculators will increase their positions by equal increments only if the price changes by increasing increments. Uncertainty and diminishing marginal utility conspire to produce the DD curve in Figure 2b. This curve relates free market prices of foreign exchange to values of y^*, in a Keynesian-compatible economy.

Again let y^* vary between M and S, with a mean of zero, during the business cycle. Since we are assuming a Keynesian economy where the idle resources effect is dominant, the ideal price line is RR. Inefficiency is $LOF + GON$, in Figure 2b; it is the sum of the absolute values of the deviations between the actual and ideal prices of foreign exchange. This inefficiency is greater than the inefficiency that would have prevailed in a market stabilized at $p=0$, $LOM+NOS$. On the other hand, if the market is stabilized at a price other than $p=0$, when the mean $Y=0$, then the balance of payments constraint would be violated. Moreover at any p other than $p=0$, inefficiency would exceed $LOM+NOS$.

Critics of stabilized exchange markets generally claim that it is extremely improbable that the monetary authorities will find the rate that will balance the payments over the entire set of n periods, given $(y_1^* \cdots y_n)$.

A free exchange market is inefficient in a Keynesian-compatible economy because currency depreciation occurs during expansions and thus stimulates output and absorption, and currency appreciation occurs during contractions and thus inhibits output and absorption. Variations in absorption are aggravated by a free market, in such a case.

C. *The Global Maximum Social Utility*

In the models developed in Section I, the y^* vector was an independent variable. The logic of the ideal price line called for variations in p to maximize total utility, given y^*. All too often, the y^* vector and the stabilized rate of exchange are not independent. Suppose that the exchange authorities select a price of foreign exchange at random (p') and then adjust aggregate demand y^* to satisfy the balance of payments constraint (4.1) above. The defense of an arbitrary exchange rate becomes the object of public policy. Why is this not optimal?

At first glance, it would seem that such a policy could satisfy (5.1) above. If a $p'=0$ were chosen, the government conceivably could adjust y^* in each period to equal zero. Then the balance of payments would be in equilibrium, so that (4.1) above would be satisfied, and the optimum conditions (3.1) would be satisfied.

What is wrong with such a supposition is that a fuller set of maximization conditions, than is specified by (5) above, is required when y^* is subject to public control, e.g., through monetary and fiscal policy. Equations (5) require that

$$\text{(a)} \qquad \frac{\partial W}{\partial p_t} = 0, \qquad t = 1, \cdots, n.$$

When y^* can be controlled, then the additional conditions

$$\text{(b)} \qquad \frac{\partial W}{\partial y_t^*} = 0, \qquad t = 1, \cdots, n$$

must also be satisfied.

The conditions for a maximum of total utility subject to the balance of payments constraint are:

$$\text{(7)} \qquad \frac{U_t' G_1}{f_1 - G_1} = \frac{U_t' G_2}{f_2 - G_2} = \lambda, \qquad t = 1, \cdots, n.$$

This means that p and y^* should be varied in each period such that the change in total utility (per dollar change in the money balance) should

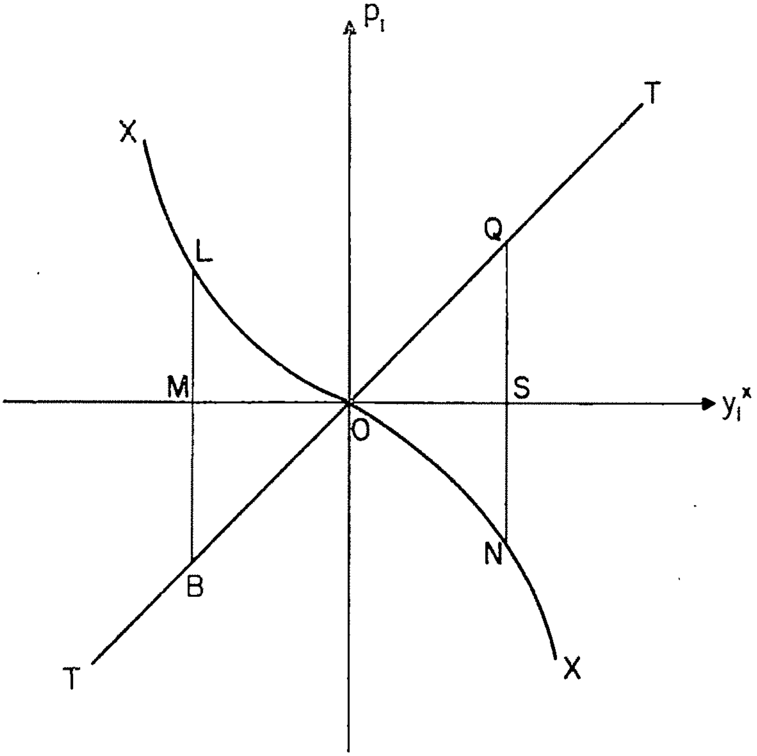

FIGURE 3. FULL EMPLOYMENT: CONFLICT ECONOMY.

be the same for a variation in p as it is for a variation in y^*.[14] The satisfaction of (7) is a necessary condition for a global maximum. A policy of stabilizing p at some arbitrary value, and adjusting y^* to preserve balance of payments equilibrium, has been shown to violate the necessary conditions (7) for a global maximum total utility. It is inefficient to resolve the conflict between internal and external stability by contracting or expanding y^*, given the stabilized exchange rate.

D. *A Full Employment Case*

The second case is one where full employment always prevails. Nevertheless, the productivity of the labor force can vary seasonally or secularly. Since y cannot be influenced in the short run by variations in the exchange rate, the terms of trade effect is dominant. An improvement in the terms of trade (i.e., a decline in p) raises real income and absorption out of a given real production. For the reasons developed in Section I(C), the ideal price line is the positively sloped *TT* line.

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[14] If the equations were linear, a unique solution would exist if the matrix were nonsingular. There are $2n$ equations plus the balance of payments constraint to determine n values of p, n values of y^*, and one λ. A geometric picture of (7) can easily be supplied. The $U'G_1/U'G_2$ would be described by utility indifference curves and $(f_1-G_1)/(f_2-G_2)$ would be described by equal balance curves. Equilibrium would exist when utility cannot be increased without violating the balance of payments conditions.

Ideally, absorption should exceed production when y^{15} is below the mean. When y is above the mean, absorption should be reduced below production. Thereby, absorption would be evened out over the entire set of n periods. The capital imports of the early period would be repaid with capital exports during the later period.

To encourage absorption during the early period (when y is below the mean), the terms of trade should move in favor of this country. Real income would rise, given y; and absorption would be increased. Later when y exceeds the mean, the terms of trade should deteriorate. Real income would decline, given y; and absorption would be reduced.

Suppose that there is no trend in the full employment level of output; but y varies seasonally around a zero mean. During the harvest season it rises from M to S; and then it declines from S to M (Figure 3). The varying productivity of labor accounts for the fluctuations in y.

The economy described is an export or conflict economy (see IA and [5]). When production y is above the mean in the harvest-export season, foreign exchange is abundant. When y is below the mean in the post-harvest (post-export) season, there is a shortage of foreign exchange. In a free market, the actual price of foreign exchange will be given by the *XX* curve in Figure 3.

Inefficiency in a free market is $LOB+QON$ whereas it is only $MOB+QOS$ in a market stabilized at $p=0$. A free market is less efficient because the terms of trade are positively associated with the level of output. Variations in absorption are thereby aggravated as both y and $1/p$ move in the same direction. By stabilizing p at $p=0$, one source of variation is reduced. Ideally, to smooth absorption, the price of foreign exchange should vary directly with y along the *TT* line. But such a movement occurs in neither the free nor the stabilized market.

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[15] Since $y=f(y^*, p)$ is always at full employment, p cannot affect y. Hence, the distinction between y and y^* can be eliminated.

COMMUNICATIONS

A Note on Public Goods Supply

The theory of economic policy upon which arguments for the collectivization of any activity must be based embodies the prediction that the behavior of individuals in markets does not produce socially desirable results. In the orthodox analysis, this prediction stems from the presence of significant externalities that the market is presumed unable to internalize. In his independent behavior, the individual is assumed to take into account only the effects of his actions on his own utility or that of his family group. From this it follows that, if private behavior exerts Pareto-relevant external economies,[1,2] the market-generated supply of resources to the activity in question falls short of the "social optimum," as this is defined by the Paretian criteria. For example, if the citizens of a social group secure, generally, genuine benefit from the existence of a healthy, disease-free population, the behavior of individuals in purchasing health services independently in private markets appears to commit, relatively, too small a share of total community resources to the provision of such services.

In this note, we shall demonstrate that this orthodox policy implication is not completely general, and that, in certain circumstances, it may be in substantial error. Independent or market organization of an activity that is acknowledged to embody relevant external economies need not result in an undersupply of aggregate resource inputs, relative to that amount required to satisfy the necessary marginal conditions for Pareto-optimality.

We shall examine alternative institutional arrangements through which one particular service may be provided. For expository purposes, we may consider the utilization of a type of medical care that will reduce the probability that the individual will catch a communicable disease. We analyze two separate cases. In the first, the consumption of the service by one person, say B, is assumed to exert relevant external economies on another person or persons, say A, but this relationship is *not reciprocal*. The consumption of the same service by A does not exert relevant external economies on B. For example, A, the "rich man," may find his utility affected by B's consumption of the service, whereas B, the "poor man," may not be affected, marginally, by the extent to which A himself purchases the service. The second case is that in

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[1] For definition of Pareto-relevant externalities, see Buchanan and Stubblebine [1]. Briefly, an externality is Pareto-relevant when the party or parties enjoying an externally-imposed benefit (suffering an externally-imposed damage) can be made better off without the acting party or parties being made worse off. Favorable interpretation of the literature suggests that economists have, generally, meant to refer only to Pareto-relevant externalities when they discuss external economies and diseconomies. Otherwise, most of the orthodox analysis is seriously deficient.

[2] The discussion in this note is limited to the external economies case since it is upon this that the collectivization argument is normally based.

which the relationship is *reciprocal*. The utilization of the service by B exerts relevant marginal economies on A, as in the first case, but, also, A's consumption of the same service exerts relevant marginal economies on B.

The first and most obvious point to be made is that if an individual receives external or spillover benefits from the activity of others, this affects his utility and may lead him to change his allocation of income among the various goods and services available. If the activity of others generating the spillover benefit is a substitute for some of his own, he will reduce the latter as the activity of others is increased. An increase in the level of immunization privately undertaken by B will tend to reduce the purchase of medical services, say vaccinations, by A. The degree to which the alternative institutional arrangements facilitate the mutually-desirable adjustments of consumption in the presence of such substitutability determines their relative efficiency in the exploitation of the external economies. It will be convenient to discuss the nonreciprocal and the reciprocal cases separately and with different tools of analysis.

I. *Nonreciprocal Case*

We discuss the nonreciprocal case in terms of a two-person geometrical model. We shall, however, treat this model as representative of the *n*-person model, and we shall not be concerned with considerations of strategic behavior. In Figure 1A, the curve, D_a, represents the marginal evaluation of individual A for a final output which, in this model, we label "Healthy Days Per Year." Throughout the analysis, we neglect income effects, allowing us to use marginal evaluation curves as demand curves. This demand curve, D_a, can also represent the demand for health-service inputs, which we define in units of the size necessary to provide one healthy day. The ordinate measures the price of these inputs, along with A's marginal evaluation. Input units are available at constant marginal cost. Assume, initially, that A considers him-

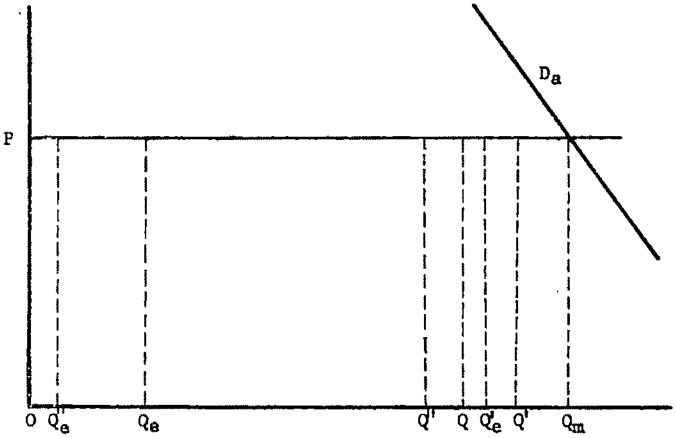

FIGURE 1A

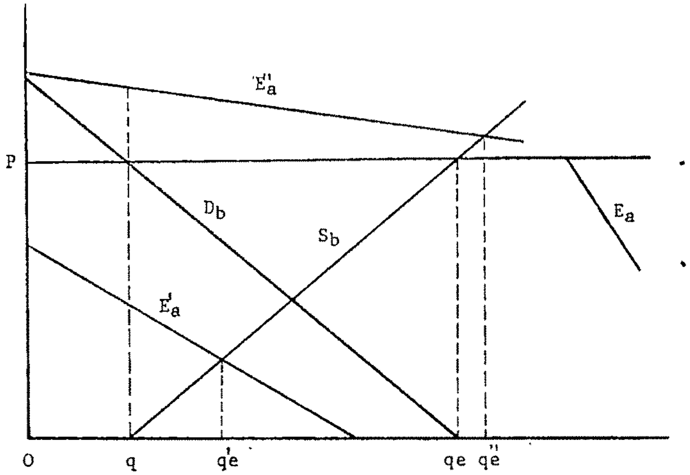

FIGURE 1B

self to live in isolation from B. He will purchase a quantity, Q_m, of health-service inputs, yielding an output, Q_m, of healthy days per year.

By assumption, B does not experience any spillover benefits from A's action. He will purchase a quantity, q, of health-service inputs, shown in Figure 1B, providing to him a like amount of final output. This is clearly not an equilibrium for the group, however, since A will recognize that B's performance of the activity is providing him with certain spillover benefits at the margin, and, under the assumptions of the model, these are substitutes for A's own purchases of health services.

Initially, we assume that A considers B's utilization of an input unit to be a perfect substitute for his own direct utilization of a like unit. That is to say, B's use of one additional input unit allows A to maintain the same quantity of healthy days per year by reducing his own direct use by one unit. Thus, A will reduce his own direct consumption from Q_m to Q, as shown in Figure 1, with this difference being exactly equal to q. This position, described by A's purchasing Q and B's purchasing q, is the equilibrium that is attained through wholly independent adjustment.

We now compare this position with that which might be attained under an alternative institutional arrangement that allows full advantage of the external economies to be taken. If interpersonal markets should become activated, this would provide a means of exploiting such externalities, and the analysis will be developed in terms of such markets. More traditionally, we may assume that some "ideally" operating collective arrangements produce the same results. Individual A will recognize that, at the independent equilibrium, he can secure additional benefits through some extension of B's consumption. Retaining the assumption that B's use of an input is a perfect substitute for his own, we now examine A's evaluation of B's activity. In this instance, A would be prepared to pay any amount below the price of an input to himself in exchange for B's marginal extension of the activity. The marginal evaluation curve—that is, A's demand for B's activity—would lie along the horizontal marginal cost or price curve, out to the point at which this intersects with A's demand curve for final output.

curve, "The [illegible] cost curve

supply curve

How much compensation will be required to encourage B to extend his direct purchases of health-service inputs? The supply curve for B's services that confronts A may be derived by subtracting B's own demand curve from the marginal cost curve, constant at the given price per input unit in this model. This supply curve is shown as S_b in Figure 1B. Equilibrium is attained when B purchases q_e units of health service. A will, in this solution, reduce his own direct input purchases to Q_e, which is less than Q by exactly the difference between q_e and q.[8] The answer to our basic question in this set of circumstances is clear. The solution that allows the full exploitation of the external economies embodies the same total resource use as that which is involved in the independent market adjustment, provided that, in the final solution, A continues to utilize directly some positive quantity of inputs. The distribution of resource use and the total amount of final output are, of course, different in the two solutions. B enjoys more healthy days than under independent adjustment, and A enjoys the same amount, although his own direct purchases of inputs have been reduced *pari passu* with the expansion in B's purchases.

The analysis indicates that, in this type of model, it is necessary to distinguish carefully between *units of resources supplied* and *units of final output* that may be consumed. In ordinary market organization, because of the assumed divisibility of benefits in consumption, these two dimensions are equivalent; a unit supplied to the consumer by the seller is identical, physically, to the unit that is consumed. Thus, the total number of units supplied to all purchasers is equal to the total number of units consumed by all persons. This direct relationship between inputs and outputs is, however, modified when some interpersonal substitutability in consumption is allowed, as must be the case with external economies. The very word "economies" suggests that a greater quantity of final output can be achieved with a given quantity of resource inputs when these economies are exploited.

We may now extend the analysis to allow for either less-than-perfect or more-than-perfect substitution between the inputs by B and those by A, in the utility function of A. If A recognizes that B's utilization of health-service inputs will yield external benefits to him, but that these still are not so productive of health as those stemming from his own utilization of inputs, his marginal evaluation curve for B's activity will lie below the price. One such curve is shown as E'_a in Figure 1B. In this situation, equilibrium is attained with B's purchase of the amount, q'_e, subsidized by A over the range beyond B's own desired independent purchases. In this case, A will find it possible to reduce his own direct purchases, but he will not reduce these sufficiently to offset the expansion in B's resource use. Hence, A will reduce his own direct resource use from Q', the position reached under independent adjustment in this case, to Q'_e. Total resource commitment will be expanded as a result of the exploitation of the externalities.

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[8] The analysis of individual adjustment in the presence of external economies has been developed in a somewhat different context by Milton Z. Kafoglis [7, pp. 33-38].

whole cost

If, however, B's utilization of health services should prove to be more-than-perfect substitutes for A's own direct consumption, these results are reversed. The marginal evaluation curve of A for B's activity will lie above the price, as is shown by E''_a in Figure 1B. Here equilibrium is attained with the amount q''_e being provided by B. Individual A will, in this instance, reduce his own activity by more than the amount of additional activity undertaken by B. The final result will embody a smaller resource use, a smaller total expenditure, than that produced under independent adjustment. As in the other two models, A will continue to enjoy the same total quantity of final services, healthy days per year, and B will, of course, enjoy substantially increased amounts of final services as he utilizes directly more inputs. Because of the extreme substitutability that is present, however, this result can be attained with a smaller total outlay than before.[4, 5]

What [illegible]

This third model is not nearly so implausible as it might initially appear. In the immunization example, it seems clear that, under certain conditions, private organization would generate an overextension of immunization on behalf of some groups in the population that might be more than proportionately reduced in response to an extension of immunization on behalf of other groups.

II. *Reciprocal Case*

We now turn to the case where reciprocal external economies are present. In the two-person model, the consumption of A yields relevant external economies to B while, at the same time, the consumption of B yields relevant external economies to A. As we did in the analysis of Section I, we assume that there are no scale effects present in the utilization of inputs. That is, were A

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[4] It is, of course, possible that B's activity might be a more-than-perfect substitute for A's own at certain levels of provision and a less-than-perfect substitute at other levels. In this case, the appropriate E_a curve in Figure 1B would cut the marginal cost curve, and the final solution might embody either more or less aggregate resource use depending on the particular configuration.

[5] In each of the models considered, individual A continues to enjoy the same quantity of final services, "healthy days per year," as indicated by Q in Figure 1A. As B's direct utilization of inputs is allowed to substitute for his own, the average price per unit of final service consumed, to A, falls. This decline in average price might suggest that A would move down his demand curve for the final service as this substitution takes place. His decision will depend, however, on marginal price, not average price, and, so long as he continues to purchase directly for himself any health-service inputs, "healthy days" are available to him only at the marginal price shown by P. Thus, since marginal price does not change, total amount of final services demanded does not change, given our previous assumption that income effects may be neglected. If the relationships were such that, as B's utilization of inputs substitute for his own, A could cease all of his own direct input purchases, marginal price would decline. In this case, the elasticity of A's demand for final services as well as the elasticity of substitution between B's resource use and his own would be relevant in determining the direction of change in resource outlay that the cooperative solution would generate.

and B to act jointly they could not purchase the inputs more cheaply than they could with each acting privately.

As in the nonreciprocal case, we examine the process through which the various members of a group attain equilibrium when we assume that only independent action is possible. The individual will not take into account here the external benefits that his own decisions impose on others. However, he will adjust his own behavior in the light of actions taken by others. As in the earlier model, we limit analysis to the situation where the relationship is one of substitution. Each individual will extend his own independent activity in purchasing inputs in some inverse relationship to the quantity of activity that he expects others in the group to generate.

We have, in this model, an example of "nonseparable" externality, defined by Davis and Whinston [3],[6] and, as their analysis suggested, the familiar economic policy analysis cannot be readily applied. In all such nonseparable cases, it is essential that the interactions among individual decisions be carefully examined. The tools of game theory can be usefully employed in this respect. The details of analysis and the determinacy of solution need not be explored in this note since we seek only to demonstrate that collective organization, even if ideally operated, ***need not*** result in greater total outlay on provision of the final service than that which independent adjustment would generate. This, of course, does not deny that, in many cases, the satisfaction of the necessary marginal conditions for Pareto optimality may require greater total outlay. In some cases it will surely do so; in others it will not.

The demonstration is presented in the numerical example shown in Figure 2. The example is presented for the two-person case, but the results can be generalized without difficulty; and, throughout, we use the two-person model as representative of an n-person model, allowing us to eliminate considerations of strategic behavior. Each of two persons in the group, A and B, is presumed to be able to take any one of five separate actions, as indicated by the five-by-five matrix. For simplicity in exposition, an example similar to the earlier one will be helpful. Suppose that the activity in question is that of getting a series of immunization shots that provide partial, but never complete, protection against some communicable disease. Each person can either do nothing, as indicated by the a_0 row and the b_0 column in the matrix, or he may get one, two, three, or four shots.

It is convenient here to measure payoffs in negative terms, that is, in terms of expected costs rather than expected benefits. Hence, players will attempt to minimize the payoffs shown in the matrix. In the upper left-hand corner of each square, we measure the expected value of the costs that catching the disease will impose on each of the two persons, A and B. These are determined by the probability of getting the disease, given the level of immunization undertaken by each person, along with the costs of the disease, if it is contracted. These costs, for A, are shown without parentheses; those for B are shown with parentheses. Since the two individuals are not assumed to be

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[6] In an unpublished manuscript [4], Davis and Whinston utilize game theoretic models similar to those that we introduce here. They do not, however, address themselves specifically to the issue here analyzed.

B A	b_0	b_1	b_2	b_3	b_4
a_0	100 $\underline{200}$ (100) 0 100 (0) (100)	60 $\underline{120}$ (50) 0 60 (10) (60)	45 $\underline{90}$ (25) 0 45 (20) (45)	30 $\underline{76}$ (16) E 0 30 (30) (46)	29 $\underline{77}$ (8) 0 29 (40) (48)
a_1	60 $\underline{140}$ (70) 10 70 (0) (70)	45 $\underline{110}$ (45) 10 55 (10) (55)	35 $\underline{85}$ (20) 10 45 (20) (40)	28 $\underline{81}$ (13) 10 38 (30) (43)	25 $\underline{82}$ (7) 10 35 (40) (47)
a_2	40 $\underline{110}$ (50) 20 60 (0) (50)	32 $\underline{91}$ (29) 20 52 (10) (39)	23 $\underline{78}$ (15) M 20 43 (20) (35)	19 $\underline{77}$ (8) 20 39 (30) (38)	18 $\underline{84}$ (6) 20 38 (40) (46)
a_3	29 $\underline{99}$ (40) 30 59 (0) (40)	23 $\underline{86}$ (23) 30 53 (10) (33)	18 $\underline{82}$ (14) 30 48 (20) (34)	17 $\underline{83}$ (6) 30 47 (30) (36)	16 $\underline{91}$ (5) 30 46 (40) (45)
a_4	20 $\underline{90}$ (30) 40 60 (0) (30)	17 $\underline{89}$ (22) 40 57 (10) (32)	16 $\underline{89}$ (13) 40 56 (20) (33)	15 $\underline{89}$ (4) 40 55 (30) (34)	14 $\underline{97}$ (3) 40 54 (40) (43)

FIGURE 2

identical, these costs will differ.[7] Note that, as the example is constructed, expected costs fall, for each individual, as either one or both of the two persons get additional shots. Individual behavior exerts external economies throughout the range of action illustrated.

There are direct resource costs involved in securing the services. In the example, immunization shots are available at $10 each. In the lower left-hand corner of each matrix square in Figure 2, the total costs of purchasing shots for each individual are shown, again with those for B placed in parentheses. The expected costs of catching the disease plus the costs of getting the shots must be added to provide a single negative payoff for each individual. This addition is indicated in the lower right-hand corner of each matrix square. This total payoff will provide the basis for individual decision under independent adjustment.

Examination of Figure 2 suggests that there exists neither row nor column dominance. Neither A nor B will choose his own level of immunization independently of the action taken by the other. If A gets anything less than three shots, B will purchase two for himself. However, if A gets three shots, B will get only one, and, if A purchases four shots, B will go without immunization of his own. The indicated behavior of A is similar. If B gets no shots, A will get three. If B gets one shot, A will get two. If B gets either three or four shots, A will not seek any immunization on his own account. The matrix is specifically constructed so that a position of equilibrium is attained only when each of the two persons gets two shots; this is shown as the (a_2b_2) box, labeled as *M*. This is the only position where the independently chosen plans of the two persons are simultaneously fulfilled. Should the private behavior of the two result in any other position being attained, at least one of the two persons would have some motivation for independently changing or modifying his behavior. This is, therefore, the equilibrium position under wholly independent adjustment processes.[8]

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[7] These expected costs may be converted into objectively determinate values, at least conceptually, if we think in terms of the costs of purchasing insurance policies guaranteeing complete payments for all damages that might be suffered. This point, as well as several others in the development of this example, we owe to Gordon Tullock.

It will be useful to examine this equilibrium position more carefully. Note that, at equilibrium, the behavior of each party continues to impose marginal external benefits on the other. Also, in one of the two cases, this marginal external economy is Pareto-relevant. This is shown by comparing (a_2b_3) with the equilibrium, (a_2b_2). If negotiations between the two parties could be introduced, A presumably could compensate B sufficiently so that B would take an additional immunization shot. The private advantages to B from getting the additional shot are not sufficient to cover the marginal cost of $10, but the combined advantages to both A and B, from B's getting the additional shot, are greater than $10. This is proved by the fact that the figure in the upper right-hand corner of the squares is lower in (a_2b_3) than it is in (a_2b_2). This is the group or collective payoff. Note that, although A's action, at the position of equilibrium, continues to impose marginal external economies on B, there is associated with A's action no Pareto-relevant external economy present. That is to say, the shift from (a_2b_2) to (a_3b_2) cannot be made without damaging at least one of the two parties.

At independent equilibrium, four units of input are being utilized in providing immunization. Total outlay on shots is $40. Now assume that the two persons merge into a collective group for the provision of immunization. If we then assume that the "government" acts perfectly or ideally, the new solution will be located where the total group payoff is minimized. This is shown in the square (a_0b_3), labeled as *E*. Only in this position, Pareto optimality is attained with B being given three shots and A none, with both persons sharing the total costs. Total outlay on inputs is now $30, less than the $40 spent under private or market equilibrium. Fewer resources are being devoted to immunization than under the independent adjustment alternative. The position of cooperative or collective equilibrium could, of course, be attained also by the operation of interpersonal markets. That gains from trade could be realized over the change from *M* to *E* is indicated by the fact that total group costs are lower in the latter position than in the former.

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[8] The matrix of Figure 2 is, of course, deliberately constructed so as to include a unique equilibrium position. In the general case, either in the two-person or a larger game, there need not be an equilibrium in pure strategies, and there may also be multiple positions of equilibria. Such questions as these need not, however, concern us here since our purpose is one of constructing an example that will embody the standard relationships while refuting the orthodox implications concerning resource use.

In the numerical example of Figure 2, Pareto optimality is attained with smaller total outlay on the service, but, also, with smaller total "consumption" of final output, in this case, immunization, than under independent adjustment. This is shown by the fact that, in E, expected costs of catching the disease are \$30 for A and \$16 for B, whereas these are reduced to \$23 and \$15 in M. This suggests that, under independent adjustment, the consumption of final output is overextended.

This conclusion need not, of course, follow, and it is easy to modify the example to produce results similar to those shown for the nonreciprocal case. It is possible that the ideal cooperative result may involve smaller total outlay while at the same time providing a larger quantity of final services consumed. This possibility may be illustrated in Figure 3, which simply substitutes partially new numbers for the upper-right three-by-three corner of the matrix in Figure 2. Remaining parts of the initial matrix remain unchanged and are not reproduced in Figure 3.

Note that, as before, private equilibrium is attained only in the position *M*, while the position of over-all cost minimization is reached in *E*. However, with the revised numerical payoffs in Figure 3, the quantity of final output is

B A	b_2	b_3	b_4
a_0	45 $\underline{88}$ (23) 0 45 (20) (43)	21 $\underline{65}$ (14) E 0 21 (30) (44)	20 $\underline{68}$ (8) 0 20 (40) (48)
a_1	35 $\underline{85}$ (20) 10 45 (20) (40)	20 $\underline{73}$ (13) 10 30 (30) (43)	19 $\underline{76}$ (7) 10 29 (40) (47)
a_2	23 $\underline{78}$ (15) M 20 43 (20) (35)	19 $\underline{77}$ (8) 20 39 (30) (38)	18 $\underline{84}$ (6) 20 38 (40) (46)

FIGURE 3

actually increased in the "efficient" solution, despite the fact that, as before, the total outlay is below that attained in the private solution. Note that the expected costs of catching the disease are reduced from $23 and $15 for A and B, respectively, in *M*, to $21 and $14 in cooperative or collective equilibrium, even though a smaller total outlay is made.

It should be repeated that the matrix illustration introduced is not designed to show that the "efficient" solution necessarily, or even normally, involves a smaller resource outlay and/or a smaller or a larger consumption of final services than that which independent or private adjustments would produce. The purpose of this analysis, as well as that of the earlier geometrical construction, is primarily negative. It is aimed at showing that the implications concerning aggregate resource use that are normally drawn from the orthodox analysis of collective or quasi-collective goods are not universally correct.

The common-sense explanation of the point made in both the nonreciprocal and in the reciprocal case is straightforward. The means of producing final output are altered when a shift is made from wholly independent adjustment to cooperative or collective arrangements. In so far as the utilization of inputs is characterized by relevant external economies which stand in a substitute relationship, the individual can, under cooperative or collective arrangements, in one sense, "purchase" the consumption of others. By contrast, under wholly private adjustments he can only modify his behavior in response to the behavior of others. And whereas here, too, he can utilize the consumption of others as a partial substitute for his own, he cannot, through his own independent efforts, modify the quantity of others' consumption.

If negotiations among all parties to the externality relationship are allowed to take place—that is, if side-payments in interpersonal markets are introduced—the "optimal" solution will tend to be reached through the emergence of private agreements. Such negotiations will surely take place to some extent in any case, and especially when the interacting group is reasonably small. However, when the interactions extend over a large number of persons, the costs of attaining voluntary agreements may become prohibitive, and any approach to the "optimal" solution in this fashion may be precluded. It is in reference to such situations that collectivization arguments are applied. A complete analysis would, however, take into account the costs of reaching collective or political decisions, and, when this set of problems is included, there is no a priori way of determining whether or not the "optimal" solution may be approached through this means.

III. *Conclusions*

In presenting both the geometrical and the numerical illustrations above, we have referred to the example of health services, especially those that provide immunization from communicable disease. There are several other practical examples that come to mind and reinforce the relevance of the analysis. If a municipal government should cease, forthwith, to provide any collective police protection, individuals would, surely, respond by hiring private policemen, guards, night watchmen. It seems quite likely that, in such a situation, total resource outlay on providing protection to life and property would be greater than under collectivization. The analysis here suggests that this might be true, even in the absence of scale factors. In other words, even if private policemen should be empowered to arrest thieves on property other than that of their employers, and even if police protection is not provided at increasing returns to scale, collectivization might well reduce total outlay on policemen.

The same broad conclusions apply elsewhere. If there were no municipal fire departments, it is likely that total outlay for fire protection would exceed the costs of maintaining the fire departments. Such general conclusions as these will not be questioned. The difficulty in reconciling them, at least initially, with the standard implications drawn from the Pigovian policy analysis seems to be based on the failure of this analysis to have considered adequately the nature of the externality relationship.[9]

Our interest in the particular comparison was stimulated by an attempt to "explain" the comparative record of Great Britain and the United States in the provision of medical care services in the last fifteen years. Over this period, medical care has been collectivized in Great Britain, while it has remained largely organized under market processes in the United States. Naive extensions of the orthodox economic policy analysis would suggest that, under collectivization, if decisions are made at all "rationally," total resource outlay on the provision of health services should have been substantially increased over that which might have been generated under market organization. An examination of the comparative performance of the United States and Great Britain over this period does not support this crude hypothesis. Even when adjustments are made for the substantially higher levels of provision in the United States over the whole period, for the relative growth in GNP in the two countries, and for relative price changes, there remains the fact that total outlay on medical services has increased more rapidly in this country, and that the gap has been expanded during the period of the National Health Service.[10] There seems to have been no demonstrable effect of nationalization, per se, on the trend of aggregate medical expenditures in Great Britain in comparison with the United States.[11]

There are, of course, other hypotheses that might be developed to "explain" this same experience. We make no particular claims that the consumption of medical services is even approximately described by relationships analogous to those presented in the illustrative analyses of Sections I and II. That some aspects of medical services are of this sort is suggested by the "realism" of the communicable disease examples employed. To some extent, at least, it is surely true that, to any particular individual, the utilization of medical services by others reduces his own private requirements for similar services. Nevertheless, other and more sophisticated hypotheses involving institutional differences between the two countries may prove more satisfactory in "explaining" the comparative record. Minimally, we can say that the facts do not contradict the illustrative analysis presented in this note. The same cannot be said for orthodox Pigovian analysis of external effects.

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[9] The theory here is being corrected, in the light of recent critical contributions. In addition to the papers previously cited, the basic article by R. H. Coase [2] should be mentioned.

[10] For an examination of the comparative record, see John and Sylvia Jewkes [6].

[11] "The effect is to channel additional money and resources into the health services." This early statement concerning the National Health Service made by Seymour Harris in 1951 has not been supported by the facts. See [5].

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The Relation of Economic Status to Fertility

Whether a family's economic position and activities influence the number of children it has is a question of long-standing interest in sociology and more recently in economics. A common expectation has been that there ought to be a positive correlation between economic status and family size[1] when widespread dissemination of contraceptive methods made effective family planning feasible for most couples. While economists have recognized that variations in family size and population growth are important factors affecting various economic phenomena, they have given scant attention to the role of economic factors as determinants of family size. This paper explores the relationship of several economic variables to fertility[2] with a unique set of data for the national population.[3]

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[1] For example, see [5, pp. 114-20] and [13, pp. 194-96].

Many studies in Western Europe have reported positive rather than negative correlations between status measures and fertility for particular periods and places, or for special parts of the status distribution [2] [7, pp. 1-25] [11, pp. 115-27] [6, pp. 107-13] [8, pp. 36-71].

In the United States before the war, most studies showed a negative relation between fertility and status measures, but some found that the highest status group had higher fertility than the next lower group. In the postwar period, the increases in fertility have been positively correlated with status measures so that most differentials have been greatly reduced, but it is still too early to tell what the final effect will be on completed fertility. The American materials on differential fertility are reviewed in [4] and [9, pp. 77-112].

In the Indianapolis Fertility Study a small positive correlation was found between fertility and various status measures for fecund couples who effectively planned fertility [10, pp. 188-244].

Historically, there has been an inverse rather than a positive relationship between economic status and fertility in the West during the period of urbanization and industrialization [14]. Large families were more prevalent among the poor and uneducated. Recently these differentials have narrowed or even been reversed in some places. This change was predicted by a number of authors for two reasons: (1) as our population has become more universally educated and urbanized, knowledge of contraception is no longer limited to the higher socioeconomic group so that an effective choice of desired number of children is possible for all strata; and (2) as our economy has lost its rural character, children have lost their economic utility. Instead, the long period of education required by a modern economy makes them liabilities rather than assets from a strictly economic point of view. Under these circumstances, children are analogous to other consumption goods in that their value resides in the direct satisfactions they provide. Some have theorized that as contraceptive use became widespread, the traditional relationship would be reversed so that there would be a positive relationship between income and fertility. Those couples having higher incomes would be able to afford more children as they consume more of other durable goods.

The problem is generally considered as involving simply the relationship between family income and fertility—more income means greater ability to support children.But this ignores some vital distinctions. First, it is desirable to consider separately the income earned by the husband and that earned by the wife. The wife's participation in the labor force may increase the family's income and thus its ability to support more children, but it is also likely to reduce her available time to bear and rear additional children.

It is also important to consider not only the husband's actual income but how his income compares with the average attained by men of his age, occupation, and educational status. Is he earning what is expected for a man in the socioeconomic group to which he is compared by himself and others? We shall use the term "relative income" to refer to the ratio between a man's actual income and the income customary in his socioeconomic reference group.

Why should relative income affect fertility independently of actual income? The reason is that the costs of rearing children are related to the socioeconomic reference group of the parents. Given the social status of a family, it has only limited control over the costs of its children [1]. Certain expenditures for each child, for example food and housing, are set by the scale of living of the parents, and this in turn depends on the family's social status. Many of the variable costs of child rearing also will be determined by the social standards of the family. Furthermore, while a consumer can choose to own both a cheap and an expensive car, this choice is not equally possible in determining the costs of his children. In fact, if the expenditures for a child do not conform to the socioeconomic standards of his family, he may be a source of disutility. Therefore, a couple whose income is low relative to the group with which it is identified may restrict its family growth so the desired standard can be furnished to a smaller number of children.

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[2] Fertility is measured by the number of live births to date of interview.

[3] The sampling design and main results of this study appear in [3].

It is also questionable whether the relationship between family income and fertility is in fact a simple positive relationship, with higher income enabling a family to support and thus enjoy more children. The costs of rearing children are not all monetary; their care involves attention and time on the part of the parents. Moreover, higher social status may involve the parents in interests and activities which are incompatible with a large family. Since there is consensus among most U.S. families on a rather narrow range of desired family size, it suggests that there is a certain minimum number of children considered essential (irrespective of size of income), but that additional children do not necessarily continue to add equal utility; after four children, an additional child may be a source of disutility.

In the present paper we have analyzed how fertility varies in relation to the husband's actual and relative income and also in relation to the wife's income and labor participation.

I. *The Data and Methods of Analysis*

All our data are from interviews with a national probability sample of white married women in the childbearing years (18-39) interviewed in 1955 [3]. We have restricted our analysis mainly to nonfarm couples who are "fecund planners" and have been married at least five years. By considering only nonfarm couples, we get a more homogeneous group and one that is increasingly more typical of the U.S. population. The "fecund planners" are those couples who have no known fecundity impairments, who have used or intend to use some method of family limitation, and who have never had a pregnancy they did not want. These couples have a potential need for family planning since they are fecund; they are practicing or are favorably disposed toward family planning; and they have planned effectively enough to date so all their pregnancies were wanted. By so restricting our sample we have limited it to those who are most able to choose the number of children they want. If economic considerations are important, they should be most evident in such a sample.

We deal with two groups of economic variables—one related to the husband's economic position and the other to the wife's participation in the labor force. The husband's economic position is measured both by his income in the year preceding the interview and by his relative income—that is, the ratio of his actual income to what it might be expected to be on the basis of his occupation, education, and age. The husband's annual income was obtained in response to a question asked of his wife in January, 1956 about his income from all sources during the preceding year. The husband's relative income was calculated as the ratio between his actual income in 1955 and the income expected for him on the basis of a regression on 1955 income of his occupation,[4] education, age, and residence in the South or elsewhere. Thus, the relative income score measures roughly whether the husband's recent income is above or below that of the group with which he probably is compared by himself and others.

The wife's work variables include: the number of years she has worked since marriage, her income during the preceding year, her current labor force status, and her expectations about future work.

We have used a method of multivariate analysis in which each of the subclasses of the independent variable is a predictor related to the dependent variable. To do this, each subclass of a predicting variable is converted into a "dummy" variable, taking the value of either zero or one, depending on whether or not the case falls in that particular subclass. The advantage of this technique is that no assumption is made about either the linearity or the ordering of the subcategories of the independent variables. Also, it is possible to see how the dependent variable is affected by each subclass of the independent variable.

In this type of analysis the following three statistical measures are derived:

1. The mean value of the dependent variable for each subclass of each independent variable expressed as a deviation from the grand mean. Also, an adjusted deviation is obtained for each subclass of the independent variable—adjusted for the effects of all other independent variables being considered.

Thus we can see what the actual variation of the dependent variable is in the different subclasses of the independent variable and the extent to which these unadjusted deviations were biased as a result of intercorrelations with the other explanatory factors being considered. For example, Table 3 (Appendix) shows how much the mean number of live births in each category of wife's income deviates from the grand mean of all families sampled. Wives with incomes of $2,000 or more had, on the average, 1.06 fewer children than the sample as a whole. But this difference is partly due to the correlation of wife's income with the other five economic variables listed. After adjusting for the influence of these other factors, this income class is found to have a net deviation of only .30 live births below the grand mean.

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[4] To scale occupations for this regression, we used a socioeconomic status index which assigns to each occupation a prestige score. This socioeconomic status index was developed by Otis Dudley Duncan and reported in [12, pp. 109-38]. This index assigns to detailed occupations a score that is based on the well-known NORC prestige score. Since the NORC prestige score is available for only a limited number of occupations, Duncan assigned scores to other occupations on the basis of the regression on NORC prestige scores of education and income for occupational classes. The correlation for this regression was extremely high.

The meaning of the adjusted deviations may be clearer if they are regarded as coefficients in a regression equation in which each category of each predicting variable is a separate independent predictor taking on the value of one if present and zero if absent. For any individual case we predict the number of births by adding the coefficients for the subcategories which characterize the individual.

2. A beta coefficient computed as a summary measure of the effect of each independent variable on the dependent variable. The relative size of the beta coefficients is used to indicate the rankings of the partial R^2's for the various independent variables.

3. A measure of the total variation explained by all the independent variables. This measure is the conventional R^2 adjusted for the loss of degrees of freedom in using each of the many subclasses as independent variables.

This multivariate analysis was carried out for two groups of nonfarm fecund planners—those married 5-9 years, and those married 10 or more years. For each marriage-duration group we relate our six economic variables to live births (step 1). We also ran an additional multivariate analysis with an augmented group of independent variables. In this step (step 2) we add eight demographic variables to our six economic variables in order to see whether the relationship for the economic variables is much affected by demographic variables usually correlated with fertility.

II. *The Findings*

We begin with the group married 10 or more years, since here fertility is close to its final level. After 10 years of marriage, husband's relative income (beta = .24) is more strongly associated with fertility than any of the other economic variables. (See Tables 2 and 4, Appendix.) The relationship is positive, as expected, and it persists and is even strengthened when the effect of other economic variables is taken into account. The group with very low relative incomes has .45 children less than the average, while the groups with high and very high relative incomes have respectively .18 and .06 children more than the average. We interpret this to mean that lower incomes lead to lower fertility *if* the diminution in income is beyond what is usual for the husband's status. Adjusting for the effect of the other economic variables, and particularly adjusting for actual income,[5] only strengthens this relationship; the lowest relative income group has .50 children less than the average while the highest has .28 children above the average.

The husband's actual income is also significantly related to fertility (beta = .21), but the relationship is negative rather than positive after allowance is made for the other variables. In the unadjusted series there is substantially no relationship except that the lowest income group has less than average fertility. However, when we control for relative income, the adjusted deviations show a clear negative relationship between actual income and fertility. The lowest income group has .19 more children than average while the highest income group has .33 less children than average. We interpret this to mean that lower income, in and of itself, leads not to small families, but to larger than average families. The low fertility of the lowest income families in the unadjusted series must be due to the weighting of this group with persons earning less than they should, since once we control for relative income, the adjusted deviations show a negative correlation of income and fertility. Likewise, higher income—if appropriate to the occupational niche—does not lead to larger families; instead it is associated with smaller families. Only if there is extra money above and beyond these normal status demands will fertility be increased.

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[5] In this analysis we are controlling for all the five economic variables, but in a previous analysis, where we restricted the variables to husband's relative and actual income, we derived almost identical results.

The years the wife has worked since marriage (beta = .19) are also associated with fertility for the group married 10 years and over but to a lesser degree than the measures of husband's income. The relationship is negative—a long work history is associated with fewer children. There is a difference in fertility of .47 children (.36 for adjusted deviations) between the group working 5 years or more and that with no work history. Wife's income is also negatively associated with fertility (beta = .13). The group with incomes over $2,000 has .61 children less than the average (adjusted deviations show .51 less children), while the group with no income has slightly higher than average fertility. Wife's labor force status and current work experience show no consistent relation to fertility.

The total variance explained by the six economic variables for the group married 10 years or more is 4 per cent. (See Table 1, Appendix.) Even though a number of these economic variables are consistently related to fertility, the association is not strong enough to explain much of the total variation. While the amount of variance may seem small, these results should be evaluated not only against the small correlations found generally in cross-section data but also in terms of the very complex series of events and influences which determine the number of children born to a family after 10 or more years of marriage. No single limited set of measures will capture more than a small part of this total reality.[6]

[6] For this particular problem there are some considerations which lend more significance to the observed relationship. (1) The study from which our data are drawn shows that there is a marked consensus among U.S. families on a very limited range in both desired and expected family size. Eighty-five per cent of the fecund planners gave 2-4 children as their personal ideal size of family, and 79 per cent expected their final family size to be in this range. Moreover, if the norm is that 2, 3, or 4 children are almost equally acceptable, an accurate scaling of perceived social differences in family size would yield an even smaller variance. Since there is such a marked consensus on a limited range of desired family size, there is less variance in family size to be explained and much less likelihood that any small group of factors will be able to account for it. In addition, the fact that family size is a discrete variable makes it unlikely that we can predict it exactly. (2) By excluding fecundity and fertility planning as variables, we have eliminated two of the primary determinants of variability in family size. With a comparable multivariate analysis for all nonfarm couples, including the subfecund and poor planners, we can explain a considerably higher proportion of the variation than we can for our fecund planners. The coefficient of variability is considerably higher for the total nonfarm group than for our nonfarm fecund planners in both marital-duration groups, reflecting the greater variability in family size in the total group.

For the group married 5-9 years, the findings are quite different. The betas in Table 2 (Appendix) and the unadjusted and adjusted deviations in Table 3 (Appendix) show that: (1) The work variables of the wife are consistently related to fertility for this intermediate marriage-duration group, with number of years wife has worked since marriage (beta = .33) having a much stronger relationship to fertility than any of the other economic variables. Wife's income (beta = .12) and wife's labor force status (beta = .10) are also associated with fertility, though the relationship is much weaker. (2) Neither of the husband's income measures has a significant or consistent relation to fertility. The largest variation from the grand mean in any of the subclasses of husband's actual or relative income is .11 live births.

The unadjusted deviations in Table 3 (Appendix) show a consistently negative effect on fertility of additional years of work by the wife. There is a difference of 1.37 children between the group working five or more years since marriage and those not working at all. But the main part of this effect is accounted for by the group of wives who have worked five or more years. They have 1.10 children less than the average. When we adjust for the effect of the five other economic variables, the period of work since marriage still has a considerable effect on fertility; the difference in fertility between wives who have not worked since marriage and those working five or more years is 1.10 children with the subclass working five or more years still accounting for most of this difference. It is interesting that those wives who have worked less than one year have higher fertility than wives who have not worked at all since marriage. This is because the wives working less than one year include women who started to work after marriage and quit because they became pregnant.

Wife's income initially shows a fairly strong relationship to fertility; there is a difference of 1.20 children between the wives with no income and those with an income of $2,000 or more. However, when we adjust for the effect of the five other economic variables, the difference in fertility between these two income categories is reduced to .32 children. It is not surprising that the number of years worked has more independent effect than wife's income since the former measure is relevant to the whole of married life while income was measured only for the year preceding the interview.

Though the years the wife has worked have the same negative relationship to fertility for the group married 5-9 years as they did for those married 10 years or more, the relationship is much stronger and of more paramount importance for the younger group. (The beta coefficient for years wife has worked is .33 for this group as against .19 for the older group.) The difference in fertility between the two extreme categories of wife's work history is 1.37 children for the 5-9 group and only .47 children for the group married 10 years or more. A plausible explanation is that the variables measuring wife's work affect the timing of children more than they affect the number finally attained. In a society where the average couple has only three children, wives who work for five years or more can still come close to the average number of children over the entire childbearing span. However, their fertility at the end of the first decade of married life will be considerably below average if they have worked five or more years during this period.[7]

The total variance in fertility explained by the economic variables for the group married 5-9 years is 18 per cent—compared to 4 per cent found for the 10-plus group. The greater proportion of variance explained for the 5-9 group is attributable to the strong effect of years wife has worked in this intermediate marriage-duration group.

In the first step of the regression analysis we dealt only with economic variables. In a second step we added to the economic variables eight other social and demographic variables usually related to fertility. The eight variables added were: years married, wife's attitude toward family limitation, wife's age, wife's education, husband's occupational status score, wife's religion, husband's education, and couple's farm background. While these variables are interesting in themselves, we resist the temptation to discuss them apart from their effect on the economic variables. The main question we want to answer is whether adjusting for the effect of these eight variables changes the previously observed relationship of the economic variables to fertility. The answer is no.

If we take the three economic variables which show the greatest relation to fertility—husband's income, relative income, and years wife has worked since marriage—we find that the relevant beta coefficients change very little and the deviations in fertility between the subclasses are only slightly altered when we adjust for the eight demographic variables. For an illustration, see Table 6 (Appendix) where we compare the adjusted deviations obtained in the first and second steps for these three economic variables. The economic variables which were found to be most important in the first step are still relatively important in the larger matrix of variables. (See Table 5, Appendix.) In the 10-years-and-over married group the summary betas for years wife worked and husband's relative income rank equally with wife's religion as most important, with husband's income following next. In the 5-9-year group the number of years wife has worked is clearly first among the 14 variables considered.

Finally, the additional variance explained by adding to the economic variables eight presumably important social and demographic variables is not as great as might have been expected. (See Table 1, Appendix.) The R^2 rises from .18 to .24 for the group married 5-9 years, and from .04 to .10 for the group married longer. Considering the large group of variables, the substantial difference in the total variation explained between these two marriage-duration groups is an important fact in itself. It is consistent with a hypothesis that family size tends to converge towards a commonly held norm over the childbearing period with the important differentials being now not in the final size of family but in the process of planning and spacing by which a relatively homogeneous result is attained. This suggests that differential child-spacing may replace differential fertility as a central interest in fertility research.

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[7] We have some independent corroboration of this explanation in an analysis of mean birth intervals for this same sample by Mr. K. Namboodiri. For wives married 10 years or more, he finds a much stronger relationship between their work history and the interval between births than we have found for fertility. Wives with many years of work have their children later and tend to catch up, although, on the average, they never actually make it.

III. *Summary*

What then can we say in summary about the effect of the economic variables on fertility in an urban population of fecund planners?

1. There is evidence that husband's income does make a difference over the longer childbearing period if it is considered in relation to the average income for the husband's occupational status and age. An income which is above the average for one's status is associated with more children, but being in a higher absolute income class means fewer children if the higher income is only what is usual for the husband's age and occupational status.

2. If the wife has an extensive work history, it will be associated with having fewer children but this relationship is more important in the earlier than the later stages of childbearing. It affects child-spacing more than total family size.

3. While economic variables considered together explain only a small part of the total variation in family size, this must be evaluated in the light of the small proportion of the variance explained even when a larger important set of social and demographic variables is considered.

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* The author is a teaching fellow in economics at the University of Michigan. She wishes to acknowledge advice from Dr. Daniel B. Suits, Director of the Research Seminar in Quantitative Economics, and Dr. Ronald Freedman, Director of the Population Studies Center, University of Michigan. The Population Studies Center provided financial and clerical assistance for the computations.

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APPENDIX

TABLE 1— PERCENTAGE OF VARIANCE[a] EXPLAINED BY MULTIVARIATE ANALYSES FOR NONFARM FECUND PLANNERS

	Duration of marriage	
	5–9 years	10+ years
Step 1		
6 economic variables	.18[b]	.04[c]
Step 2		
6 economic and 8 demographic variables	.24[b]	.10[d]
Live births:		
Mean number	2.17	2.61
Standard deviation	.99	1.29
Number of cases	433	374

[a] R^2 adjusted for degrees of freedom.

[b] Significant at .001 level.

[c] Significant at .01 level.

[d] Significant at .05 level.

TABLE 2—BETA COEFFICIENTS FOR ECONOMIC VARIABLES USED IN STEP 1

Economic variables	Duration of marriage	
	5–9 years	10+ years
Husband's income	.06	.21
Relative income	.08	.24
Years wife worked since marriage	.33	.19
Wife's income	.12	.13
Wife's future work expectation	.01	.05
Wife's labor force status	.10	.04

TABLE 3—UNADJUSTED AND ADJUSTED DEVIATIONS IN MEAN NUMBER OF LIVE BIRTHS FOR NONFARM FECUND PLANNERS MARRIED 5–9 YEARS, FOR ECONOMIC VARIABLES USED IN STEP 1

Economic variables	Number of cases	Deviations from the grand mean[a]	Deviations from the grand mean adjusted for the effect of other independent variables
Years wife worked since marriage			
None	113	.27	.23
Under 1	86	.34	.26
1–4	190	−.06	−.05
5 or more	44	−1.10	−.87
Wife's income			
None	319	.14	.02
$1–$999	52	.15	.18
$1,000–$1,999	28	−.64	−.23
$2,000 or more	34	−1.06	−.30
Wife's current labor force status			
Full-time	48	−.82	−.28
Part-time	37	−.04	.10
None	348	.12	.03
Wife's future work expectations			
Yes	199	−.08	.01
No	234	.07	−.01
Husband's income			
Under $3,000	58	−.05	−.05
$3,000–$3,999	118	.01	.07
$4,000–$4,999	109	.00	−.08
$5,000–$5,999	86	−.02	.05
$6,000 or more	62	.05	−.12
Relative income			
Very low	31	−.08	−.02
Low	123	−.04	−.02
Average	144	−.03	−.03
High	77	.11	.16
Very high	58	−.05	−.07

[a] The grand mean number of live births for the group married 5–9 years is 2.17.

TABLE 4—UNADJUSTED AND ADJUSTED DEVIATIONS IN MEAN NUMBER OF LIVE BIRTHS FOR NONFARM FECUND PLANNERS MARRIED 10 OR MORE YEARS, FOR ECONOMIC VARIABLES USED IN STEP 1

Economic variables	Number of cases	Deviations from the grand mean[a]	Deviations from the grand mean adjusted for the effect of other independent variables
Relative income			
Very low	31	−.45	−.50
Low	121	−.16	−.32
Average	79	.17	.07
High	88	.18	.37
Very high	55	.06	.28
Husband's income			
Under $3,000	42	−.33	.19
$3,000–$3,999	56	.07	.31
$4,000–$4,999	98	.07	.26
$5,000–$5,999	60	.04	−.19
$6,000 or more	118	.01	−.33
Years wife worked since marriage			
None	106	.04	−.01
Under 1	37	.50	.49
1–4	139	.12	.12
5 or more	92	−.43	−.37
Wife's income			
None	282	.09	.07
$1–$999	34	.06	.00
$1,000–$1,999	20	−.16	−.01
$2,000 or more	38	−.61	−.51
Wife's current labor force status			
Full-time	53	−.42	.11
Part-time	28	−.11	.10
None	293	.09	−.03
Wife's future work expectations			
Yes	173	−.09	−.07
No	201	.07	.06

[a] The grand mean number of live births for the group married 10 or more years is 2.61.

TABLE 5—BETA COEFFICIENTS FOR ECONOMIC AND DEMOGRAPHIC VARIABLES USED IN STEP 2

Economic and Demographic Variables	Duration of Marriage	
	5–9 years	10+ years
Years wife worked since marriage	.30	.21
Husband's income	.06	.18
Relative income	.09	.21
Wife's religion	.11	.21
Years married	.20	.16
Wife's attitude towards family limitation	.15	.17
Wife's age	.14	.06
Husband's occupational status score	.11	.14
Wife's income	.09	.13
Wife's education	.12	.04
Wife's labor force status	.10	.09
Husband's education	.08	.02
Couple's farm background	.06	.07
Wife's future work expectations	.01	.03

TABLE 6—COMPARISON OF DEVIATIONS FROM GRAND MEAN FOR SEVERAL IMPORTANT ECONOMIC VARIABLES IN STEPS 1 AND 2 (WITH AND WITHOUT ADJUSTMENT FOR 8 ADDITIONAL SOCIAL AND DEMOGRAPHIC VARIABLES), FOR COUPLES MARRIED 10 YEARS OR MORE

Economic variables	Deviations from the grand mean[a] in Step 1 (adjusting for 5 other economic variables)	Deviations from the grand mean in Step 2 (adjusting for 5 other economic variables and 8 other social and demographic variables)
Relative income		
Very low	−.50	−.42
Low	−.32	−.28
Average	.07	.10
High	.37	.32
Very high	.28	.21
Years wife worked since marriage		
None	−.01	.07
Under 1	.49	.49
1–4	.12	.09
5 or more	−.37	−.42
Husband's income		
Under $3,000	.19	.17
$3,000–$3,999	.31	.29
$4,000–$4,999	.26	.19
$5,000–$5,999	−.19	−.21
$6,000 or more	−.33	−.25

[a] The grand mean number of live births for couples married 10 years or more is 2.61.

Liquidity and the Multiple Key-Currency Proposal

It appears that a tentative decision has been reached to adopt the multiple key-currency proposal as a solution to the anticipated future shortage of international liquidity [6, p. 12]. The proposition that "liquidity" may be increased by multiplying holdings of foreign exchange necessarily rests upon the assumption that the reserve centers are able to act asymmetrically with regard to their assets and liabilities.[1] This has been true of the United States and the United Kingdom in the past, but it does not necessarily follow that it would continue to hold in an extended system. In this paper we shall attempt to outline the contribution which the proposal might make to increasing liquidity, and to indicate some of the conditions which its successful implementation would require.

I. *The Concept of International Liquidity*

The purpose of holding reserves is to permit a country the convenience of running a balance of payments deficit at some future date. The ability to incur a deficit is valued by a country's decision-makers because of the freedom of action which it gives them to avoid or mitigate certain "undesirable" policy measures. Many deficits, presumably, are either self-reversing, reversed by what one may consider exogenous shocks, or reversible at less cost if policy measures to this end may be spread over a longer period of time. We may classify the "undesirable" actions which might be used to reverse a deficit in the absence of adequate liquidity as follows:

1. Inefficient policies.
   (a) Measures that disrupt an internal stabilization program.
   (b) Measures that disturb the allocation of resources, e.g., exchange restrictions or other trade barriers.
2. Policies with arbitrary effects on income distribution.
   (a) Manipulation of unilateral transfers, such as foreign aid, to reflect payments deficits rather than wealth and income.
   (b) Manipulation of long-term capital transfers in a similar manner.
3. Alteration of the exchange rate.

If "international liquidity" is to be an analytically useful concept, it must be defined to reflect the rarity with which the above "undesirable" policies need be applied. Of course, a proper measure would require taking into consideration the severity as well as the frequency of the necessary corrective actions. Liquidity would thus have to be measured by a vector instead of a scalar, each component of which would reflect the frequency with which actions with a given disutility to society's decision-makers would be called for. This complication will be largely ignored in what follows, since it will be assumed that liquidity may be adequately measured by a scalar that reflects the frequency with which corrective measures of a given intensity are required. The simplification is permissible insofar as the frequency and severity of such measures are positively correlated.

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[1] A country will be said to "act asymmetrically" with regard to its assets and liabilities if it acts as though its reserve position is more secure when its short-term assets and its short-term liabilities are increased by equal amounts.

We are now in a position to suggest a conceptually appropriate scalar measure of a country's liquidity at time t. It will be defined as the size of the total deficit (excluding short-term capital outflows), subsequent to time t, that the country could incur without being forced to take "undesirable" corrective actions.[2] The larger this sum, the greater, *ceteris paribus*, is the probability that a deficit subsequent to t would be reversed without resort to one of the "undesirable" actions, thus giving a correct ordinal reflection of the frequency which is of conceptual significance. It follows that a country's liquidity, L, at time t may be measured by the formula:

$$L = R - R_{min} + F_o + F_p - \triangle R_{min},$$

where:

R = the country's gold and foreign exchange reserves;
R_{min} = a (psychologically determined) minimum level of reserves;
F_o = "official financing," i.e., extra lending by the I.M.F. or foreign monetary authorities without the imposition of "undesirable" conditions;
F_p = "private financing," i.e., extra private lending which might be attracted by a rise in interest rates that did not interfere with domestic stabilization objectives;
$\triangle R_{min} = f(F_o, F_p)$ = the increase in R_{min} that would be induced by fully utilizing F-liquidity.

R is, of course, the conventional measure of a country's liquidity; it is admitted to be a crude measure and no time need be wasted in attacking its use. F_o may be expected to depend principally on the confidence which foreign authorities have that the country's liabilities will not lose value in terms of alternative means of holding reserves. The possibilities of financing a substantial deficit by F_p inflows, without a major rise in interest rates, would seem to be limited; indeed, F_p is quite likely to be negative, as it would appear to be in the United States at the present time.

The items of major interest are R_{min} and $\triangle R_{min}$. The inclusion of the former represents the postulate that there is some minimum reserve level at which capital flight would become cumulative and so force the authorities to take restrictive actions. One may expect R_{min} to depend upon the reputation of the country's Finance Minister, the size of its contingent liabilities arising from trade, and many other factors that may be taken as given for a particular country at a particular time. Of special importance, however, is the dependence of R_{min} on the size of the country's short-term liabilities. One may assume that there is a positive relationship—it does not seem plausible, for example, to imagine that there would be serious concern over the current U.S. deficit if her short-term liabilities did not exceed her gold stock. This being so, it follows that the using-up of F_o and F_p liquidity would raise R_{min}, thus restricting the size of the total possible deficit. The inclusion of the term ΔR_{min} allows for this fact.

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[2] Two comments seem in order. (1) Under the definition given, a country's liquidity is zero when it is forced by external circumstances to take actions contrary to the needs of the domestic economy. Examples of this are the British deflations in the summers of 1957 and 1961 and, even more obviously, the Canadian austerity program of June 1962. The fact that the I.M.F. loans to these countries would have permitted a further deficit after imposition of the deflationary measures illustrates the limitations of a scalar measure of liquidity. (2) We shall assume in the remainder of this paper that the liquidity of individual countries may be added to obtain world liquidity.

II. *The Liquidity-Creating Potential of Additional Key Currencies*

It is hoped that the above remarks may contribute to the evolution of a useful measure of international liquidity regardless of the application we are now going to make. Our immediate concern will be to illuminate the contribution one might expect the recognition of additional key currencies to make to increasing liquidity as defined above.

The existing reserve centers (C-countries) have enjoyed considerable liquidity in the past by virtue of the willingness of non-center countries (N-countries) to hold dollars and pounds. It is a willingness to lend to a C-country, by holding reserves in the form of C-currency—whether or not this is a conscious act of lending by the N-country—which forms the basis of the liquidity-creating potential of a key currency. It should be noted that C's liquidity is increased when other countries become *willing* to increase their holdings of its currency. (The actual accumulation of C-currency constitutes only a redistribution of existing liquidity.) Since the actual increase of C's liabilities would raise R_{min}, the net addition to liquidity is $F_o - \Delta R_{min}$. This term is positive if, as is generally assumed, a reserve center is able to act asymmetrically with regard to its assets and liabilities. Liquidity would be increased with an increase in the number of center countries only if the four factors that seem capable of explaining the above asymmetry continue to hold in an extended system.

The first factor explaining the asymmetry is that some part of foreign exchange holdings may be held as transactions balances for the purpose of routine financing of trade. It is clear that a currency which is not used to finance trade will not be held to meet this transactions demand. Hence no countries other than the United States and Britain could base an asymmetrical treatment of their assets and liabilities upon this factor.

Second, an asymmetry is again rational insofar as it is true that N-countries have no alternative or equally attractive form in which to hold their reserves. To a large extent this has been the case in the past. The Sterling Area countries were held by political ties and the advantages of access to the London capital market. Those holding dollars had no alternatives except gold, which does not earn interest, or sterling, which was a comparatively weak currency. The creation of new reserve centers would greatly weaken this basis of the asymmetry, since any one country would have a larger choice of forms in which to hold its reserves. In other words, an N-country would be more likely to convert its holdings of C-currency to some other form of reserves when it felt this to be advantageous, thus raising the level of R_{min} for any given level of C's liabilities.

Third, just as a one-bank banking system need not concern itself with shifts between depositors, so can a single center country safely assume that as some creditors require repayment others will be building up their currency holdings. This is the assumption on which the Sterling Area was operated prior to World War II, and presumably it retains a certain validity even with the two reserve centers of the present day. A multiplication in the number of reserve centers (unless different nations maintained a similar reserve composition) would again tend to eliminate this basis for asymmetrical behavior.

Fourth, a center country may be able to rely upon the willingness of its creditors to avoid "embarrassing" actions. Foreign central banks may give an explicit or implicit pledge that they will manipulate the composition of their reserves in such a manner as to maintain C's reserves at a constant level, except insofar as C is in current deficit. An institutional guarantee of the safety of treating assets and liabilities asymmetrically would be provided by any agreement which resulted in a standardization of the ratio in which the major nations combined gold and different currencies in their reserve portfolios.

It has been suggested above that the first three reasons for considering the asymmetry to be explicable on rational grounds would not apply significantly to new key currencies, while the creation of new centers would weaken the second and third reasons which the existing centers have for acting asymmetrically. Hence it would be possible for the recognition of new key currencies to reduce world liquidity, unless the fourth factor alone were sufficiently dependable to induce markedly asymmetrical behavior.[3] Liquidity would be increased in the extended system only insofar as central bankers can rely upon their foreign counterparts to continue holding a deficit currency, in conditions where the immediate interests of any single country point to the reverse. It is possible that introduction of the proposal would be accompanied by agreements between the world's central bankers that would provide the necessary assurance of willingness to continue holding a deficit currency. In the absence of such agreements it is difficult to see how any significant increase in liquidity would flow from a multiplication in the number of key currencies. The current reluctance of European countries to increase their holdings of a currency with such underlying strength as the dollar, and the British belief that it is dangerous for one key currency to be backed by another, hardly suggest that central bankers have developed the type of instinctive cooperation that would be essential for success of the proposal in the absence of formal commitments.[4]

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[3] An actual reduction in world liquidity might come about by pressure exerted on the United States and United Kingdom to make available funds that N-countries wished to hold in the form of new key currencies, as outlined in the text. It is possible to conceive of an alternative mechanism that could lead to this result. Suppose that the world total of R were greatly increased by mutual currency swaps. Suppose also, as seems improbable but not impossible, that central bankers become unwilling to hold a currency in current deficit sooner than do private speculators. It seems plausible to suppose that speculators acquire a notion of what constitutes a safe level of reserves that is based on recently attained actual levels. If this is so, then foreign central bankers would have the power to compel a deficit country to take actions to terminate its deficit; for, by liquidating the swaps, they would reduce R to a level below the point at which private capital flight would become cumulative. By hypothesis, the point at which central bankers threatened to liquidate the swaps would occur sooner than the point at which private speculators would have come into play had the country simply run down uninflated R-reserves. In other words, it would be possible for mutual currency swaps to raise R_{min} more than they raised R.

III. *The Necessity of Exchange Guarantees*

One of the more striking occurrences of the past decade is the way in which elasticity pessimism has vanished in the face of historical evidence. The British devaluation of 1949, the French devaluation of 1958, and the German revaluation of 1961 have demonstrated conclusively that a change in a country's exchange rate is capable of curing a "fundamental disequilibrium" without imposing severe burdens in terms of inefficiency or arbitrary income redistribution. No other policy variable with this happy property has come to light. Now there is no reason for supposing that "fundamental disequilibria" have become things of the past. If historical experience is any guide, one may expect these to arise most often when governments pursue what central bankers, in their self-appointed role of philosopher-kings, consider to be "unsound" policies. But any plausible view of the determinants of a country's payments balance would suggest that fundamental disequilibria will also arise on occasion if perfect internal stabilization is achieved. It is only if stabilization is subordinated to external balance that one can expect the disappearance of fundamental disequilibria. It follows from this, given certain obvious and rather weak assumptions as to desired goals, that the possibility of periodic changes in exchange rates is a highly desirable feature of any future international monetary system.

The essence of the multiple key-currency proposal is a multiplication in the number of forms in which a country may hold its reserves. It is obvious that countries will wish to hold their reserves in a currency that they are confident will not lose value in terms of other forms of holding reserves. Yet any expansion of liquidity depends upon some countries adding to their reserves the currencies of nations in current deficit (i.e., those currencies most likely to be devalued). If countries are to be induced to do this, it seems evident that the exchange risks of holding a reserve currency would have to be reduced to something approaching zero. There are two methods by which this might be accomplished: a renunciation of the weapon of exchange-rate adjustment, which it was suggested above would be highly undesirable, or the giving of exchange guarantees.

R. V. Roosa has criticized the exchange-guarantee proposal in the context of the present system [6, pp. 5-7], on the grounds that N-countries might still object to accumulating indefinitely large amounts of C-currency and would therefore seek to impose restrictive conditions on the center countries. But this is inevitable under any version of the gold exchange standard, and there can be no reasonable doubt that these pressures will be less acute, and come into play less rapidly, when a guarantee is in effect than otherwise. Moreover, the objections are crucial only if one accepts Roosa's premise that the purpose of a guarantee is to ". . . assure . . . the world that devaluation will not occur" [6, p. 5]. If its purpose is, rather, to permit adjustments in the par values of key currencies without precipitating a crisis, the objections are almost entirely irrelevant.

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[4] The author's suspicion that there would be very little addition to liquidity from adoption of the proposal is dependent upon the simplification of using a scalar rather than a vector measure of liquidity. There would seem more hope that the system might contribute to the ability of a country to run a prolonged deficit while relatively mild corrective measures were put into effect—an ability that would be reflected by an increase in the second (or higher) elements of a vector measure of liquidity.

The creation of additional liquidity, and therefore the international interest, requires that surplus countries accept the currencies of deficit countries; but the national interest of any single surplus country suggests a refusal to hold deficit currencies in the absence of a guarantee.[5] In contrast to an ideal price system, the proposal's success depends upon parties acting in direct conflict with their immediate interests. Although one might hope that the major Western countries would be prepared to accept the inherent risks in reasonably favorable times, one can scarcely escape the conclusion that such a system would prove extremely fragile in the event of a crisis.[6] It has been suggested by F. A. Lutz [4, p. 73] that countries would be more willing to hold their reserves in forms other than gold if they had a choice as to the currency involved. While true, this fact is hardly a merit of the scheme, for if countries decide to exercise such a choice they will obviously tend to move out of the currencies of deficit countries, and thereby, at best, destroy the liquidity-creating potential of the scheme, or, at worst, accentuate rather than offset current deficits and surpluses and so lead to a breakdown of the system.

A guarantee would obviously create new problems and only partially solve some of the existing ones.[7] Nevertheless, there appears no other way of reducing the divergence between national and international (or "private" and "social") interests to proportions that would give some hope of durability to a multi-center system. Unless one believes that countries would accept with equanimity a writing-down of the value of their reserves, or unless one is prepared to forego the possibility of exchange-rate adjustment, any major extension of the gold exchange standard is dependent upon the introduction of guarantees. It is misleading to suggest that the multiple key-currency system is an alternative to a guarantee, as implied by Roosa [6, pp. 5-7 and 9-12].

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[5] This point is essentially one made by Aliber [1, p. 1115].

[6] The conclusion that a multi-center system might prove fragile is not original. See [5, p. 217]. Historical experience also seems to provide support for this conclusion. The domestic banking system of the United States at the beginning of the century was rather similar in structure and was notorious for the severity of its crises prior to the establishment of a central banking system.

[7] The difficulties of implementing an exchange guarantee have been outlined by Beza and Patterson [2]. Although significant, there is no reason to suppose that the difficulties are insuperable. The principal way in which their conclusions (as modified by [3]) conflict with those reached in this paper is their judgment that, because devaluation would be more expensive to a *C*-country in terms of real resources if a guarantee were honored, center countries would be less willing to devalue if a guarantee were in effect than otherwise. The author believes that Beza and Patterson have underestimated the psychic costs of devaluation under the present system. Be that as it may, the case for guarantees is obviously much stronger under a multi-center system, because in this instance the devaluation of one key currency would almost always threaten at least one other reserve currency and so lead central bankers to desire to liquidate non-guaranteed reserves held in that form.

IV. *Conclusion*

The most noteworthy conclusion to be drawn from this analysis is that the successful operation of a multiple key-currency system would require both exchange guarantees and continuing cooperation between central bankers of a type that would effectively limit their choice as to the form in which they hold their reserves. Yet these are two of the conditions whose undesirability has frequently been held to be an obstacle to implementation of the alternative proposal to create a world central bank. The multiple key-currency proposal represents an attempt to avoid the impracticality supposedly associated with a world central bank, but if both proposals in fact depend on the fulfillment of similar conditions, it is difficult to convince oneself that the sacrifice of the additional liquidity that an almost closed system would permit is worth while. Unless, of course, the object of the exercise is to reinforce discipline rather than to expand liquidity.

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Corporate Income Taxes and the Cost of Capital: A Correction

The purpose of this communication is to correct an error in our paper "The Cost of Capital, Corporation Finance and the Theory of Investment" (this *Review*, June 1958). In our discussion of the effects of the present method of taxing corporations on the valuation of firms, we said (p. 272):

> The deduction of interest in computing taxable corporate profits will prevent the arbitrage process from making the value of all firms in a given class proportional to the expected returns generated by their

> physical assets. Instead, it can be shown (by the same type of proof used for the original version of Proposition I) that ***the market values of firms in each class must be proportional in equilibrium to their expected returns net of taxes*** (*that is, to the sum of the interest paid and expected net stockholder income*). (Italics added.)

The statement in italics, unfortunately, is wrong. For even though one firm may have an *expected* return after taxes (our $\bar{X}^\tau$) twice that of another firm in the same risk-equivalent class, it will not be the case that the *actual* return after taxes (our X^τ) of the first firm will always be twice that of the second, if the two firms have different degrees of leverage.[1] And since the distribution of returns after taxes of the two firms will not be proportional, there can be no "arbitrage" process which forces their values to be proportional to their expected after-tax returns.[2] In fact, it can be shown—and this time it really will be shown—that "arbitrage" will make values within any class a function not only of expected after-tax returns, but of the tax rate and the degree of leverage. This means, among other things, that the tax advantages of debt financing are somewhat greater than we originally suggested and, to this extent, the quantitative difference between the valuations implied by our position and by the traditional view is narrowed. It still remains true, however, that under our analysis the tax advantages of debt are the *only* permanent advantages so that the gulf between the two views in matters of interpretation and policy is as wide as ever.

I. *Taxes, Leverage, and the Probability Distribution of After-Tax Returns*

To see how the distribution of after-tax earnings is affected by leverage, let us again denote by the random variable X the (long-run average) earnings before interest and taxes generated by the currently owned assets of a given firm in some stated risk class, k.[3] From our definition of a risk class it follows that X can be expressed in the form $\bar{X}Z$, where $\bar{X}$ is the expected value of X, and the random variable $Z=X/\bar{X}$, having the same value for all firms in class k, is a drawing from a distribution, say $f_k(Z)$. Hence the random variable X^τ, measuring the after-tax return, can be expressed as:

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[1] With some exceptions, which will be noted when they occur, we shall preserve here both the notation and the terminology of the original paper. A working knowledge of both on the part of the reader will be presumed.

[2] Barring, of course, the trivial case of universal linear utility functions. Note that in deference to Professor Durand (see his Comment on our paper and our reply, this *Review*, Sept. 1959, *49*, 639–69) we here and throughout use quotation marks when referring to arbitrage.

[3] Thus our X corresponds essentially to the familiar EBIT concept of the finance literature. The use of EBIT and related "income" concepts as the basis of valuation is strictly valid only when the underlying real assets are assumed to have perpetual lives. In such a case, of course, EBIT and "cash flow" are one and the same. This was, in effect, the interpretation of X we used in the original paper and we shall retain it here both to preserve continuity and for the considerable simplification it permits in the exposition. We should point out, however, that the perpetuity interpretation is much less restrictive than might appear at first glance. Before-tax cash flow and EBIT can also safely be equated even where assets have finite lives as soon as these assets attain a steady state age distribution in which annual replacements equal annual depreciation. The subject of finite lives of assets will be further discussed in connection with the problem of the cut-off rate for investment decisions.

$$(1)\quad X^\tau = (1-\tau)(X-R)+R = (1-\tau)X+\tau R = (1-\tau)\overline{X}Z+\tau R$$

where τ is the marginal corporate income tax rate (assumed equal to the average), and R is the interest bill. Since $E(X^\tau) \equiv \overline{X}^\tau = (1-\tau)\overline{X}+\tau R$ we can substitute $\overline{X}^\tau - \tau R$ for $(1-\tau)\overline{X}$ in (1) to obtain:

$$(2)\qquad X^\tau = (\overline{X}^\tau - \tau R)Z + \tau R = \overline{X}^\tau\left(1-\frac{\tau R}{\overline{X}^\tau}\right)Z+\tau R.$$

Thus, if the tax rate is other than zero, the shape of the distribution of X^τ will depend not only on the "scale" of the stream $\overline{X}^\tau$ and on the distribution of Z, but also on the tax rate and the degree of leverage (one measure of which is $R/\overline{X}^\tau$). For example, if Var $(Z)=\sigma^2$, we have:

$$\text{Var}\,(X^\tau) = \sigma^2(\overline{X}^\tau)^2\left(1-\tau\frac{R}{\overline{X}^\tau}\right)^2$$

implying that for given $\overline{X}^\tau$ the variance of after-tax returns is smaller, the higher τ and the degree of leverage.[4]

II. *The Valuation of After-Tax Returns*

Note from equation (1) that, from the investor's point of view, the long-run average stream of after-tax returns appears as a sum of two components: (1) an uncertain stream $(1-\tau)\overline{X}Z$; and (2) a sure stream τR.[5] This suggests that the equilibrium market value of the combined stream can be found by capitalizing each component separately. More precisely, let ρ^τ be the rate at which the market capitalizes the expected returns net of tax of an unlevered company of size $\overline{X}$ in class k, i.e.,

$$\rho^\tau = \frac{(1-\tau)\overline{X}}{V_U} \quad\text{or}\quad V_U = \frac{(1-\tau)\overline{X}}{\rho^\tau};^{6}$$

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[4] It may seem paradoxical at first to say that leverage *reduces* the variability of outcomes, but remember we are here discussing the variability of total returns, interest plus net profits. The variability of stockholder net profits will, of course, be greater in the presence than in the absence of leverage, though relatively less so than in an otherwise comparable world of no taxes. The reasons for this will become clearer after the discussion in the next section.

[5] The statement that τR—the tax saving per period on the interest payments—is a sure stream is subject to two qualifications. First, it must be the case that firms can always obtain the tax benefit of their interest deductions either by offsetting them directly against other taxable income in the year incurred; or, in the event no such income is available in any given year, by carrying them backward or forward against past or future taxable earnings; or, in the extreme case, by merger of the firm with (or its sale to) another firm that can utilize the deduction. Second, it must be assumed that the tax rate will remain the same. To the extent that neither of these conditions holds exactly then some uncertainty attaches even to the tax savings, though, of course, it is of a different kind and order from that attaching to the stream generated by the assets. For simplicity, however, we shall here ignore these possible elements of delay or of uncertainty in the tax saving; but it should be kept in mind that this neglect means that the subsequent valuation formulas overstate, if anything, the value of the tax saving for any given permanent level of debt.

[6] Note that here, as in our original paper, we neglect dividend policy and "growth" in the

and let r be the rate at which the market capitalizes the sure streams generated by debts. For simplicity, assume this rate of interest is a constant independent of the size of the debt so that

$$r = \frac{R}{D} \quad \text{or} \quad D = \frac{R}{r}\,.^{7}$$

Then we would expect the value of a levered firm of size $\overline{X}$, with a permanent level of debt D_L in its capital structure, to be given by:

$$V_L = \frac{(1-\tau)\overline{X}}{\rho^{\tau}} + \frac{\tau R}{r} = V_U + \tau D_L.^{8} \tag{3}$$

In our original paper we asserted instead that, within a risk class, market value would be proportional to expected after-tax return $\overline{X}^{\tau}$ (cf. our original equation [11]), which would imply:

$$V_L = \frac{\overline{X}^{\tau}}{\rho^{\tau}} = \frac{(1-\tau)\overline{X}}{\rho^{\tau}} + \frac{\tau R}{\rho^{\tau}} = V_U + \frac{r}{\rho^{\tau}}\tau D_L. \tag{4}$$

We will now show that if (3) does not hold, investors can secure a more efficient portfolio by switching from relatively overvalued to relatively undervalued firms. Suppose first that unlevered firms are overvalued or that

$$V_L - \tau D_L < V_U.$$

An investor holding m dollars of stock in the unlevered company has a right to the fraction m/V_U of the eventual outcome, i.e., has the uncertain income

$$Y_U = \left(\frac{m}{V_U}\right)(1-\tau)\overline{X}Z.$$

Consider now an alternative portfolio obtained by investing m dollars as follows: (1) the portion,

$$m\left(\frac{S_L}{S_L + (1-\tau)D_L}\right),$$

is invested in the stock of the levered firm, S_L; and (2) the remaining portion,

$$m\left(\frac{(1-\tau)D_L}{S_L + (1-\tau)D_L}\right),$$

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sense of opportunities to invest at a rate of return greater than the market rate of return. These subjects are treated extensively in our paper, "Dividend Policy, Growth and the Valuation of Shares," *Jour. Bus.*, Univ. Chicago, Oct. 1961, 411–33.

[7] Here and throughout, the corresponding formulas when the rate of interest rises with leverage can be obtained merely by substituting $r(L)$ for r, where L is some suitable measure of leverage.

[8] The assumption that the debt is permanent is not necessary for the analysis. It is employed here both to maintain continuity with the original model and because it gives an upper bound on the value of the tax saving. See in this connection footnote 5 and footnote 9.

is invested in its bonds. The stock component entitles the holder to a fraction,

$$\frac{m}{S_L + (1-\tau)D_L},$$

of the net profits of the levered company or

$$\left(\frac{m}{S_L + (1-\tau)D_L}\right)[(1-\tau)(\bar{X}Z - R_L)].$$

The holding of bonds yields

$$\left(\frac{m}{S_L + (1-\tau)D_L}\right)[(1-\tau)R_L].$$

Hence the total outcome is

$$Y_L = \left(\frac{m}{(S_L + (1-\tau)D_L)}\right)[(1-\tau)\bar{X}Z]$$

and this will dominate the uncertain income Y_U if (and only if)

$$S_L + (1-\tau)D_L \equiv S_L + D_L - \tau D_L \equiv V_L - \tau D_L < V_U.$$

Thus, in equilibrium, V_U cannot exceed $V_L - \tau D_L$, for if it did investors would have an incentive to sell shares in the unlevered company and purchase the shares (and bonds) of the levered company.

Suppose now that $V_L - \tau D_L > V_U$. An investment of m dollars in the stock of the levered firm entitles the holder to the outcome

$$\begin{aligned} Y_L &= (m/S_L)[(1-\tau)(\bar{X}Z - R_L)] \\ &= (m/S_L)(1-\tau)\bar{X}Z - (m/S_L)(1-\tau)R_L. \end{aligned}$$

Consider the following alternative portfolio: (1) borrow an amount $(m/S_L)(1-\tau)D_L$ for which the interest cost will be $(m/S_L)(1-\tau)R_L$ (assuming, of course, that individuals and corporations can borrow at the same rate, r); and (2) invest m plus the amount borrowed, i.e.,

$$m + \frac{m(1-\tau)D_L}{S_L} = m\frac{S_L + (1-\tau)D_L}{S_L} = (m/S_L)[V_L - \tau D_L]$$

in the stock of the unlevered firm. The outcome so secured will be

$$(m/S_L)\left(\frac{V_L - \tau D_L}{V_U}\right)(1-\tau)\bar{X}Z.$$

Subtracting the interest charges on the borrowed funds leaves an income of

$$Y_U = (m/S_L)\left(\frac{V_L - \tau D_L}{V_U}\right)(1-\tau)\bar{X}Z - (m/S_L)(1-\tau)R_L$$

which will dominate Y_L if (and only if) $V_L - \tau D_L > V_U$. Thus, in equilibrium, both $V_L - \tau D_L > V_U$ and $V_L - \tau D_L < V_U$ are ruled out and (3) must hold.

III. *Some Implications of Formula* (3)

To see what is involved in replacing (4) with (3) as the rule of valuation, note first that both expressions make the value of the firm a function of leverage and the tax rate. The difference between them is a matter of the size and source of the tax advantages of debt financing. Under our original formulation, values within a class were strictly proportional to expected earnings after taxes. Hence the tax advantage of debt was due solely to the fact that the deductibility of interest payments implied a higher level of after-tax income for any given level of before-tax earnings (i.e., higher by the amount τR since $\bar{X}^\tau = (1-\tau)\bar{X} + \tau R$). Under the corrected rule (3), however, there is an additional gain due to the fact that the extra after-tax earnings, τR, represent a sure income in contrast to the uncertain outcome $(1-\tau)\bar{X}$. Hence τR is capitalized at the more favorable certainty rate, $1/r$, rather than at the rate for uncertain streams, $1/\rho^\tau$.[9]

Since the difference between (3) and (4) is solely a matter of the rate at which the tax savings on interest payments are capitalized, the required changes in all formulas and expressions derived from (4) are reasonably straightforward. Consider, first, the before-tax earnings yield, i.e., the ratio of expected earnings before interest and taxes to the value of the firm.[10] Dividing both sides of (3) by V and by $(1-\tau)$ and simplifying we obtain:

$$\frac{\bar{X}}{V} = \frac{\rho^\tau}{1-\tau}\left[1 - \tau\frac{D}{V}\right] \qquad (31.c)$$

which replaces our original equation (31) (p. 294). The new relation differs from the old in that the coefficient of D/V in the original (31) was smaller by a factor of r/ρ^τ.

Consider next the after-tax earnings yield, i.e., the ratio of interest payments plus profits after taxes to total market value.[11] This concept was discussed extensively in our paper because it helps to bring out more clearly the differences between our position and the traditional view, and because it facilitates the construction of empirical tests of the two hypotheses about the valuation process. To see what the new equation (3) implies for this yield we need merely substitute $\bar{X}^\tau - \tau R$ for $(1-\tau)\bar{X}$ in (3) obtaining:

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[9] Remember, however, that in one sense formula (3) gives only an upper bound on the value of the firm since $\tau R/r = \tau D$ is an exact measure of the value of the tax saving only where both the tax rate and the level of debt are assumed to be fixed forever (and where the firm is certain to be able to use its interest deduction to reduce taxable income either directly or via transfer of the loss to another firm). Alternative versions of (3) can readily be developed for cases in which the debt is not assumed to be permanent, but rather to be outstanding only for some specified finite length of time. For reasons of space, we shall not pursue this line of inquiry here beyond observing that the shorter the debt period considered, the closer does the valuation formula approach our original (4). Hence, the latter is perhaps still of some interest if only as a lower bound.

[10] Following usage common in the field of finance we referred to this yield as the "average cost of capital." We feel now, however, that the term "before-tax earnings yield" would be preferable both because it is more immediately descriptive and because it releases the term "cost of capital" for use in discussions of optimal investment policy (in accord with standard usage in the capital budgeting literature).

[11] We referred to this yield as the "after-tax cost of capital." Cf. the previous footnote.

$$V = \frac{\bar{X}^\tau - \tau R}{\rho^\tau} + \tau D = \frac{\bar{X}^\tau}{\rho^\tau} + \tau \frac{\rho^\tau - r}{\rho^\tau} D, \tag{5}$$

from which it follows that the after-tax earnings yield must be:

$$\frac{\bar{X}^\tau}{V} = \rho^\tau - \tau(\rho^\tau - r)D/V. \tag{11.c}$$

This replaces our original equation (11) (p. 272) in which we had simply $\bar{X}^\tau/V = \rho^\tau$. Thus, in contrast to our earlier result, the corrected version (11.c) implies that even the after-tax yield is affected by leverage. The predicted rate of decrease of $\bar{X}^\tau/V$ with D/V, however, is still considerably smaller than under the naive traditional view, which, as we showed, implied essentially $\bar{X}^\tau/V = \rho^\tau\text{-}(\rho^\tau - r)D/V$. See our equation (17) and the discussion immediately preceding it (p. 277).[12] And, of course, (11.c) implies that the effect of leverage on $\bar{X}^\tau/V$ is *solely* a matter of the deductibility of interest payments whereas, under the traditional view, going into debt would lower the cost of capital regardless of the method of taxing corporate earnings.

Finally, we have the matter of the after-tax yield on *equity* capital, i.e., the ratio of net profits after taxes to the value of the shares.[13] By subtracting D from both sides of (5) and breaking $\bar{X}^\tau$ into its two components—expected net profits after taxes, $\bar{\pi}^\tau$, and interest payments, $R = rD$—we obtain after simplifying:

$$S = V - D = \frac{\bar{\pi}^\tau}{\rho^\tau} - (1 - \tau)\left(\frac{\rho^\tau - r}{\rho^\tau}\right)D. \tag{6}$$

From (6) it follows that the after-tax yield on equity capital must be:

$$\frac{\bar{\pi}^\tau}{S} = \rho^\tau + (1 - \tau)[\rho^\tau - r]D/S \tag{12.c}$$

which replaces our original equation (12), $\bar{\pi}^\tau/S = \rho^\tau + (\rho^\tau - r)D/S$ (p. 272). The new (12.c) implies an increase in the after-tax yield on equity capital as leverage increases which is smaller than that of our original (12) by a factor of $(1 - \tau)$. But again, the linear increasing relation of the corrected (12.c) is still fundamentally different from the naive traditional view which asserts the cost of equity capital to be completely independent of leverage (at least as long as leverage remains within "conventional" industry limits).

IV. *Taxes and the Cost of Capital*

From these corrected valuation formulas we can readily derive corrected measures of the cost of capital in the capital budgeting sense of the minimum prospective yield an investment project must offer to be just worth undertaking from the standpoint of the present stockholders. If we interpret earnings streams as perpetuities, as we did in the original paper, then we actually have two equally good ways of defining this minimum yield: either by the required increase in before-tax earnings, $d\overline{X}$, or by the required increase in earnings net of taxes, $d\overline{X}(1-\tau)$.[14] To conserve space, however, as well as to maintain continuity with the original paper, we shall concentrate here on the before-tax case with only brief footnote references to the net-of-tax concept.

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[12] The i_k^* of (17) is the same as ρ^τ in the present context, each measuring the ratio of net profits to the value of the shares (and hence of the whole firm) in an unlevered company of the class.

[13] We referred to this yield as the "after-tax cost of equity capital." Cf. footnote 9.

Analytically, the derivation of the cost of capital in the above sense amounts to finding the minimum value of $d\overline{X}/dI$ for which $dV=dI$, where I denotes the level of new investment.[15] By differentiating (3) we see that:

$$\frac{dV}{dI}=\frac{1-\tau}{\rho^{\tau}}\frac{d\overline{X}}{dI}+\tau\frac{dD}{dI}\geq 1 \qquad \text{if } \frac{d\overline{X}}{dI}\geq\frac{1-\tau\dfrac{dD}{dI}}{1-\tau}\rho^{\tau}. \tag{7}$$

Hence the before tax required rate of return cannot be defined without reference to financial policy. In particular, for an investment considered as being financed entirely by new equity capital $dD/dI=0$ and the required rate of return or marginal cost of equity financing (neglecting flotation costs) would be:

$$\rho^{S}=\frac{\rho^{\tau}}{1-\tau}\,.$$

This result is the same as that in the original paper (see equation [32], p. 294) and is applicable to any other sources of financing where the remuneration to the suppliers of capital is not deductible for tax purposes. It applies, therefore, to preferred stock (except for certain partially deductible issues of public utilities) and would apply also to retained earnings were it not for the favorable tax treatment of capital gains under the personal income tax.

For investments considered as being financed entirely by new debt capital $dI=dD$ and we find from (7) that:

$$\rho^{D}=\rho^{\tau} \tag{33.c}$$

which replaces our original equation (33) in which we had:

$$\rho^{D}=\rho^{S}-\frac{\tau}{1-\tau}r. \tag{33}$$

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[14] Note that we use the term "earnings net of taxes" rather than "earnings after taxes." We feel that to avoid confusion the latter term should be reserved to describe what will actually appear in the firm's accounting statements, namely the net cash flow including the tax savings on the interest (our $\overline{X}^{\tau}$). Since financing sources cannot in general be allocated to particular investments (see below), the after-tax or accounting concept is not useful for capital budgeting purposes, although it can be extremely useful for valuation equations as we saw in the previous section.

[15] Remember that when we speak of the minimum required yield on an investment we are referring in principle only to investments which increase the *scale* of the firm. That is, the new

Thus for borrowed funds (or any other tax-deductible source of capital) the marginal cost or before-tax required rate of return is simply the market rate of capitalization for net of tax unlevered streams and is thus independent of both the tax rate and the interest rate. This required rate is lower than that implied by our original (33), but still considerably higher than that implied by the traditional view (see esp. pp. 276–77 of our paper) under which the before-tax cost of borrowed funds is simply the interest rate, r.

Having derived the above expressions for the marginal costs of debt and equity financing it may be well to warn readers at this point that these expressions represent at best only the hypothetical extremes insofar as costs are concerned and that neither is directly usable as a cut-off criterion for investment planning. In particular, care must be taken to avoid falling into the famous "Liquigas" fallacy of concluding that if a firm intends to float a bond issue in some given year then its cut-off rate should be set that year at ρ^D; while, if the next issue is to be an equity one, the cut-off is ρ^S. The point is, of course, that no investment can meaningfully be regarded as 100 per cent equity financed if the firm makes any use of debt capital—and most firms do, not only for the tax savings, but for many other reasons having nothing to do with "cost" in the present static sense (cf. our original paper pp. 292–93). And no investment can meaningfully be regarded as 100 per cent debt financed when lenders impose strict limitations on the maximum amount a firm can borrow relative to its equity (and when most firms actually plan on normally borrowing less than this external maximum so as to leave themselves with an emergency reserve of unused borrowing power). Since the firm's long-run capital structure will thus contain both debt and equity capital, investment planning must recognize that, over the long pull, *all* of the firm's assets are really financed by a mixture of debt and equity capital even though only one kind of capital may be raised in any particular year. More precisely, if L^* denotes the firm's long-run "target" debt ratio (around which its actual debt ratio will fluctuate as it "alternately" floats debt issues and retires them with internal or external equity) then the firm can assume, to a first approximation at least, that for any particular investment $dD/dI = L^*$. Hence, the relevant marginal cost of capital for investment planning, which we shall here denote by ρ^*, is:

$$\rho^* = \frac{1 - \tau_L{}^*}{1 - \tau}\rho^\tau = \rho^S - \frac{\tau}{1 - \tau}\rho^D L^* = \rho^S(1 - L^*) + \rho^D L^*.$$

That is, the appropriate cost of capital for (repetitive) investment decisions over time is, to a first approximation, a weighted average of the costs of debt and equity financing, the weights being the proportions of each in the "target" capital structure.[16]

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assets must be in the same "class" as the old. See in this connection, J. Hirshleifer, "Risk, the Discount Rate and Investment Decisions," *Am. Econ. Rev.*, May 1961, *51*, 112–20 (especially pp. 119–20). See also footnote 16.

[16] From the formulas in the text one can readily derive corresponding expressions for the required net-of-tax yield, or net-of-tax cost of capital for any given financing policy. Specifi-

V. *Some Concluding Observations*

Such, then, are the major corrections that must be made to the various formulas and valuation expressions in our earlier paper. In general, we can say that the force of these corrections has been to increase somewhat the estimate of the tax advantages of debt financing under our model and consequently to reduce somewhat the quantitative difference between the estimates of the effects of leverage under our model and under the naive traditional view. It may be useful to remind readers once again that the existence of a tax advantage for debt financing—even the larger advantage of the corrected version—does not necessarily mean that corporations should at all times seek to use the maximum possible amount of debt in their capital structures. For one thing, other forms of financing, notably retained earnings, may in some circumstances be cheaper still when the tax status of investors under the personal income tax is taken into account. More important, there are, as we pointed out, limitations imposed by lenders (see pp. 292–93), as well as many other dimensions (and kinds of costs) in real-world problems of financial strategy which are not fully comprehended within the framework of static equilibrium models, either our own or those of the traditional variety. These additional considerations, which are typically grouped under the rubric of "the need for preserving flexibility," will normally imply the maintenance by the corporation of a substantial reserve of untapped borrowing power. The tax advantage of debt may well tend to lower the optimal size of that reserve, but it is hard to believe that advantages of the size contemplated under our model could justify any substantial reduction, let alone their complete elimination. Nor do the data

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cally, let $\tilde{\rho}(L)$ denote the required net-of-tax yield for investment financed with a proportion of debt $L = dD/dI$. (More generally L denotes the proportion financed with tax deductible sources of capital.) Then from (7) we find:

$$\tilde{\rho}(L) = (1-\tau)\frac{dX}{dI} = (1-L\tau)\rho^\tau \tag{8}$$

and the various costs can be found by substituting the appropriate value for L. In particular, if we substitute in this formula the "target" leverage ratio, L^*, we obtain:

$$\tilde{\rho}^* \equiv \tilde{\rho}(L^*) = (1-\tau L^*)\rho^\tau$$

and $\tilde{\rho}^*$ measures the average net-of-tax cost of capital in the sense described above.

Although the before-tax and the net-of-tax approaches to the cost of capital provide equally good criteria for investment decisions when assets are assumed to generate perpetual (i.e., non-depreciating) streams, such is not the case when assets are assumed to have finite lives (even when it is also assumed that the firm's assets are in a steady state age distribution so that our X or EBIT is approximately the same as the net cash flow before taxes). See footnote 3 above. In the latter event, the correct method for determining the desirability of an investment would be, in principle, to discount the net-of-tax stream at the net-of-tax cost of capital. Only under this net-of-tax approach would it be possible to take into account the deductibility of depreciation (and also to choose the most advantageous depreciation policy for tax purposes). Note that we say that the net-of-tax approach is correct "in principle" because, strictly speaking, nothing in our analysis (or anyone else's, for that matter) has yet established that it is indeed legitimate to "discount" an uncertain stream. One can hope that subsequent research will show the analogy to discounting under the certainty case is a valid one; but, at the moment, this is still only a hope.

indicate that there has in fact been a substantial increase in the use of debt (except relative to preferred stock) by the corporate sector during the recent high tax years.[17]

As to the differences between our modified model and the traditional one, we feel that they are still large in quantitative terms and still very much worth trying to detect. It is not only a matter of the two views having different implications for corporate financial policy (or even for national tax policy). But since the two positions rest on fundamentally different views about investor behavior and the functioning of the capital markets, the results of tests between them may have an important bearing on issues ranging far beyond the immediate one of the effects of leverage on the cost of capital.

FRANCO MODIGLIANI AND MERTON H. MILLER*

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[17] See, e.g., Merton H. Miller, "The Corporate Income Tax and Corporate Financial Policies," in *Staff Reports to the Commission on Money and Credit* (forthcoming).

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Consumption, Savings and Windfall Gains: Comment

In her recent article in this *Review* [3], Margaret Reid attempted to answer previous articles by Bodkin [1] and Jones [2] challenging the validity of the permanent income hypothesis. Bodkin and Jones used income and expenditure data for those consumer units who had received the soldiers' bonus (National Service Life Insurance dividends) during 1950, the year of the urban consumption survey [4]. These bonuses were regarded as windfall gains for the purposes of their analyses.

Professor Reid used data from the same survey, but her windfall gains were represented by "other money receipts." These are defined as "inheritances and occasional large gifts of money from persons outside the family . . . and net receipts from the settlement of fire and accident policies" [4, Vol. 1, p. xxix]. She assumed that the soldiers' bonus was included, and that it accounted for about one-half of other money receipts. Here she made an unfortunate mistake in interpreting the data for the main critical purpose of her article.

The soldiers' bonus is not part of "other money receipts" (O) but rather a part of "disposable money income" (Y). It is the main part of an item in the disposable money income category called "military pay, allotments, and pensions" [4, Vol. 11, p. xxix].

This would appear to alter completely the relationship of Professor Reid's main findings to the Bodkin results and to change the windfall interpretation of the O variable. Surely, fire and accident policy settlements are not windfall income, but rather a (partial) recovery of real assets previously lost. Likewise, inheritances are probably best considered as a long-anticipated increase in assets—not an increase in transitory income.

The discovery of this error probably does not affect whatever importance Professor Reid's secondary finding may have: ". . . the need, in any study of savings and expenditure for durable goods, for differentiating consumers by tenure and by year of purchase of their dwelling unit" [3, p. 736]. However, the interpretation of the results obtained by Bodkin and Jones remains untouched by her article.

ROGER C. BIRD*

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Consumption, Savings and Windfall Gains: Reply

Mr. Bird's report on my misinterpretation of "other money receipts" reopens the issue as to whether the behavior of consumers with respect to the bonus differed from that with respect to other disposable income. The city averages yield information of interest in that the bonus is likely to be positively correlated with that category of income described as military payments.[1] Such payments, here symbolized by M, will, under this condition, serve as a crude proxy for the bonus; and the permanent-income theory predicts that the marginal propensity to spend for consumer durables will tend to be higher with respect to M, than for Y_{-m}, that is money income other than M. In addition, it predicts that for other consumer products the propensities will be higher for Y_{-m} than for M.

These relationships have been estimated for 58 cities.[2] Consumer durables are represented by expenditures for furnishings and equipment plus the net purchase price of automobiles and symbolized by C_{fa}. Other expenditures are symbolized by C_o.

The correlations are as follows:

$$C_{fa} = \$\ 70 + \$0.622M + \$0.100Y_{-m} \qquad R^2 = .41 \tag{1}$$

$$C_o = \$230 + \$0.580M + .831Y_{-m} \qquad R^2 = .86 \tag{2}$$

Thus out of every \$100 of M, \$62 went to consumer durables, and of Y_{-m}

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[1] Military payments include mustering-out pay, bonuses, war insurance refunds, unemployment insurance, dependency allotments, and retirement, survivor, service-connected disability pay and other allowances. See *Study of Consumer Expenditures, Incomes and Savings: Statistical Tables, Urban U.S.—1950*, Vol. XI, p. xxix.

[2] These are the 49 cities for which at least 100 consumer units reported plus the nine classes of cities, that is, large cities, suburbs, and small cities in the North, South, and West.

only $10. Furthermore, $r_{C_{fa}M.Y_{-m}}$ is .50, and the corresponding partial for Y_{-m} is .56. Thus both types of income contribute appreciably to the explanation of C_{fa}.

On the other hand, out of every $100 of M, $58 went to C_o, and of Y_{-m}, $83. However, $r_{C_0M.Y_{-m}}$ is only .16, and the corresponding partial for Y_{-m} is .93. Thus, M contributes little to the explanation of C_o.

These findings are not inconsistent with the permanent-income hypothesis. Information on the bonus by cities would permit a more definite estimate.

One further piece of information is of interest: the percentage of consumer units receiving M explains 20 per cent of the variation among the 58 cities in the percentage who were tenants at the beginning and owners at the end of the survey year. Thus, it is reasonable to infer that the bonus in some measure, like other money receipts, tended to be used as the down payment on a dwelling unit.

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Windfall Income and Consumption: Comment

Two recent publications in this *Review* have examined the magnitude of the marginal propensity to consume windfall income. The present writer, working with data based on the B.L.S. Survey of 1950, found that the National Service Life Insurance dividend payments of that year were largely spent by their recipients [2]. The regression coefficient estimates of the marginal propensity to consume this receipt were .97 or .72, depending upon whether durable goods expenditure was included in or excluded from the concept of consumption. Mordechai E. Kreinin, using the Israeli Survey of Family Savings of 1957-58, observed a weak tendency to spend restitution payments (from West Germany) on the part of the recipients [3]. Kreinin estimated that the relevant marginal propensity to consume was .17 if durables were included in consumption and .16 if they were excluded. Thus the conclusions of these two studies, which seemingly focus on the same type of income receipt, appear diametrically opposed.

Several surface explanations come to mind, and these will be discussed below. In this note, however, I shall argue that the principal explanation of the differing results is to be found in certain institutional features surrounding the restitution payments received by Israeli residents. In particular, it will be contended that the foreign exchange regulations pertaining to these payments provided a powerful incentive for the recipients *not* to spend these receipts, but to hold them instead in liquid assets in the form of foreign exchange.

During the years 1957 and 1958 (the period of Kreinin's study), some moderately complicated regulations governed the way in which Israeli residents could dispose of their restitution receipts.[1] If these payments (originally in West German marks) were converted into Israeli pounds, there was no restriction on the use of these receipts. But Israeli citizens were free to spend abroad, at the time of payment or in the future, only one-fifth of their total receipts, either for imported goods for personal use or for travel expenses abroad. In addition, recipients were allowed to spend, on foreign travel, a maximum of $1200 per person (a member of the immediate family) in any one year. The remaining four-fifths had to be converted into Israeli pounds, either immediately or at some future date. If the recipient elected immediate conversion, a 20 per cent premium over the official exchange rate was paid, in the form of government securities. If the recipient (presumably anticipating a future devaluation) wished to convert these funds at a later date, he was required to deposit them in a special account. If the recipient wished to purchase foreign securities, the regulations required him to finance this placement out of the unrestricted (the 1/5) portion of his receipt.

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[1] The discussion in this paragraph is based on K. Yaacobi's letter to the author [5]. The original statement of these regulations appeared in directives issued to Israeli commercial banks. These directives, which of course were written in Hebrew, have not been translated into English.

It seems likely that these regulations would have led the recipients to have saved their restitution payments or at least to have deferred the associated consumption. Indeed, economic pressures to defer immediate consumption existed for both portions of these receipts. The restricted (the 4/5) portion might have been saved, by at least some recipients, in anticipation of a devaluation of the Israeli pound, as it was well known that the official exchange rates overvalued the currency (relative to either its purchasing power or a free market rate). The fact that there was a 20 per cent premium on immediate conversion suggests that the authorities wished to take some action against this type of speculative motive, without, however, coercing the recipients into an immediate conversion. Furthermore, when the devaluation finally came, on February 9, 1962, the depreciation of the Israeli pound was considerably greater than 20 per cent [4, p. 1]. (The official exchange rate went from 1.8 Israeli pounds per dollar to 3.0 Israeli pounds per dollar.) Thus, it seems likely that the overvaluation of the exchange rate induced at least some of the recipients of these restitution payments to defer immediate conversion and hence to forego, in large measure, immediate consumption.[2] Kreinin's sample (employed salary recipients, ages 20-55) might be expected to have been reasonably sophisticated in financial matters and hence aware of such a possibility. The estimate of the Bank of Israel that 45 per cent of the restitution payments went into liquid assets is also consistent with this supposition [1] [3, paraphrased, pp. 388-89].

Moreover, there were economic pressures to defer consumption out of the unrestricted (the 1/5) portion of the restitution payments. With foreign exchange restrictions, an individual recipient would not have been able to obtain in the future unlimited foreign exchange for personal use. Consequently, he might have deferred consumption out of at least a fraction of this portion, also. (For example, the recipient might have saved these funds for use on a trip abroad at a later date.) In addition, the unrestricted portion of the restitution payments would have constituted, for many of the recipients, the only foreseeable opportunity to obtain foreign securities. For this reason, also, the foreign exchange restrictions may have encouraged some saving out of even the unrestricted portion of the restitution payments.

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[2] The decision to forego immediate conversion of the restitution funds could have been accompanied by increased consumption, if the recipient possessed a large enough holding of liquid assets and if in addition he regarded his nonconverted restitution fund as a reasonable substitute for these liquid assets. But this would appear to be a rather special case.

The superficial differences between the Bodkin and Kreinin studies deserve perhaps a quick mention. Kreinin's restitution recipients had, as a group, experienced many hardships (and worse) during an earlier period of their lives. It seems quite likely that such experiences would make individuals cautious, alert to adverse possibilities, and hence less likely to spend a one-time receipt than would be likely for a group of young American veterans. Secondly, the restitution payments might be considered different in character (as well as in magnitude) from the National Service Life Insurance dividends. The restitution payments might be considered analogous to insurance receipts, as they were intended to be a partial compensation (or, as the name states, restitution) for losses sustained by the individual in the past. Thus, some individuals might have regarded their receipts as similar to a return of capital and hence might have been less prone to consume out of such a receipt. Finally, casual observation suggests that the typical restitution payment was not a surprise to the recipient at the time of payment, but had been the subject of some prior negotiation. Thus, some skepticism as to whether these receipts qualify as a "windfall" seems justified. By contrast, as the author has already argued [2], the period between the announcement of the National Service Life Insurance dividend and the actual payments appears to have been very short.

In summary, the differing results found by Kreinin and Bodkin may be largely attributable to the foreign exchange regulations governing restitution payments received by Israeli residents. It seems quite clear that these regulations created strong incentives to save these receipts or at least to postpone immediate consumption financed by them. For this reason, Kreinin's analysis may be regarded as a rather weak test of the relevant strand of Milton Friedman's permanent income hypothesis.

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* The author, who is a lecturer in economics at Yale University, wishes to acknowledge his indebtedness to Menahem Yaari and to K. Yaacobi. The responsibility for errors of fact or analysis remains exclusively his own. Dr. Yaacobi is a Legal Advisor to the Ministry of Finance (Foreign Exchange Division) of the State of Israel.

Windfall Income and Consumption: A Further Comment

My original paper pointed to the incomparability of the American and Israeli data. The references to the "availability of goods on the markets" (page 389) and the possible earmarking of the restitution receipts "for future consumption" (page 390) alluded in part to the phenomena which Bodkin discusses here. They were hypothetical then, and remain so now. Bodkin makes them explicit but sheds no light on the magnitude of their effect. Suppositions are not an adequate substitute for empirical measurements.

There is very recent evidence that these factors had at least some impact. In assessing the first postdevaluation year (February 1962-February 1963), the Israeli press continuously refers to the increase in domestic consumption (not fully anticipated by the planning authorities) financed in part by the conversion of restitution receipts to Israeli pounds at the new and more favorable rate of exchange. However, the extent to which they contribute to a reconciliation of Bodkin's and my results can be determined only by a quantitative assessment of the amount and kind of expenditures. The most suitable data for such an investigation would be reinterviews of restitution recipients at a postdevaluation date.

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The Ranis-Fei Model of Economic Development: Comment

In the September 1961 issue of this *Review*, Gustav Ranis and John Fei presented an elegant model which (together with Harrod's equation) may supply us with the best framework for thinking about macrodynamic problems of underdeveloped countries. In their two-sector model, the migration of workers from agriculture to industry is the central process around which the theory is constructed, taking into account certain basic problems, e.g., worsening terms of internal trade, disguised unemployment, institutional wages, commercialization point, balanced growth, and technological changes. I feel that these problems should be discussed further, if only to clarify the issues for empirical research.

Ranis and Fei divide the development of surplus-labor countries into three stages, assuming as a first approximation no changes in agricultural productivity and population. In the first phase farm workers who are not adding to farm output are shifted to the industrial sector with no rise in the existing wage levels of the economy since there is no reduction in farm output. In the second phase, however, farm workers producing something, but less than their wages, are shifted to the industrial sector. Farm output declines and the terms of trade between the two sectors are altered in favor of the farm sector, requiring a rise in the nominal wage of the industrial sector. If migration continues, a point is eventually reached where farm workers producing output equal to their wages (the commercialization point) are shifting to industry. This begins the third stage, marking the end of the take-off and the beginning of self-sustained growth, and "we enter a world in which the agricultural sector is no longer dominated by nonmarket institutional forces but assumes the characteristics of a commercialized capitalistic system" [10, p. 543].

With agricultural productivity increasing, the model shows that the difficult second phase can be shortened or skipped. This is because the rise in the marginal physical productivity (MPP) of migrating workers enables their output (1) to catch up with their wages more quickly than otherwise, and (2) to generate more agricultural surplus per migrating worker (that part of his consumption which he is not producing), lessening the deterioration of the terms of trade. (These results are obtained because wages are assumed to be unchanged although farm productivity is rising.) If the rise in productivity is sufficient, the entire second phase can be eliminated, enabling the economy to move smoothly into self-sustained growth.

In this note, I would like to raise some serious issues about the validity and empirical relevance of the Ranis-Fei model, especially with respect to problems of foreign trade, disguised unemployment and its correlate, institutional wage. At the end, the conclusions of Ranis and Fei are examined in the light of our discussion.

1. *The worsening of the terms of trade characterizing the second phase.* The worsening occurs in the model mainly because the authors have excluded foreign trade. In an open economy, industrial goods can be and are in fact exchanged for the agricultural goods produced in a foreign country, multilaterally, if not directly. In Asia, the Indo-Chinese countries have exported food to the major cities of the deficit countries to the south—Philippines, Malaya, Indonesia, Ceylon. If the Ranis-Fei model is to be of general applicability, it is imperative that the authors bring foreign trade into their model and work out its implications. Otherwise the theory will remain a very special one. It seems highly artificial to assume a closed system in a theory which makes the worsening of the terms of trade a cornerstone. For example, in Asia, if import of agricultural products is assumed, the terms of trade need not change. In Japan before the war, the import of cheap farm products from Taiwan and Korea improved the terms of trade for the industrial sector.

2. *The commercialization point which begins the third phase.* To the authors, this is a "major landmark in the development process." Enough workers have left the farms, leaving a labor shortage in the countryside. Landlords must now pay wages commensurate with MPP and must compete with industrial employers. It appears to me that in this closed model this point will be the beginning of hyper-inflation. For now, to the extent of the MPP of the workers leaving the farm, there will be a shortage of agricultural products, and this shortage will increase rapidly as the migration continues, even if some decline in the income elasticity of agricultural-product demand is taken into account.

Let us assume an open economy. A large part of the industrial product in the main cities will be exported to foreign countries and exchanged for farm products, as in prewar Japan. In this case, a large peasant economy, predominantly subsistence, can coexist with a highly industrialized sector, with MPP = W in the economy as a whole. Instead of "commercialization," we have here a dualistic economy. Thus the Ranis-Fei model shows that, even after the commercialization point is reached, there must be continual increases in agricultural productivity in the absence of foreign trade if the farm population is to continue to fall.

3. *Disguised unemployment and institutional wages.* Ranis and Fei feel that in certain countries—Pakistan, India, and Ceylon—marginal farm workers producing little or no output may be as large as 30 per cent of the farm labor force. They refer to my denial of the existence of substantial (say 5 per cent or more) zero-marginal product workers [8]. They observe that "with a fixed amount of land, there will be *some* size of population which is large enough to render MPP zero" [10, p. 547]. I would agree, but in such cases the excess population will move to the cities and in the short run will be openly unemployed, as is the case in most parts of Asia today. In due time, these unemployed workers will be absorbed into small industries and shops or will return to their villages. In the long run, the amount of land is by no means fixed, as the statistics of farm acreage for Asian countries show.[1] Extra-marginal land is brought into cultivation, e.g., hillsides, forests, and jungles. Traditionally in peasant societies the younger sons who cannot be provided with family land must go out to the cities and towns or rent out marginal land and eke out a living.[2] Though their returns are low, they are substantially above zero. Since disguised unemployment plays such an important role in many of the theories of underdevelopment, we need some empirical investigations into this matter.[3]

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[1] Rice acreage harvested in the major Asian countries (excluding Red China) rose from an annual average of 52.3 million hectares in the years 1934-38 to 65.9 million for the period 1955-59, a growth rate of about 1 per cent per year, which is roughly the growth rate of the farm population in this period. See FAO [1].

[2] B. Johnston and J. W. Mellor [4, p. 568]: "The most common of these institutional arrangements is the family farm in which the unit of production is also the unit of consumption."

[3] There is one empirical study, that of J. W. Mellor and R. D. Stevens [7], of 104 farms in one Thai village. In this pioneer study, the conclusion is reached that there is substantial zero MPP farm workers. I feel it is hazardous to regard this study as conclusive for either theoretical or policy use. The spread of the data in the scatter diagram relating rice yields to labor input for each of the 104 farms suggests to me, not a linear regression line as it does to the authors, but inadequate data and/or dubious assumptions. For example, they assume that rice production functions for each of the 104 farms were the same. (Dr. Gordon R. Sitton [11] found much variability in what appeared to be homogeneous situations in a Thai village.) In estimating labor input, the authors exclude working children under 15 years old and include all persons 15 years and above, whether working or not. The concept relevant to disguised unemployment should include all labor actually put into cultivation. The labor of an owner-cultivator should not be equated with that of a hired worker, that of a male worker with that of a female worker, nor that of an adult worker with that of a young worker. One of the authors, J. Mellor, in a recent study of an Indian village did not find evidence to support the thesis of disguised unemployment [6]. In fact, the data suggested "that yields can be raised by greater use of labour . . . ," a conclusion similar to that of G. R. Sitton for Thailand [11].

According to Edwin F. Jones [5 pp. 4-5], the basis for the shift to commune agriculture in Mainland China was the belief by Mao Tse-tung that there was much disguised unemployment in rural China. When it became clear that the commune movement was a failure, large masses of people were shifted back to agriculture in 1962. In Taiwan, Empirical studies are also necessary to substantiate the concept of an institutional wage, i.e., a caloric-minimum wage substantially higher than MPP which the landlord is willing to pay in order to prevent wage levels from falling to caloric levels too low for efficient work. This assumes that the redundant workers have no other place to go and that cultivable land is fixed in the long run. And it contradicts the usual picture of sharp struggles between landlord and tenant in Asian countries. See, e.g., [3]. On farm wage systems, see [8]. I think a stronger case can be made for assuming wages to be below MPP.[4]

4. *Productivity changes and institutional wage.* Farm technological change is shown in the model by drawing new total productivity curves denoting an upward shift. The flat portion of the curves representing zero MPP labor force is reproduced in the new curves since the shift is assumed to be proportional.

This type of technological change is unlikely to be general under the conditions of Asian paddy agriculture. Even in the case of the introduction of improved seeds (which seems to fit the Ranis-Fei case best), this is likely to call for more fertilizing and more weeding, both requiring additional labor. In this situation, the flat portion of the new curves may be wiped out. Where productivity rises because of the construction of irrigation facilities, new land can be put into cultivation and double-cropping on the old land becomes feasible, both requiring more labor. And in the Japanese type of rice-growing technique which is succeeding in many parts of Asia, labor requirements per acre increased due to careful breeding of seedlings, transplantation, fertilizing, weeding, etc. In all these cases, the flat portion of the new productivity curves will disappear and there is not likely to be proportional upward shifts in other parts of the new curve.[5]

The part of the model (and most crucial to it) which is most difficult to accept from an empirical standpoint is the assumption that, with the rise in farm productivity, farm wages in the first two phases remain unchanged. A general rise in Asian farm productivity will be immediately reflected in a rise of real income for the peasant-proprietor and his family, since the tax structure is such that only a small part of the increases is likely to be taxed away. The absolute share of tenants will increase because in Asia the total crop of landlord farms is often divided on the basis of percentage shares. I suspect that investigations into farm wages will show that, with productivity increases, wages do rise in Asian agriculture.

In conclusion, if the Ranis-Fei model is to be made more generally appli-

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between 1950-1956, agricultural output grew at an annual average rate of 6 per cent but this required a growth in farm labor input of about 2 per cent[2, Appendix Table]. Japan's experience in the Meiji period appears to have been similar to that of Taiwan [9].

[4] If we drop the assumption regarding disguised unemployment, the flat portion of the total and marginal productivity curve in the model will be replaced by a gently rising slope. The first phase loses its distinctive character and becomes merged with the second as far as the industrial supply curve of labor is concerned.

[5] However, it must be admitted that we know very little about the nature of the shifts in productivity curves in Asia and more studies are indicated.

cable by bringing in foreign trade and leaving out disguised unemployment and constant institutional wage, the following modifications are indicated: by bringing in foreign trade rapid rises in farm productivity become less urgent and balanced growth appears easier to attain; on the other hand, by excluding disguised unemployment the need for increasing agricultural productivity is enhanced. And even though (1) and (3) will mean that concepts such as shortage, commercialization, and turning points, take-off and self-sustained growth will drop out of the model, the model's service in depicting the importance of food and other agricultural products in the accumulation of capital in low-income countries is a noteworthy contribution in the literature of underdevelopment.

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The Ranis-Fei Model of Economic Development: Reply

We are grateful to Mr. Oshima for his comments on our paper and heartily endorse his effort to nudge our model closer to the real world and towards a fuller empirical verification. We certainly agree, for example, now as before [3, footnote 22, p. 554], with his suggestion that the relaxation of our closed-economy assumption would represent an important next step in the evolution of our model; such relaxation would considerably soften the balanced growth constraint by relaxing a source of rigidity in the system. Similarly, it might be pointed out that foreign aid (e.g., shipments of P.L. 480 wheat) may also play a role in postponing the arrival of the shortage point. In general the importance of such open-economy aspects of the development process depends both on the size of the domestic economy and on the opportunities to participate in foreign trade under mid-twentieth century conditions. As the recent Ford Foundation's agricultural team on India indicated, the problems of large less-developed economies must basically be solved internally as the huge domestic food requirements cannot conceivably be met through major reliance on imports.

Although our article did not concern itself formally with development after the "turning point," Oshima's characterization of this landmark as "the beginning of hyperinflation" in the closed economy strikes us as somewhat misguided. To us, reaching the turning point signifies a major measure of success in the development effort in the sense that the disguisedly unemployed in the hitherto "dragging" agricultural sector have finally been productively mobilized. Thereafter we view the agricultural sector as moving towards the role of an "overly productive" appendage to the industrial sector which must be subsidized (as is typical in many advanced countries) rather than squeezed for the benefit of the rest of the economy.

Oshima questions four assumptions of our model: the long-run fixity of land, the existence of disguised agricultural unemployment, the level of the institutional real wage, and the nature of technological change. With respect to the fixity of land, we are simply adhering to the classical (Ricardo) assumption which we think to be particularly relevant to the many less-developed countries where a long history of population pressures has served to render rather unlikely further significant increases of cultivable acreage attainable at reasonable cost.

With respect to the disguisedly unemployed agricultural labor force, Oshima seems to have abandoned his "seasonal peak agricultural labor demand" argument [2] and now grants the existence of surplus population either disguisedly unemployed in the fields or openly unemployed in the cities. Clearly, the physical location of such unemployed workers is immaterial to our analysis as long as they are consumers of, but non-contributors to, the national product.

With respect to the level of the institutionally (or calorically) set minimum real wage, we find ourselves in more serious disagreement with Oshima's contention that such a wage is likely to lie below the MPP of labor rather than, as we assume, above it. While we certainly endorse his call for empirical research on this issue, we would like to emphasize that, if Oshima is right, what we really face here is a *shortage* of agricultural labor in the sense that landlords will want to hire additional workers to increase agricultural surplus. Under such assumptions of a Marxist-type exploitation theory of agricultural wage determination, the correct policy conclusion is the encouragement of further population increases! Small wonder that the overwhelming consensus on this issue (e.g., Lewis, Nurkse, Rosenstein-Rodan) is on our side of the argument.

With respect to the nature of technological change in the agricultural sector, it can easily be shown that the innovations which we have assumed are neutral in the Hicksian sense, i.e., they raise the MPP_L curve everywhere by the same constant multiple, leaving the horizontal (i.e., 0—MPP_L) portion of the curve unaffected. Oshima points out that innovations are, in fact, likely to be labor-using, causing a disappearance of the horizontal portion of the curve in the course of the growth process. While we may accept the possibility or even likelihood of labor-using innovations where there exists a substantial redundant agricultural labor force, such activity would not annihilate the horizontal portion of the *TPP* curve immediately, but only gradually over time. The solution provided in our model really constitutes a first approximation based on the assumption of neutrality of innovations. Oshima's proposition would involve a modification of our constant T (non-redundancy coefficient) assumption and can be accommodated in our model. Clearly such modification with an equivalent upward shift of the agricultural production function would, under *ceteris paribus* assumptions, lead to an earlier arrival of the shortage point.

Finally, Oshima finds it difficult to accept our simplifying assumption of the constancy of the real wage under conditions of increasing agricultural productivity. While we do not, now as before, deny that the real wage may increase either at the source of labor supply (i.e., in the agricultural sector) [3, footnote 6, p. 538] or in the industrial sector due to a gradual widening of the "wage gap" between the two sectors [3, p. 554], the basic question is whether such increases are likely to be significant. In the case of nineteenth century Japan, increases in the real wage were extremely moderate while agricultural productivity increased sharply.[1] In the contemporary world it may well be that a combination of ineffective fiscal systems and misguided welfare emphases contributes to more substantial increases in the industrial real wage over time. It is, however, difficult to imagine how such increases can be sustained in the face of a pool of the disguisedly unemployed in the agricultural sector.

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[1] Referring to Umemura's data [4], Kaldor states that "in the case of Japan, real wages increased very little between 1878 and 1917, despite a one-and-a-half-fold increase in real income per head over the period" [1, p. 294].

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BOOK REVIEWS

General Economics; Methodology

Economic Philosophy. By JOAN ROBINSON. London: C. A. Watts & Co., 1962. Pp. 147. 15 s.

The intellectual climate in the Western world today is one of disillusionment and disorientation. Gone is the faith in progress and enlightenment; and in the field of economics gone is the centuries-old confidence that ours is a progressive discipline destined to contribute to the steady advancement of human welfare. A recently delivered Presidential Address at the American Economic Association's annual meeting turned in as a matter of course the profession's collective resignation from playing an active part in determining the course of society by observing that "in the long run, the economic scholar works for the only coin worth having—our own applause." And an altogether differently oriented outstanding British economist at about the same time took a long look at the history of economic thought and concluded in despair that "the solutions offered by economists were no less delusory than those of the theologians that they displaced." Mrs. Robinson's outlook for the future is no more hopeful: "Social life," she predicts, "will always present mankind with a choice of evils," and economics "offers no 'better 'ole' to go to . . . [because] . . . there is no 'better 'ole' " (p. 146).

In an age when the harnessing of atomic energy is not helping to solve the problem of scarcity in the world but rather serves as a basis for an "overkill" arsenal of means of destruction, when in the United States alone the conservatively estimated "cumulative excess of potential over actual output in the period 1958 to 1962 totals $170 billion,"[1] when urgent social problems at home and the desperate needs of two thirds of the human race remain unattended to—in such an age it is indeed hard to resist being swayed by the mood which words like these reflect. In the face of such irrationality, the melancholy question "what is the use?" must obviously come to the mind of many an economist, in particular of one who, like Mrs. Robinson herself, has done so much for the advancement of a more rational understanding of the economic process.

It is this melancholy question that dominates the compact, witty, and suggestive little book under review which might well be called "A Brilliant Woman's Guide Away From Economics." Ranging over wide areas of social philosophy, methodology, economic theory and policy, it demonstrates the persistent bias in favor of the *status quo* which has vitiated the work of most economists and made it "the business of the economists, not to tell us what to do, but show why what we are doing anyway is in accord with proper principles" (p. 21).

And yet, as so often happens when moods come to influence analytic efforts, this one too results in a certain misplacement of accents. Thus there cannot be much quarrel with Mrs. Robinson when she shows how from Adam Smith on, nearly every economist committed more or less grievous errors of reasoning.[2] Still the bitter characterization of economists as a never-ceasing, self-propelling sequence of misconceptions, metaphysical vacuities, and partial pleading would seem to convey an inaccurate impression of the evolution (and the role) of economic thought. To attribute at the same time the present state of the world to the undeniable failings of economics, as Mrs. Robinson tends to do, amounts moreover to assigning it by implication a truly extraordinary importance and an almost unique historical responsibility.

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[1] *Economic Report of the President*, Transmitted to the Congress January 1963, p. 28.

Neither view would appear to me to be warranted. In the first place, there is hardly any aspect of what is now considered, by Mrs. Robinson as well as by many others, to be a more rational view of economic development that has not been arrived at as the result of the long history of economic thought. In working out his models of simple and expanded reproduction which, in the light of subsequent achievements of economics, appear to have been among the most significant contributions to economic analysis, Marx was inspired by the earlier efforts of the physiocratic leader, Quesnay. Michael Kalecki, who, as Mrs. Robinson recognizes, independently discovered the *General Theory,* remarks in a collection of essays (written in the years 1933-39 when this discovery was formulated)[3] that his conceptions bear a relation to the theory of Rosa Luxemburg, who in turn was following in the footsteps of Marx. And although the accounts of the more recent history of economic thought tend to give the impression of the immaculate conception of Keynes' *General Theory,* it is probably not unreasonable to suppose that even if Keynes himself never descended into the "underworlds of Karl Marx, Silvio Gesell or Major Douglas," the rumblings in that basement did not fail to influence his reflections.

Secondly, the development of economic thought, as of any thought for that matter, is surely less fortuitous and less "autonomous" than a listing of "great books" would suggest. As Marx observed: "Bourgeois society is the most developed and the most complex historical organization of production. The categories that express its relation, the comprehension of its structure, thus together afford an insight into the organization and the productive relations of all those lost forms of society from the ruins and elements of which it is constructed. . . . In anatomy man is the key to the anatomy of the apes."[4] In fact, it is apparently only from the vantage point of advanced capitalism that we begin to understand more adequately the working principles of the competitive system, just as the increasing comprehension of the principles of planning accords us a deeper insight into the *modus operandi* of the corporate system. Wouldn't this suggest, as we are told in Ecclesiastes, that "there is a time for every matter under heaven," and that no more can be known than is knowable at any given time?

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[2] I must acknowledge that Mrs. Robinson is right when she points out (p. 93) that I also have been unable to escape that fate; the only comfort is that, at least in her account, I find myself in truly illustrious company.

[3] *Prace z Teorii Koniunktury 1933-1939* (Works on the Theory of Fluctuations 1933-1939), Warszawa, 1962.

[4] *Grundrisse der Kritik der Politischen Oekonomie (Rohentwurf) 1857-58,* Berlin, 1953, p. 25.

Thirdly, I should think that when seeking to assess the value of the work of an economist, the foremost requirement is to formulate clearly the criteria by which it is to be judged. There is the question as to the impact exercised on the consciousness of society and the part played in the social and intellectual tensions of the time. And there is a different problem: in what measure do the economist's efforts advance (or sidetrack) the clarification of the conditions for and the requirements of a more rational and more human economic and social order? If judged by these standards rather than by what a writer has to say about the determination of relative prices, about the mysteries of banking policy, or about stock market fluctuations, a number of economists could claim a very high position indeed in the firmament of human thought. What is more, looked upon in this light, modern economists who "hide in the thickets of algebra" (p. 119) may also call for a differentiated appraisal. The work of many of them undoubtedly constitutes nothing but an escape from the commitment to expose and to criticize the prevailing irrationality; that of others—regardless of their individual intentions—makes genuine and important contributions to the toolbox of a rational administration of economic life and advances significantly the theory of economic planning (input-output analysis, programming, formulation of correct investment criteria and the like).

What is perhaps most important, the disappointment of Mrs. Robinson and others with the failure of economics to show society a "better 'ole' " is itself an aspect of what she herself castigates as a metaphysical component in economic thought. Of all the dicta of Keynes, probably the least substantiated is his contention that "The ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else." To expect economic science, however flourishing, to change the underlying reality is a form of "scientism" which wholly miscontrues the mechanism of historical dynamics. Not that ideas, and scientific ideas in particular, have no part to play in historical development. It is only that the emergence, acceptance, and power of ideas at any stage of historical evolution must be explained rather than accepted as irreducible data. Occasionally Mrs. Robinson sees this very clearly, as for instance when she points out that the wide acclaim which free trade doctrines once enjoyed in Britain had much to do with the fact "that in the pre-1914 world Great Britain had everything to gain from other nations' adopting free trade and very little to lose from maintaining it herself" (p. 65). But these glimpses are overshadowed by her constant stress of *ideas* as prime movers in history. Thus speaking of India and China she writes: "Western liberalism had only warmed the surface of the deep waters of Indian tradition, while in China a violent reversal of *ideas* has opened the way for rapid changes in technology and in the social forms appropriate to exploiting them" (p. 113, my italics). There were surely many differences in the developments in Indian and Chinese societies in the course of the last 15 years to account for the divergence of the ideas that have prevailed in the two countries. As against this overemphasizing of the role of ideas, the "vulgar" insistence on the strategic importance of *interests* would seem to be a more scientific, less metaphysical position.

The trouble with economics is not that it does not yet "know enough," as many of its practitioners love to repeat. Its fatal shortcoming is that it does not incorporate in its knowledge the understanding of what is necessary for the attainment of a better, more rational economic order. Hemingway's Old Man was a virtuoso fisherman. If he had a fault, it was his incapacity to realize the overwhelmingly destructive power of the sharks.

PAUL A. BARAN

Stanford University

Capitalism and Freedom. By MILTON FRIEDMAN. Chicago: University of Chicago Press, 1962. Pp. v, 202. $3.95.

This rewrite of lectures and articles by Milton Friedman is largely a defense of liberalism against "liberalism"—that is, (to spell it out since the quotation marks are easily transposed) of nineteenth-century European liberalism's concern for individual freedom against twentieth-century American "liberals." To these he refers (quoting Schumpeter) as "the enemies of the system of private enterprise [who] have thought it wise to appropriate its label" (p. 5), and he characterizes them by their readiness to rely on the state for the furtherance of welfare and equality.

Although Friedman is no more against welfare than the "liberals" are against freedom, he seems to have reacted so strongly against the failure of many "liberals" to understand the nature, the merits, and the potentialities of the market system, and their consequent tendency, in or out of season, to cry "there ought to be a law," that he tends to cry "never, never pass a law —well hardly ever!" In this he is buttressed by a deep political pessimism about the inevitable corruption of even the best-intentioned government action by narrow partisan interests. The result is that although he erects his system on an unimpeachable framework that all "liberals" as well as all liberals would accept—recognizing that a case may be made for government intervention whenever the market system fails to work satisfactorily by reason of monopoly or "neighborhood effects" (divergence between private and social benefits or costs)—his pessimistic antigovernmentism leads him to some strange extremes.

Thus, he is against fair employment practices legislation (FEPC) because by admitting the government into that area one opens the way to unfair employment practices legislation like the Nuremberg laws (UEPC?). (He does *not* object to government enforcing honest labeling on the grounds that this would open the way to enforcing or protecting dishonest labeling.) His treatment of racial and other discrimination as a matter of taste whose gratification is legitimate (however bad the taste) would be applicable, and not merely elegant, if the resulting supply of black, white, and mixed schools or housing had no great "neighborhood effect" in depriving Negroes of reasonable facilities. His displeasure with the Keynesian concentration on fiscal policy (a result of its development at a time when monetary policy was not working well or was being worked very badly) leads him to declare the belief that "a rise in government expenditures relative to tax receipts, even when financed by borrowing, is *necessarily* expansionist" (page 82) to be based on one or the other of two extreme assumptions; either a liquidity elasticity of infinity (the liquidity trap) or an investment elasticity of zero (collapse of confidence). But although one of these extreme assumptions is required for the belief that *monetary policy* is completely ineffective, neither is really necessary for the belief that expansionary *fiscal policy* "is *necessarily* expansionist." All that is needed is the rejection of the opposite extreme assumption of an unchangeable velocity of circulation. Friedman himself judges that "$100 increase in government expenditures can on the average be expected to add just about $100 to income" (page 84), so that, unless the government expenditures have no value whatsoever, they would bring about a significant expansion not only of employment but also of useful output. Nor would Friedman deny that in time of less than average prosperity the multiplier would be greater than unity. Nevertheless, he concludes that the belief that expansionary fiscal policy can relieve depression is "economic mythology" (*ibid.*).

And yet in spite of these and a few other extravagances, *Capitalism and Freedom* is an important and valuable book with which as a liberal "liberal" I find myself in enthusiastic agreement some 90 per cent of the time. For the book powerfully demonstrates an impressive number of ways in which both freedom and welfare could be increased by a fuller utilization of the price mechanism. In some cases this could be achieved by freeing the price mechanism from governmental or private hindrances that have outlived any justifications they may once have had; in other areas, like education, by developing new institutions (for providing security of investment in human capital) that would permit the competitive price mechanism to begin to operate in them. Annoyance with Friedman's antigovernment complex should not be permitted to obscure the legitimacy and the persuasiveness of his plea for permitting private enterprise to compete freely with government enterprise in such activities as schooling or mail delivery, or his arguments against price supports, tariffs and import restrictions, output restrictions, rent and wage controls, and the ICC's perversion from combating railroad monopolies to enforcing general transportation monopolies. He provides a beautiful analysis of the American Medical Association's self-virtuous conspiracy against the American public. Particularly welcome is his treatment of the control of money, even though it suffers from his inability to credit governmental authority with the power to learn to avoid even "inexcusable" misjudgments, and his chapter on international trade and financial arrangements, where he deserves special praise for his unflinching courage in demonstrating how any compromise on the unpopular cause of flexible exchanges endangers all the popular aspects of free trade and sentimental internationalism.

Every liberal, "liberal," or simple decent fellow interested in public affairs can gain much by reading this book and even more by rereading any part that may repel him. The antigovernment extravagances that I have perhaps overemphasized in this review are to be deplored less for the (to my mind) bad advice to which they lead than for their inhibitory effect on potential readers of an important book that will provoke much thinking and rethinking.

ABBA P. LERNER

Michigan State University

Man, Economy, and State: A Treatise on Economic Principles. Two vols. By MURRAY N. ROTHBARD. Princeton: D. Van Nostrand Co., 1962. Pp. xviii, 462; ix, 525. Set, $20.00.

The author, appalled by the lack of "treatises" on economics since World War I, has set out to rectify the deficiency, and has at the same time "felt it necessary to include at pertinent points, refutation of some of the main opposing doctrines," a course of action deemed necessary "because economic fallacy prevails far more widely than in Taussig's time."

The two volumes are cast entirely within the framework of the Austrian School, particularly the contributions of Bohm-Bawerk, and, in addition, rely upon "praxeology" as developed by Ludwig von Mises. In fact, Dr. Rothbard really appears to be infatuated with the sound of the word. He uses it at least fourteen times in the four pages making up Appendix A to Chapter 1 and an uncounted number of other times in the remaining pages of the text.

While we are told on the dust jacket that the author has written "a comprehensive and systematic exposition of the principles and doctrines of economics" and that the volumes are addressed to the "intelligent, educated layman, as well as to the economist," nowhere is that intelligent, educated layman told that this is only one system of economic principles and paricularly that it is a system that has been found to be deficient. This reviewer seriously doubts that "an unhampered market" of the sort envisioned by Rothbard has ever existed, or that it would work in the manner he describes even if it did exist, or that society would particularly like it if it should come into existence.

Rothbard is very impatient with the breakdown of "verbal logic" in economic theory and its replacement by mathematics "seemingly more precise and basking in the reflected glory of the physical sciences." Thus, the only mathematics he employs are geometric diagrams or algebraic formulas, and the latter only to exhibit economic doctrines reached by other paths.

The reviewer found the most interesting reading in the two volumes to be in the voluminous footnotes conveniently grouped at the end of each volume. These footnotes, which often take a petulent tone, convey better than the text itself Rothbard's conviction that intervention by the State is the root of all evil in economic life. Hence, he attacks monopoly grants, government enterprises of all kinds, the granting of privileges to labor unions, and above all the welfare state. Anything remotely suggesting state coercion he regards as the Nemesis of both productivity and freedom. In other words, the footnotes, and the things to which they refer in the text, become a convenient compendium of the viewpoint of a militant conservative concerning the contributions of any economist since the last quarter of the 19th century who has dared to disagree with the Austrian viewpoint. Singled out for particular attention are of course the writings of such wrongdoers as Marshall, Galbraith, and above all Keynes.

VICTOR C. HECK

Mercer University

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Groei en Inkomensverdeling. Capita Selecta der Economie, XXIV. By TH. C. M. J. VAN DE KLUNDERT. Leiden: Stenfert Kroese, 1962. Pp. viii, 144.

Dr. Van de Klundert tags one type of growth-and-distribution theory with the names of Ricardo and Kaldor: the different savings propensities of workers and capitalists determine an income distribution consistent with equality of saving and investment. A second type, typified by Meade's work, focuses on scarcity and marginal productivity. Avowedly combining both approaches and building on Solow's contributions, Van de Klundert questions any supposed long-run constancy of labor's income share. To prepare for his analysis, he reviews several proposed ways to classify inventions and rejects them all in favor of his own. He calls an invention labor-saving or capital-saving according to which factor requirement for a given output it reduces by the larger percentage, apart from any substitution between factors induced by the implied change in their prices per efficiency-unit. (He himself confesses some doubt whether this is an operational definition.) Labor-saving inventions together with low substitutability between factors could conceivably lead to a Marxian situation; but on plausible assumptions, labor is likely to gain. Capital-saving inventions are found to pose special difficulties for the analysis, if not for the economic system.

Moving beyond his preliminary assumption of one homogeneous final output, Van de Klundert considers the influence of how demand patterns evolve as incomes rise. In contrast with the industrial sector (and recent U.S. experience), agriculture in one model is highly labor-intensive and is handicapped by slow technological progress. The necessary relative rise in its prices could make it shrink even absolutely, unless the substitution response of consumer demand were outweighed by the low but positive income elasticity. Anyway, agriculture's shrinkage relative to low labor-intensive industry keeps wages from rising as fast as over-all capital accumulation would suggest. Another model contrasts industry with a services sector, characterized by high labor intensity and slow technical progress but facing a high income elasticity of demand. With labor less than infinitely responsive to wage differentials, higher wages in the services sector might be necessary to attract labor. The model provides one counterexample to discredit the "half-and-half rule," popular in the Netherlands, of tying wage increases to a compromise between each sector's own and the nationwide average productivity growth trends. Imposing a full or partial sector-by-sector correspondence between gains in productivity and in wages would interfere with consumer sovereignty, hamper pricing conducive to expansion of the technologically most progressive sectors in particular, and retard the rise in real national product. Sweeping conclusions like this one are not the main purpose of the models, however; Van de Klundert offers them mainly as examples of analysis.

A two-country, two-factor, two-commodity model makes familiar Heckscher-Ohlin propositions dynamic: free trade makes the differentials in *rates of rise* of incomes more favorable to labor in the abundant-labor agricultural country and more favorable to capital in the capital-rich industrial country than the differentials would have been under autarky. This rather superficial sort of dynamics runs throughout the book: instead of quantities demanded depending on prices and incomes, *rates of change* of quantities depend on *rates of change* of prices and incomes. The mathematics does not go beyond simple calculus, but symbols and entries in numerical examples do proliferate.

What happens to income distribution usually depends on the particular assumptions of the models; few noteworthy generalizations emerge. (Perhaps this indefiniteness corresponds to reality and deserves no criticism.) Whether Van de Klundert's self-consciously dynamic and abstractly formal approach is suggestive enough to be worth while in its own right probably depends on each reader's own tastes and imaginativeness. Some readers will at least welcome the book's bibliography of some 57 items in English, 36 in Dutch, and 6 in German.

Symbolic and numerical rigor vanishes in the last chapter, just when brief pronouncements on live issues—on conflict between full employment and price-level stability, on the asserted primacy of the latter, on imported inflation, and on wage-setting in accordance with the relative scarcities of workers and jobs—promise to become interesting. Van de Klundert proposes that impartial experts give guidance from time to time about the economically justifiable sizes of wage increases in various sectors, but he begs off from looking into administrative details.

LELAND B. YEAGER

University of Virginia

Wealth and Power in America—An Analysis of Social Class and Income Distribution. By GABRIEL KOLKO. New York: Frederick A. Praeger, 1962. Pp. xii, 187. $4.85.

This interesting short book by a Harvard historian is a refutation of those theories which herald a redistribution of wealth and a decline of poverty in the United States. In his opinion the U.S. economy has largely failed to attain those equalitarian economic goals that have been a part of our democratic heritage since Jefferson and Jackson. Most recent studies of this country assume that since the end of the depression the nation's wealth has been redistributed and prosperity has been extended to the vast majority of the population, that our society has attained a substantial measure of social and economic democracy, that poverty has been eliminated, and that the life of abundance has been created for many. The author argues that this assumption is fallacious, for despite the obvious increase in prosperity since the depression, the basic distribution of income and wealth in the United States is essentially the same now as it was in 1939 or even in 1910. Even though low-income groups live substantially better today, nevertheless, well over one-third of the nation's households subsist on incomes too meager to provide minimum standards of health and decency. Thus the book is really an answer to Galbraith's *The Affluent Society* and similar studies.

The book attempts to examine the factual data regarding the extent and causes of poverty and low incomes, to predict whether poverty will rise or decline in the near future, and to ascertain whether a high level of consumption and prosperity is possible for all classes even within the economy's framework of unequal incomes. It further attempts to assess the dominant social theory of our time and to discover the outlines of the actual U.S. class structure that emerges from the inequalities of income, wealth, and economic power. When political democracy is challenged by a totalitarain way of life that holds out the promise of economic equality, it is of crucial importance to evaluate the structure of the U.S. economy and decide whether the equalitarian goal of Jefferson has been achieved. Thus the author raises the questions: "Is America an affluent society?"; "Has a middle-class way of life been achieved for all the people?"; and "Is this a People's Capitalism?" To most of these questions his answers are in the negative.

The analysis is basically from a sociological rather than an economic point of view, which probably reflects the author's basic training in sociology and history, including economic history, rather than in economics proper with a theory orientation. Nevertheless, many problems of poverty and low income are sociological in nature, yet economic in consequence. Accordingly the book should be of interest to all social scientists.

In terms of available data the author concludes that since 1910 in this country there has been no significant trend in income distribution. The upper 10 per cent of the households (including families and unattached individuals) received more of the total national income than did the lower 50 per cent during the 1950's just as they did in 1910, with only a very modest drop in percentage of total income received by the upper 10 per cent during the half century. The drop in percentage, however, is eliminated if consideration is given to unreported incomes by the upper 10 per cent. In terms of subgroups, in the period 1910-59 the share of the upper 10 per cent dropped slightly, the shares of the second through the seventh 10 per cent rose, and the shares of the eighth through the tenth declined. The lowest 50 per cent received 23 per cent of total reported income in 1959 as compared with 27 per cent in 1910.

While real income has risen, it is concluded that since 1947, 50 per cent of the households have an income less than that of a maintenance (health and decency) standard of living ($3300 in 1947 and $4500 in 1957), and one-third have less than an emergency standard of living (70 per cent of the maintenance standard). The figure would be much higher for an abundant life standard of living, which would require provision for education and security—the latter, it is believed, is a basic economic fact of life for the majority of the population. The conclusions appear to be warranted from the data presented although the data seem somewhat meager and somewhat dated.

The traditional causes of poverty are held to be low wages and unemployment, along with depressed areas, declining industries, and minority groups. The new causes are low-wage occupations, problems of the aged, the increasing number of households headed by women, youthful marriages, etc. There is no mention of low productivity or a lack of bargaining power as factors in low income. The only reference to productivity in the book is in connection with the problem of automation and full employment. Yet rising productivity obviously is an important factor in any long-run solution to poverty.

The solution offered is largely a political one. The problem is judged to be one of economic organization rather than a lack of economic capacity, which will require sweeping political decisions regarding the economy if unemployment and poverty are to be solved. Some of the suggestions, such as "The low economic status of the Negroes . . . could be radically changed in short order if the 2,000 or so men who control the major American corporations really desired such changes" (p. 109), appear to be extremely trite and unrealistic. However, the book as a whole is very interesting, informative, and challenging.

Ervin K. Zingler

University of Houston

The Progress of Economics—A History of Economic Thought. By Warren B. Catlin. New York: Bookman Associates, 1962. Pp. 788.

In his *History of Economic Analysis,* Joseph Schumpeter made a distinction between histories of economic thought and histories of economic analysis. If this distinction is accepted, and there seems to be no important reason why it shouldn't be, Professor Catlin's book must be considered a history of economic thought. The subject is not limited to tracing the development of the economist's "kit of tools," but is broader in scope and encompasses ideas and opinions on many aspects of economics. Furthermore, these ideas and opinions are not just those expressed in the writings of the familiar giants, but include writings and utterances of lesser known and obscure economists, presidents and prime ministers, pamphleteers and public relations men, and even thoughts found in congressional reports and business records. Not since Schumpeter's own effort has such a conglomeration of names and ideas been presented to the student of economics.

For those readers who prefer to retrace the path over which economics has progressed and to pause in respect at the great landmarks of Smith, Ricardo, Mill, and Marx, etc., this book will be a disappointment. The author prefers the topical rather than the chronological author-school approach and, consequently, the landmarks are value, rent, capital, and other important categories of economics. However, this method is adequately justified in an early chapter and no criticism is called for. This approach is, after all, one of preference and not one of right or wrong. Yet, even with the topical approach, the great names usually stand out above the rest, but so many men and ideas are introduced into the discussion and, since the author occasionally lapses into playing the game of priorities, as economists often do in this field, the new student in economic thought may miss the forest for the trees. Some chapters are excellent; for example, the two on wages where the various wage theories and the men associated with them are clearly delineated. Others, however, such as the chapter on interest, are somewhat confusing, and it is difficult to sort out the important strands. Even Catlin seems to agree, for, after picking and threading his way through the multitude of interest theories, he ends the chapter by saying, "Perhaps, except for our love of wandering and historical excursions, we might just as well have confined our study to Alfred Marshall's *Principles*" (p. 430).

Actually, one might detect three distinct books in this one volume. The first, dealing with historical and environmental background, biographical influences, religion, style, and other related elements which contribute to the economist's over-all philosophy, might be considered a primer on how to write a history of economic thought, a guide to what is significant as background. It is in this section that Catlin is most enjoyable; the text is well spiced with colorful and humorous examples, although, presumably owing to the lack of space, they are restricted to the important names of the nineteenth century with the exception of Keynes and an occasional nod to Fisher, Mitchell, and other Americans. The second part of this volume could be considered a history of economic thought by itself; chapters on nearly all of the salient topics of economics are included—population, international trade, consumption, etc. And, if the last part of the book is included, the history of thought becomes even more complete, but top-heavy with money, credit, and cycles. However, this part should be considered separately, for the author's treatment of these topics is much more detailed and comprehensive. In six chapters he offers excellent historical background on money and monetary standards, central banks and financial institutions, including the bitter controversies that accompanied their development, and a detailed discussion on the causes and remedies of business fluctuations from sunspots to modern investment theories. His conclusion favors the human element and, as far as causes of the business cycle are concerned, ". . . whatever other theories may be advanced, they are mainly secondary, and that the fundamental explanation is essentially psychological" (p. 609).

For the most part, the author restricts himself to the role of narrator for this story of progress, a role justly deserved after years of experience in teaching and writing,. but on occasions he steps out of this part and offers valuable insights and suggestions which make his book much more than mere history. And nowhere is he better than in the final few chapters where he really lets himself go. Yet, despite some harsh words about Keynes, Keynesians, fiscal policy, and doctrines which have made us ". . . perhaps more ready to adopt half-baked proposals" (p. 603), Catlin is no old-generation reactionary pleading for a return to the good old days. He does not cringe at the word "planning" and even offers suggestions along this line.

For him, the problem is not one of more or less government intervention in economic affairs, but one of better government. On a specific point, one of his suggestions for avoiding the wastes of duplication of plants is looking at the possibility of broadening the application of our franchise and license laws. On an international level, he believes some sort of world organization will be necessary; an organization strong enough to maintain peace and to force law and order. Strange suggestions, perhaps, for a history of economic thought, but, nevertheless, valuable.

The book is a really good history of economic thought. It is extremely well written and well annotated. The author displays much more than just a passing acquaintance with the literature of the field, and the patient research is obvious. There is no skimming the surface or touching down only on the high peaks, and there is no attempt to present the ideas of past economists in the sartorial splendor of econometric or graphical analyses. Some selectivity was necessary and readily admitted by the author; but this is excusable; actually, the history of economic thought may have arrived at that stage where it is no longer possible to cover it completely in one volume. Nevertheless, Catlin has done an admirable job.

Some instructors may avoid this work because of its topical approach and others because it may lack detail in the development of analytical tools, but these are only part of the total potential uses for this book. It can be utilized as a text in the history of doctrine, especially in one-semester courses where it would seem advisable to avoid the repetition usually associated with the author-school approach to the subject. Furthermore, because of the topical approach, the book can be employed with some profit as background reading in many other courses, particularly in money and banking and business cycle courses. In any event, every college and university library can well afford to put a copy on its shelves.

EDWARD J. POWERS

St. Anselm's College

The Keynesian System. Millar Lectures No. 4. By DAVID MCC. WRIGHT. New York: Fordham University Press, 1962. Pp. xi, 90. $3.00.

The first three Millar lectures were given by Arthur F. Burns, Edward S. Mason, and Milton Friedman respectively. Now comes David McC. Wright with what must be described as a slim book on "what Keynes' *General Theory* really means to me." The point of view expressed is that Keynes was more sensible than his disciples or enemies believed. The aim of the book is to provide an introduction to the *General Theory* for a lay audience. The surprising thing about the book is its date of publication.

JOHN A. BUTTRICK

University of Minnesota

Price Theory: A Provisional Text. By MILTON FRIEDMAN. Chicago: Aldine Publishing Company, 1962. Pp. 285. $6.00.

In the preface to this provisional text, the author expresses his own reluctance to have published these revised summaries of his lectures in his famous two-quarter price theory course at the University of Chicago. Acknowledging the "scrappy nature" of these notes, Friedman bows to the apparent demand for the mimeographed version (hitherto obtainable at Chicago) which "it has not been feasible to keep . . . from getting fairly wide circulation." Apparently despairing of being able to prepare "a fuller and more satisfacory treatment of price theory," he deems it "best to make . . . [the notes] . . . generally available" (p. 3).

Friedman lists as the "chief gaps" in the notes "which it is necessary to supplement by class presentation: (1) the theory of the division of income between current consumption and the accumulation of wealth; (2) industrial organization, with special reference to problems in the economics of the individual firm; (3) fact and theory about the size distribution of income; (4) the theory of profits; (5) capital theory—the final section of the notes on this topic have turned out to be too succinct and condensed, particularly with respect to the arithmetic of the relation between income streams and capital values and the stock-flow analysis embedded in that section; and (6) the theory of general equilibrium."

To the 258 pages of text (44 pages of which are reprints of some of Friedman's previously published work), Friedman has added ". . . two appendixes which may help to fill these gaps as well as to supplement the notes" (p. 4). The first gives the reading list that he has used in his course; the second presents a collection of problems assigned to the class. Thus this "Provisional Text" is designed as a supplementary treatment, as is appropriate for lectures: this volume is a "text" provided that one is already "familiar with material equivalent to that contained in George Stigler, *Theory of Price*, or Kenneth Boulding, *Economic Analysis*" (p. 264). This explains the otherwise curious unevenness of both depth and emphasis in the notes. The book is neither complete enough nor rigorous enough to be useful as a self-contained text, but it is certainly provocative enough to be a very useful supplement to other texts.

The book is essentially nonmathematical; the exposition relies heavily upon simplified numerical examples. Since general principles are presumed to be otherwise acquired by the student, the elaboration of these examples is perhaps not misplaced. There is much to be said for Friedman's view that "We shall . . . gain insight if we proceed more slowly and less formally to this final result" (p. 176). Certainly, with respect to the points Friedman is most concerned to make, he sharpens the rigor of familiar nonmathematical treatments. Also in these respects Friedman gives good expositions of the institutional foundations of some of the relationships he is discussing, particularly with respect to the supply of factors of production. Finally, a conscientious effort is made to be explicit about the questions which the analysis begs at certain crucial points, particularly with respect to the definition of a firm. When all of this is said, however, I must confess to a residual feeling of dissatisfaction with such nonmathematical expositions—the result of many pages of verbal analysis is too often a maze of intersecting geometry illustrating a very simple (albeit important) point.

The tremendous complexity of the determinants of economic behavior, which partly explains the increasing use of more powerful mathematical tools of analysis, places a special burden on the writer of a nonmathematical theory text. The analytic contribution of the economist, as opposed to the mathematician, lies in the choice of the function used to represent the behavior of a decision unit, and the specification of sensible auxiliary restrictions on the variables that are relevant to the substantive problem being analyzed. For any given function, variations in the auxiliary restrictions can be utilized to distinguish among short, intermediate, and long runs, between partial and general analyses, and between static and dynamic analyses. The expositor who abjures the use of mathematics attempts to capture the force of such variations in auxiliary restrictions by considering a few cases, in each of which a different set of variables is not allowed to vary at all. When ringing these variations on which arguments of the function are to be impounded in *ceteris paribus,* Friedman gives a much more thorough and rigorous treatment than is customary. This is true for all major topics covered. For this reason, these notes are especially valuable in supplementing nonmathematical texts. Perhaps this is Friedman's single most important *analytical* contribution in these notes, and it is a very worth-while contribution indeed.

Even though Friedman's exposition is the best nonmathematical treatment I have seen, there is one respect in which his exposition leaves something to be desired. Having derived several different curves to represent a bivariate relation under different sets of "given conditions," he follows an asymmetric practice in labelling the curves and this confuses the basic point he is making. For example, Figure 74 (p. 180) presents three different curves relating the quantity of a factor that will be hired by a firm per unit of time to the price of that factor; all three of the curves represent the demand function for the factor by the firm, under three different specifications of what is being held constant; only one of the curves is labelled a demand curve for the factor. Friedman is trying in this way to emphasize that the curve given the label "demand curve" is derived under a less restrictive set of auxiliary conditions than are the other two curves, but the question of which set of conditions is appropriate to a given problem cannot be answered by simply noting which set of conditions is least restrictive. Friedman's own strong statement to this effect when considering the theory of consumer demand (in Chapter 2) should be kept in mind when reading the later chapters.

Friedman's most significant and provocative substantive departure from standard texts is his concept of "entrepreneurial capacity." This concept plays an important role in his treatments of the firm and of distribution, in spite of his commendably frankly acknowledged failure to define the concept in an empirically meaningful way (pp. 95-96). It will be a challenge to graduate students and their professors alike to try to decide whether Friedman is pointing in this way to a real area of ignorance, or whether he is merely sweeping under a rug of supposed ignorance the fragments of economic theory which he chooses to ignore. Without meeting this challenge, and without critically examining the discussions of rent and profits which are related to the concept of entrepreneurial capacity, readers will have a difficult time with Friedman's discussion of distribution should they find the latter unpalatable.

A few additional cautions may be offered to prospective users of this book: Friedman ignores "second order" or sufficiency conditions of maxima; there is some ambiguity in his treatment of the notions of "utility" and a "utility function" (pp. 38, 73); and the brief discussion of the "all-or-nothing demand curve" appears to be simply incorrect (p. 15).

Many of the problems presented in Appendix B are quite thought-provoking. They are especially valuable in focusing attention on policy applications of the analysis, and they make a real contribution to the usefulness of the book.

Despite its shortcomings, this collection and elaboration of some of Friedman's ideas is a very valuable supplement to standard graduate theory texts. I believe that the state of economic thought would be advanced by a wide reading of these notes, both by graduate students and by their professors.

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Economic History; Economic Development; National Economies

Central Planning in Poland. Yale Studies in Economics, No. 13. By JOHN MICHAEL MONTIAS. New Haven and London: Yale University Press, 1962. Pp. xv, 410. $7.50.

This important book should interest economic theorists as well as students of Eastern Europe. This reviewer knows of no book on Soviet-type economies which has so much to offer economists at large. Early theories of planning, such as those of Barone, Lange, and Lerner, disregarded the utility functions of the planners. The literature on materials-balancing (the Soviet theory) is mainly bureaucratic in tone, but where it is not, it amounts to a partial equilibrium input-output analysis. The macroeconomic planning theory, misnamed "Keynesian," is nonoperational, in the sense that it provides no guides to the management of nationalized enterprise. Theorists have simply not understood the planned economy; they still do not; but Montias' book is a major step toward the theory that is needed. In addition, it is a careful description of facts and interpretation of a complicated set of real problems.

An interesting problem in the theory of planning is this: suppose planners have a desired "bill of goods" and some knowledge of the relevant input-output matrix. Can they then obtain by successive approximation a set of output goals which do not exceed the capacity of any industry or upset the balance of trade, and which are efficient in the sense that no one output can be increased without decreasing others? Montias discusses this problem in the light of input-output literature and in terms of his own experience with Polish data. It is a rewarding discussion. In addition, he discusses the "transpose" of this problem: given a set of desired national accounts, and an input-output matrix, find a suitable price structure. The input-output literature, as well as practical planners, have tended to slight this problem, but Montias does not.

Do prices and money matter in an economy when the targets of planners and the rewards of management are based on outputs rather than on values or prices? A decade ago, institutionalism dominated both Soviet-bloc and U.S. literature. Even now, it is often questioned whether a microeconomics is needed to understand the planned economy. Montias has given convincing evidence that it is. Indeed, while money is used, operating plants operate on information which includes prices and money as well as production goods. This information frequently is a garbling of the planners' intentions, and therefore leads to more or less undesired events. Montias' careful spelling out of how precisely prices and money may be fixed in such a way as to disturb planned economies is a most useful demonstration of how prices matter, even when there is no "price mechanism."

The planned economy, then, is not characterized solely by a plan and by the statement that enterprises are state property. Economic scarcity does persist and dictates the response of enterprises to the entire set of information available to them. The planners face a choice between a flexible but systematic price policy (which has been repugnant to Stalinist economists) and the use of force to compel the execution of production goals. The last decade has brought out in the open a questioning by Soviet-bloc economists of the earlier price doctrines. One aspect of this controversy, the role of "the interest rate" in Soviet investment decisions, has been discussed in U.S. journals. In Poland, other equally basic questioning took place in the late 1950's. Montias has explored most interestingly the facts which led to Polish dissatisfaction with the rigid pricing of the period before 1956 and has summarized admirably the points of view which came to light during this period of relatively open discussion. Anyone who is interested in the process by which governmental theories are challenged by the inexorable flow of events will appreciate Montias' analysis. Rigid prices were not under attack in Poland on grounds of principle, but because it seemed to be unnecessarily hard and unpleasant to maintain them.

It is also a matter of interest that the execution of Polish plans has depended to an important degree upon fiscal and monetary policy. These have been neglected, both in Soviet-bloc and U.S. discussion of planning. Yet it seems clear that the state budget and the banking sector, which one would expect to be the most easily controlled part of the economy, have in fact been difficult to control. The budget surplus has been hard to predict. Credit and the money supply have tended to expand more than was consistent with the framework of the plans. In the very areas where one would expect greatest control, there has apparently been the least. Is this attributable to a lack of concern by the state or to the interaction between the state and its enterprises? The answer to this question is not clear, partly because data are not very complete. Montias does not have an exhaustive treatment of the problem because his interest lies mainly in the "real" sector of the economy. However, he does not slight the issue, and one can hope he will eventually discuss it directly.

Our understanding of the Soviet-type economy has greatly increased over the past decade. The reader who wishes to obtain from a single source the best statement to date of how such a system works, and why it works as it does, should read this book. Poland is not the U.S.S.R., but in many respects it operates the same way, and Montias has given an excellent interpretation of the system.

EDWARD AMES

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Mathematical Models of Economic Growth. By J. TINBERGEN AND H. C. BOS. New York: McGraw-Hill Book Co., 1962. Pp. viii, 131. $6.90.

This book's somewhat misleading title should have been expanded to emphasize the authors' preoccupation with development planning. As the most recent addition to the Economic Handbook Series, the book is essentially a text, but it is not a volume to which one can refer for a general description of mathematical growth theory. The models of the title, over thirty basic ones plus many variations, were "... selected according to the following criteria: (a) practical applicability in planning for underdeveloped countries; (b) illustrative of some important problems around development planning."

The growth paths of income, consumption and/or the capital stock are found for various specifications of an economy's structure. The growth paths are then examined from the point of view of Professor Tinbergen's well-known instruments and targets approach. The models proceed from the simple barebones Harrod-Domar case of one scarce factor and one sector to a complicated generalization involving many factors, sectors, regions, and prices. Gestation lags, depreciation, population and technological change, and accounting prices all put in an appearance, albeit briefly in some cases. The models address themselves in varying degrees to open and closed economies, interindustry and interregional analysis, and factor and product substitution. The mathematical demands on the reader extend only a little beyond the calculus to elementary differential and difference equations, but the presentations are so terse that even well-prepared graduate students will probably find the going slow. A helpful companion-piece would be the 1960 report of the United Nations Economic Commission for Asia and the Far East, entitled *Programming Techniques for Economic Development,* which was authored by, among others, Tinbergen.

As a text designed essentially for "civil servants in charge of development planning as well as ... future planners during their university education," *Mathematical Models of Economic Growth* breaks no new ground. In the preface, the authors indicate that they consider their main contribution to be their choice of models for exposition. Seeing what they consider important is interesting; but more illuminating is seeing what they think unimportant.

Investment, of course, is crucial to development and so it plays a key role in every model. Labor supply for the most part is assumed to be given exogenously, but with provision for a growing labor force in some models. Constant capital-output ratios are ubiquitous, either for the economy as a whole in one-sector models, or for each sector in interindustry models. The possibility of substitution between labor and capital by means of the Cobb-Douglas production function is treated in early one-sector models. In multisector models, factor substitution is discussed as a problem of choice of investment composition. In this case, capital-output ratios are taken as fixed, but different, within sectors; thus prices do not enter into the planning problem. Interindustry analysis is almost solely in input-output terms. Surprisingly, activity analysis gets virtually no attention. In the single model involving linear programming, attributed to Sandee, the presentation centers exclusively on the characterization of an underdeveloped country's structure and stops short of any discussion at all of the maximization problem. The curious omission of serious discussion of activity analysis apparently is a consequence of the authors' distaste for the assumption of a linear maximand. It is not clear why they neglect entirely the possibilities of quadratic and nonlinear programming. Though they are interested in long-range planning as well as short, they exhibit no interest in turnpike theorems and their implications for development planning. However, they do discuss in some detail the problem of adapting an economy's capital stock to the requirements of balanced growth when the composition of the initial stock is not right.

Despite its disarming introduction, the final summing-up chapter will be regarded as churlish by many. "Some Critical and Concluding Remarks" contains critical remarks indeed about some present planning practices: linear programming; overly simple assumptions about the behavior of consumption and about gestation periods; unsatisfactory hypothesizing about the source of investment funds; and more. Most interesting is the admission, or contention, that "There is too little awareness of the very unsatisfactory state of our insight into the main problem: the problem of the optimum rate of development." The authors had supported this statement in an earlier chapter by describing their attempt to devise a model which would permit solving for the optimum rate. After seven pages of somewhat intricate analysis (involving Frisch's attempts to measure marginal utility, no less) they were discouraged to find that optimization called for savings rates over one-half. Thus, they decided that economic science does not seem capable of indicating an optimum rate of development. Many economists would more readily accept this conclusion than the underlying reasoning.

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Economic Backwardness in Historical Perspective: A Book of Essays. By ALEXANDER GERSCHENKRON. Cambridge: Belknap Press of Harvard University Press, 1962. Pp. 456. $8.75.

These fourteen essays, three of which appear for the first time in English, are the product of a cultured and fertile mind, versatile enough to speculate intelligently on economic, historical, and literary problems. The essays, though they range over a broad gamut of topics, are built around two foci: the first eight are concerned with the lessons of economic history for developing countries and the last six with Soviet growth, economic organization, and literature.

Among the essays on economic development in historical perspective, three contain Professor Gerschenkron's observations and theoretical speculations on the character of the development process in backward economies on the way to progress. The other essays in this section apply or test these hypotheses in the light of the historical performance of Italy, Bulgaria, and Russia in the last century.

Gerschenkron's theory is woven out of these principal threads:

1. Relative backwardness engenders "tension" between a nation's present and potential state of development. This tension is revealed to the dominant elite and reinforced by invidious comparisons with other nations that have forged ahead.

2. The store of foreign technology that may be borrowed by the lagging country offers an opportunity—or "advantage"—that will allow the latecomer to grow fast, perhaps faster than its predecessors, and avoid some of the misery the latter went through when they started *ab ovo*. Developing countries will tend to concentrate on branches of production where they can adapt the most advanced technology to their resource endowment. Since they are especially short of skilled labor, it may pay them to introduce capital-intensive automatic equipment for key processes; in general, they will devote a greater effort to the expansion of heavy industry than their forerunners did at a comparable stage.

3. Once tension has built up and the leaders of a country are bent on catching up, the absence of traditional "prerequisites," such as W. W. Rostow emphasizes, does not represent an insuperable obstacle to development. To start an industrialization drive, one need not necessarily have an agricultural sector capable and willing of delivering the food supplies necessary for urbanization; nor is there likely to be an adequate supply of honest entrepreneurs, an able civil service, or adventurous and forward-looking banks. If they are not already present, these prerequisites will often be generated in the course of development. The important thing is to bolster institutions capable of taking the place of the missing ingredients until they are ready to make their appearance. In Russia, for example, the state supplanted the banks in promoting industry until about 1900 when standards of honesty and enterprise had so matured that the banks could take over at least a part of the state's responsibility for financing the national economy. The lack of domestic capital can be overcome by capital imports, once a favorable climate has been created, as was the case after Russia and Japan adopted the gold standard in the late '90's. The absence of local skills can be made good by a hospitable immigration policy. In the Soviet Union industrialization and urbanization were rendered possible despite the stagnation of agricultural production by what Alexander Erlich calls the "institutional landslide of collectivization," thanks to which a much larger share of the total farm produce was exacted from the peasants and made available to feed the city population than would have been marketed under normal conditions. The author deplores the heavy human cost of this type of substitution, but he expresses the hope that developing countries will find methods more susceptible of preserving human welfare and happiness (p. 51).

Gerschenkron's ideas are highly suggestive and they certainly help "address relevant questions to the facts." But is his model capable of predicting the nature of the development process in a country the author did not study before formulating his hypotheses? Two limitations, which Gerschenkron implicitly or explicitly recognizes, reduce the model's predictive value. First, there may be a poor response to the challenge of backwardness; opportunities for developing substitute institutions to take the place of missing prerequisites may not be perceived or grasped in time to be successful. Second, comparative advantage in international trade may predetermine the growth pattern, technological considerations notwithstanding. Gerschenkron's studies of Italy and Bulgaria are relevant to the first point, and an independent study—or "test"—of Gerschenkron's model by H. Rosovsky in his book on *Capital Formation in Japan* bears on the second. It was not the lack of "tension" but the failure of the governments of these countries to discharge their responsibilities as leading promoters of industrialization that hindered development. In Italy, before World War I, the wrong industries were protected; in Bulgaria tariffs and subsidies were too low to spur a rapid industrial expansion. In Japan, according to Rosovsky, comparative advantage in textiles swamped such predilections as Japanese entrepreneurs might have had for developing the branches of heavy industry where the newest technological advances had been made in the West.

Gerschenkron's criticism of the Bulgarian government for not pursuing a sufficiently vigorous industrialization policy is somewhat one-sided. It is true, as he points out, that the country's agriculture was technologically backward, but this handicap did not rule out a comparative advantage in this sector. Already strong at the turn of the century, this advantage tended to deepen as farming techniques improved. From 1890 to 1912, Bulgaria's exports, consisting primarily of foodstuffs, were growing quite rapidly—at an average of 3 to 4 per cent per year. Imports of farm machinery, facilitated and to some extent subsidized by the state, were beginning to modernize the nation's agriculture. After World War I the nation was too exhausted to sustain a major industrialization drive even if such a policy had been ideally desirable; the budget was already so tied up in the expenses of reconstructing war damage, in the resettlement of Bulgarian refugees, and in reparation payments that it could not devote substantial funds to industry, even if a favorable political situation for such a policy had prevailed. During the depression of the 1930's, the contraction of exports reduced the availability of foreign exchange to finance minimum needs, let alone to import the machinery and equipment necessary for industrial expansion. The Draconian austerity measures that might have succeeded in mobilizing the resources for industrialization might also have touched off a peasant revolt. (Gerschenkron lays a part of the blame for the Russian revolutions of 1905 and 1917 on the Vyshnegradski-Witte industrialization policies carried out at the expense of the peasantry, yet he appears willing to countenance similar—if perhaps milder—policies for Bulgaria, whose natural endowment was much less favorable to the growth of industry than Russia's.)

There are some remarkable parallels and divergences between Gerschenkron's book and Tugan-Baranovsky's collection of essays [*K lushchemu* *budushchemu* (For a Better Future)] published in St. Petersburg in 1912. Both authors are fascinated by Kantian ethics and by Marxian economics. The excellent essay on Pasternak in *Economic Backwardness in Historical Perspective* echoes Tugan-Baranovsky's essay on Dostoevski: it is the vibrant belief of these great writers in the "normative world of individual freedom" that attracts their critics. Tugan-Baranovsky's reflections on "The Russian Intelligentsia and Socialism" center on much the same personalities and problems as Gerschenkron's "Economic Development in Russian Intellectual History of the Nineteenth Century." Both of them trace the historical roots and causes of the negative attitude of the intelligentsia toward the bourgeoisie and toward capitalistic development. They concur on most points, including the conclusion that by 1912 the intellectual popularity of socialism was on the wane; economic thought was "thawing" under the warming climate of successful economic development. (The period 1908-14 was marked by a resurgence of the rapid industrial growth of the 1890's, accompanied this time by gradual improvement in agricultural conditions.) Where the two economists diverge is in their appraisal of the role of the state in economic development. Tugan-Baranovsky argued that the "patriarchal" interference of the Tsarist government in Russian economic life was "neither accidental nor easily corrected but intimately bound up with our entire social system. There is too much Asia in us—and this cannot help but hamper our industrial development" (*K lushchemu budushchemu,* p. 212). Gerschenkron, as I have already pointed out, assigns a major role to the state in promoting economic development. Although he speculates that the government may eventually yield its baton to other institutions when they become ready to take up the race, he does not tell us much about the correct timing or the mode for making this switch. It is by no means clear whether he would agree with Tugan-Baranovsky that by 1912 the Russian state should already have ceased its meddling.

A book that is as original, provoking, and richly textured as this is of course likely to occasion some disagreement and controversy. It should encourage cohorts of scholars to check or test its hypotheses. But it is especially recommended as a book to read at leisure and enjoy.

JOHN MICHAEL MONTIAS

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British Economic Growth, 1688-1959: Trends and Structure. By PHYLLIS DEANE AND W. A. COLE. University of Cambridge, Department of Applied Economics, Monograph No. 8. New York and Cambridge: Cambridge University Press, 1962. Pp. xvi, 348. 60s., $11.50.

Approximately ten years ago the Committee on Economic Growth of the Social Science Research Council, under the guidance of Simon Kuznets, initiated a project to elaborate in quantitative terms the economic history of a number of industrialized nations over time periods as long as the underlying statistical data would support. This is the first complete country-study to be published. Preliminary and partial results on Britain and several other countries have appeared earlier, and publication of a multi-volume *Histoire quantitative de l'économie française* is currently in progress. For a number of reasons the British study is also likely to be the most important. For one, special importance attaches to the history of Britain, the first industrial nation. Moreover (and in part for the reason above), the time span covered is unusually long; Gregory King's precocious late-seventeenth-century estimates provide a felicitous starting point. Presentation of the final results in one compact volume will facilitate their critical study and utilization. (A companion *Abstract of British Historical Statistics*, compiled by B. R. Mitchell with the collaboration of Phyllis Deane, provides supporting statistical series not included in the volume under review.) Last, but by no means least, the meticulous care with which the authors have assembled, analyzed, and presented their evidence and conclusions insures that the volume will occupy a prominent position on the shelves of economic historians and other students of economic growth for many years to come.

Two approaches are possible to the historical estimation of national income and related aggregates: the refinement of contemporary estimates, and the construction of independent accounts from surviving records of trade, taxation, etc., most of which were collected for administrative reasons and conform only slightly and by accident, if at all, to the categories of interest to the national income statistician. In practice the approaches are normally combined, as in this study. Even with the combined approach, however, the difficulties in the way of precise and reliable estimates are enormous. The conceptual problems are formidable enough. For example, how meaningful is it to apply the concepts of modern, specialized economies to the relatively unspecialized economies of the preindustrial era? What significance should we attach to the quantitative statement that the "average" standard of living has increased eightfold over a period of two or two and a half centuries, when the qualitative content of that standard has changed almost beyond recognition? Once we have made up our minds to swallow these elephants, however, there remain those equally unpalatable gnats, the data problems. Contemporary observers, even when well informed (as King and Arthur Young, for example, surely were), not only operated with different concepts but rarely had access to the kinds of information which modern national income statisticians would consider essential before beginning. The independent information available to the authors is no more reliable and even less complete for any given year. For the entire first half of the period with which they deal they are forced to make more or less informed guesses even for such basic items as population and labor force. The improvement of official statistics in the late nineteenth and twentieth centuries, together with the existence of historical time series compiled by other modern scholars, alleviate some of the data problems, but few disappear altogether. One major problem, conceptual as well as mechanical, and affecting the entire period, is the difficulty of finding satisfactory price deflators to express money aggregates in real terms.

No one is more aware of the difficulties involved and the consequent necessity for caution in the interpretation of their findings than the authors themselves; and it is not the least of the merits of their book that they frequently emphasize the dubious reliability of the underlying data, pointing out areas of special weakness and giving lucid explanations of their methods of processing and derivation. In some cases it is necessary, for a complete explanation, to refer to one or another of the senior author's preliminary journal articles, all of which are plainly indicated. The authors maintain throughout the volume an attitude of commendable modesty with respect to the tentative nature of their findings. If they are more concerned to emphasize the weaknesses of the data than the conceptual problems, this is surely justified in view of the likely employment of their results by other, less cautious students and analysts.

The authors begin by sketching the long-term trends of population growth, the value of money, real wages, and Britain's international economic relations. They follow with an intensive analysis of the evidence of economic growth in the eighteenth century, a period doubly crucial in that it witnessed the beginnings of Britain's subsequent industrialization, but for which basic statistical data are unusually fragmentary and unreliable. The ingenious manner in which they process the data in order to extract the maximum of usable information is one of the highlights of the book. A separate chapter is devoted to the relationship between industrialization and population change in the eighteenth and early nineteenth centuries. Subsequent chapters deal with the industrial distribution of the labor force, the structure of output, the factor composition of national income, and trends in capital formation, all chiefly in the nineteenth and twentieth centuries. The authors also give detailed individual attention to major nineteenth-century industries—textiles, mining, iron and steel, and transport. In a final chapter they again review the long-term trends and summarize their conclusions.

Perhaps the most striking over-all conclusion to emerge from the entire study is the surprisingly low (by modern standards) rate of growth of the British economy throughout the period covered, but especially during that of the classical "industrial revolution" or currently fashionable "take off." Calculated on 30-year intervals to eliminate short-term variations, and with exceptions for the world wars of the twentieth century, the average annual growth rate of total real product varied from a minimum of 0.2 per cent to a maximum of 3.3 per cent; corresponding per capita rates are 0.0 per cent and 2.1 per cent. At no time during the eighteenth century, not even during the marked acceleration of the last two decades, did the over-all growth rate exceed 1.8 per cent, and the maximum may have been as low as 1.5 per cent, depending on the method of weighting employed in the estimates (pp. 80, 281). During the nineteenth century the over-all growth rate (also measured on 30-year overlapping intervals) varied from a low of 2.2 to a high of 3.3 per cent. Under the impact of twentieth-century catastrophes the rate slumped again (although it already showed signs of deceleration before World War I), and even with the renewal of growth since World War II has not regained the best levels of the nineteenth century. (One implication of this finding which is not explored by the authors—admittedly, it lies beyond the scope of their study—is the much higher growth rates that must have been posted by other nations in the nineteenth century which, beginning to industrialize

50 or 100 years after Britain, equalled or exceeded the twentieth-century levels of British per capita incomes.)

In spite of the relatively low rates of growth in the eighteenth century, the authors feel that their findings do lend support to the idea of an industrial revolution. Moreover, in addition to their mastery of the data problems, their analysis of the timing and causation of that phenomenon is truly outstanding. Although the most rapid increases in output occurred at the end of the century, after about 1780 or 1785, they think they detect the beginnings of growth much earlier, about 1745 or 1750, after nearly half a century of virtual stagnation.[1] They associate this early phase of growth with the rapid expansion of foreign trade, especially trade with the American colonies. The difficulties leading up to and including the American Revolution interrupted both trade and growth, but these quickly resumed at even higher rates after the disturbance. This raises the tantalizing possibility of a causal connection between, or at least joint causation of, the American Revolution and the industrial revolution, a notion which is strengthened by the findings of other recent and independent research.[2] More fundamentally, Deane and Cole make British economic growth in the eighteenth century more explicable in purely economic terms, with less dependence on the *deus ex machina* of "exogenous" factors. In particular, both population growth and technological advance are closely related, in their view, to effective demand and supply. This not only accords with the views of recent students of technology and demography, but is also more aesthetically satisfying to the scientist.

Among the least satisfactory aspects of the study are the estimates of national income in constant prices, and the corresponding "real" rates of growth. In order to have a price index covering the entire nineteenth century, the authors are forced to fall back on the Rousseaux index of wholesale commodity prices based on an average of the years 1865 and 1885. This has the effect, among others, of converting the undoubted economic expansion of the 1850's and 1860's into a secular trough, with a lower growth rate than the "Hungry Forties" which preceded or the "Great Depression" which followed. This may be the place to point out that the authors, in common with most economic statisticians, employ the term "qualitative evidence" in a pejorative sense, as though quality were something to be avoided. In this case they would have done better to stick to the qualitative evidence—or other, partial and fragmentary, quantitative data—in preference to giving misleading and clearly inaccurate long-term quantitative series. Using the Rousseaux index, they obtain a real per capita growth rate of 1.7 per cent per annum for the period 1861-71—1891-1901, but the Bowley index for 1860-69—1890-99 gives a rate of 2.1. The difference is significant. In view of the large amounts of skill, time, and money invested in this study and the great importance of its results, it is a pity that the authors did not find it worth-while to construct a new index more suited to their purposes.

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[1] A recent article by A. H. John, "Aspects of English Economic Growth in the First Half of the Eighteenth Century," *Economica,* May 1961, questions the usual assumption of "stagnation," but in other respects supports the analysis of Deane and Cole.

[2] See, for example, D. A. Farnie, "The Commercial Empire of the Atlantic, 1607-1783," and Ralph Davis, "English Foreign Trade, 1700-1774," both in *Economic History Review,* December 1962.

In other respects their treatment of the nineteenth and twentieth centuries, while not matching the *tour de force* of their analysis of the eighteenth century, yields much new and valuable information and many stimulating suggestions and insights. The spectacular decline in the importance of agriculture is clearly recorded, as are the other notable shifts in industrial structure and location, the composition of factor incomes, etc. A conclusion of fundamental importance, tentative though it is, is that net capital formation as a proportion of national income probably did not rise above 7 per cent during the Rostovian take-off period, 1783-1802, and did not reach 10 per cent until the railway booms of the 1830's and 1840's. This should occasion some rethinking by growth theorists about the relative importance of capital *vis à vis* other factors in economic development. If it does, that alone will justify the effort and resources which have gone into the project. But that is only one of many reasons why this volume will become a landmark in the history of economic history and the study of economic growth.

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The Development of the British Economy 1914-50. By S. POLLARD. New York: St. Martin's Press; London: Edward Arnold, 1962. Pp. ix, 422. $7.50.

In Britain the consumer of economic historiography is better served than in the United States. For a very long time there have been relatively more economic historians in Britain, a wider knowledge of economics among these historians, more primary research workers, a more secure place for economic history in undergraduate curricula, and a much wider general interest in economic and social development. There has also been less fad and fashion in British economic historiography, and possibly less coterie research. Hence the consumer—student, general economist, or common reader—enjoys a substantial choice between various narrative textbook surveys, good and bad. Pollard's text is a distinguished addition to this large family.

The book is organized on topical and chronological lines. Introductory chapters deal with Britain before 1914 and with World War I; then follow chapters on industry and transport between the wars, commercial and financial developments, and social conditions. The penultimate chapter covers World War II, and the last chapter treats the period of postwar reconstruction. There is an adequate index, and in the footnotes there is an up-to-date survey of the literature. It is a pity that the prevalence of *op. cit.* makes this survey awkward to use, and it is also unfortunate that Pollard has included no list of tables. But these are minor annoyances. U.S. economists and historians will find the book a reliable guide to the complexities of the period.

Pollard interprets Britain's interwar economic difficulties and her surprisingly tranquil passage through the storms of the thirties along currently orthodox lines. Thus the troubles of the twenties are traced to excess capacity in the old staple industries, to immobility of labor and capital in these industries, to the overvaluation of the pound after 1925, to foreign competition in the old staple trades, and to foreign protectionism. Her rapid recovery in the thirties is credited to the favorable movement in the terms of trade, to the domestic housing boom, and to the development of new industries. Devaluation, cheap money, fiscal policy, protectionism, and rationalization all receive little praise, though Pollard is inclined to think that by directing new demand to home industry the tariff probably did Britain more good than harm. Latter-day free traders should not ignore Pollard's discussion of the applicability of comparative-cost doctrine to interwar Britain. Disciples of Professor Rostow should check Rostow's generalizations against Pollard's evidence.

While reading Pollard I was constantly reminded of Clapham's multivolume study of the nineteenth-century British economy. Perhaps Pollard's text can best be typified as a latter-day sequel to Clapham. I suppose that those numerous readers who find Clapham otiose will find Pollard annoying. Certainly Pollard shares Clapham's chief virtues—readibility, accuracy, comprehensiveness, balance, and attention to detail. He also possesses the traits which in Clapham are often thought to be faults—a concentration on minutiae at the risk of concealing broader developments and an underplaying of statistics and economic analysis in the interest of smooth narrative. The reader cannot justly accuse Pollard of theoretical naïveté, nor will he find Pollard uninterested in problems of causation. But I am sorry that Pollard has not given more prominence to causal and theoretical problems—especially because nonspecialists are bound to use his book extensively.

IAN M. DRUMMOND

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Modern Economic Problems in Historical Perspective. By DOUGLAS F. DOWD. Boston: D. C. Heath and Co., 1962. Pp. viii, 198. $2.40.

This book is intended for use as supplementary reading in the principles of economics course. It provides historical and institutional details about the U.S. economy which introduce a helpful perspective to the problems usually analyzed in the course.

The first chapter contains a brief introduction to the history of economic doctrines. Other chapters deal with the decline of *laissez-faire* capitalism, industrial concentration, labor problems, agriculture, business cycles, international trade, and the role of the government in the economy. Well-chosen suggestions for further reading in each of these subjects are to be found at the end of the book. Also included is a statistical appendix with 24 tables showing GNP, population, immigration, income distribution, consumer price index, federal budget expenditures, etc. Most of these tables are taken directly from the *Historical Statistics of the United States, Colonial Times to 1957*, and most of the data have not been carried beyond 1957.

The chapter devoted to the world economy discusses the development of patterns of trade as the outcome of the spread of industrialization. In includes a clear explanation of the economic significance of tariffs, a lengthy discussion of the tariff history of the United States, and a description of the various international programs and agencies (IMF, IBRD, GATT, EEC). The agricultural essay states carefully arguments for and against governmental assistance to farmers. Chapter 4, "Labor," emphasizes the long-run trend of increasing governmental intervention in industrial relations. Some might question the author's interpretation that, because a secure labor movement is inherently conservative, "American labor, as a significant social force, is an adolescent. . . ." Most undergraduates of the reviewer's acquaintance would read "juvenile delinquent" here.

A chapter on the underdeveloped nations seems slightly out of place in this book. Although the author speaks briefly about the record of Western penetration of the former colonies, his treatment of this subject is essentially nonhistorical. Instead, it is primarily a summary of development literature which duplicates the material found in most of the principles texts.

As the author acknowledges, technical and theoretical concepts (e.g., terms of trade, diminishing returns, disguised unemployment, external economies) are introduced and used without definition. This is done deliberately, since the chapters are to be read in conjunction with textbook assignments and class discussion. The instructor should nevertheless be aware that an interpretation of these terms is important to the student's understanding of sizeable portions of the readings. One definition which does appear in the book should be rewritten. This is, "The aim of parity is to find a price per unit of crop produced that will produce today what it would have in some past period."

These essays do not contain specific policy recommendations. They do, however, reflect the author's point of view which can perhaps be summarized as follows: the growing interrelatedness of the many parts of the capitalistic economy resulting from the spread of industrialization leads to a need for increased social controls.

The book has certain minor faults. Some statements appear which need further elaboration. Examples are, "an industrial nation is generally able to bargain more effectively in international markets than a non-industrial nation" and, "he [Keynes] saw the basic elements of the crisis [of the 1930's], but he did not see the crisis in its historical meaning, and therefore he did not develop a theory fully adequate to the problem." In the chapter on industrial concentration the student is overdosed with statistics on degrees of market control and on mergers. Finally, it must be admitted that this is not a very lively book. While many might argue that the siren call of economic history is all but inaudible, the reviewer feels that a strong attempt should be made to enliven the subject matter for the beginning student. However, in terms of content, this book is well designed to fill a place in the introductory syllabus which has had very few occupants in the past.

F. S. O'Brien

Williams College

A Primer of Economic Development. By Robert J. Alexander. New York; Macmillan Company, 1962. Pp. 218. $5.00.

Professor Alexander, from a wide experience in the Latin American field, offers us here a brief, lucid, and elementary guide on how our economic system, with a few slight modifications, might be applied to underdeveloped countries. "Underdevelopment" is defined (p. 6) in terms of seven criteria: low per capita real income; an "unbalanced" economy; natural resources not exploited for local benefits; a traditional, rather than a market, economy; production based on labor rather than capital; structural underemployment; and a local belief that the area is underdeveloped. It is clear, however, from the statement (p. 5) that Australia and New Zealand "may correctly be considered as underdeveloped" that the salient characteristics of this status, in the author's opinion, are the existence of what he calls an "unbalanced" (that is, colonial) economy and of the familiar discontent of rising expectations.

On this basis, ten brief chapters consider how an economy, similar to our own (even in its possession of democracy, trade unions, social legislation, and free choice) can be established in underdeveloped areas. The chief topics are the familiar ones of industrialization, agricultural reform, capital accumulation, foreign aid and borrowing, lack of entrepreneurial and managerial skills, and labor problems. Alexander's point of view is entirely that of old-fashioned, pre-Keynesian, elementary economic theory with only two significant exceptions: (1) his rejection of the "law" of comparative advantage, and (2) his emphasis on the accumulation of capital locally by state action rather than from private savings.

The clarity, simplicity, and ethnocentricity of this book give it an immediate appeal. In fact, much discussion in Washington on the subject of underdevelopment has been expressed, far less well it is true, from Alexander's point of view. This is unfortunate because these ideas have, in my opinion, little relevance to the real problems in development. Their irrelevance appears so clearly in this book for the very reason that the ideas are presented so lucidly and simply.

The basic errors here are four in number: (1) the assumption that we are dealing with a dual situation (developed states and underdeveloped states) instead of a triple one (developed societies, traditional societies, and the area of mutual reaction between these); (2) the effort to advise and operate in terms of political units rather than areas or regions; (3) the effort to deal with problems arising from the interaction between developed and traditional societies in narrow economic terms rather than in terms of total acculturation; and (4) the assumption that the economic contribution to the accultural problem lies in export of our economic patterns with only that minimum of modification which seems absolutely unavoidable to our own ethnocentric vision.

In this context, traditional societies have no problems. Problems arise only when the impact of developed societies disrupts the patterns of thought, feeling, and action in a traditional society so that it can no longer function adequately or, in many cases, even survive. Alexander is perfectly right in his emphasis on discontent (or rising expectations) and on "unbalanced" economies, for these are characteristics of acculturation and not of traditional (that is, really underdeveloped) societies, but he is mistaken in believing that anything can be done about such problems of acculturation without deep understanding of the nature of the traditional societies themselves. And he is totally wrong if he believes that our patterns of behavior, even in the relatively narrow field of economics, can be exported to them with only slight modifications. This is impossible because the operational context in the acculturative process is (because of the nature of traditional societies) totally unfitted for our economic patterns.

For example, Alexander assumes that the discontent and rising expectations of native peoples are similar to our own and that they are, in economic matters, expressible in terms of our ideas of standards of living. Our whole economy is based on largely unconscious assumptions of future preference and infinitely expansible material demands. Most native peoples have outlooks based on present preference and, most non-Christian areas, especially Africa, have little idea of a future. Many African languages have no future tense and must use the present tense with a circumlocution to speak of the future. This reveals minds totally unadapted to planning, capital accumulation, progressive training, or even steady work habits. Many native peoples have little conception of individualism (including individual choice, responsibility, or initiative) and are almost totally absorbed in family, tribe, or local groups. The expectations of middle class Americans can be listed largely as material (and thus marketable) desires. The expectations of rising standards of living among most Africans would be food; social activities including music, dancing, and sex-play; leisure; a bicycle or, for a woman, possibly a sewing machine. Our Puritan outlook may condemn this as shiftless, but we should recognize that our economic system rests, to a great extent, on our Puritan heritage and cannot be exported to people who lack that heritage.

The priority of food in the rising expectations of most native peoples and the pressing need to increase food in the face of growing populations make agriculture the most urgent problem in any development scheme. But in agriculture, above all, we have little idea of what to do. U.S. agriculture looks like the most successful in the world; it arose in a context of cheap land and expensive labor, and sought high output per man-hour through labor-saving devices. Most underdeveloped areas have surplus labor and scarcity of land. Our agriculture, developed on temperate, glacial soils, with rain in all four seasons, has little to offer to underdeveloped, tropical, sun-created soils with seasonal rains and high ultra-violet radiations. The billion dollar fiasco of the Tanganyika ground-nut scheme of 15 years ago is a case in point. Quite different, but equally powerful, obstacles exist in Latin America (see Edgar Anderson, *Plants, Man, and Life;* Boston, 1952).

The unrealistic character of this book, even in the area Alexander knows best, can be seen by comparing it with such recent works, from the accultural point of view, as George M. Foster's *Traditional Cultures and the Impact of Technological Change* or Charles J. Erasmus' *Man Takes Control: Cultural Development and American Aid.* In fact, this work ignores things which are perfectly clear in earlier volumes from this same author. For example, he is fully aware, in other books, of the role played by armed forces in Latin American life. Yet here he ignores completely the vital problem of weapon control as part of the context within which economic development must operate. For example, he suggests (quite mistakenly) that agrarian reform (breaking up large estates) is a necessary condition for increased agricultural production (pp. 70-75) and suggests taxation up to 100 per cent of uncultivated large estates, but he does not consider for a moment that the success of this depends on who controls the local armed forces (as was shown so clearly in Spain in 1936-39). Alexander is prepared to ignore weapons and politics here, apparently because he considers them less fundamental than economics; but really they are more fundamental, for no development program can operate without political order which, in turn, is largely a question of weapon control (see, for example, Edwin Lieuwen, *Arms and Politics in Latin America*). The Congo's economic collapse is still too close for any of us to ignore the role of political stability and weapons in economic development, even in a primer on the subject.

CARROLL QUIGLEY

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Desenvolvimento e subdesenvolvimento. By CELSO FURTADO. Rio de Janeiro: Editôra Fundo de Cultura, 1961. Pp. 268.

Economic development has been a popular issue in Brazil for a decade and a half, with nationalistic overtones, Marxist undertones, journalistic trumpeting, and some rattles of outdated economic theorists. Celso Furtado undertook the task of cutting through this cacophonous din with simple but well-orchestrated instruments of economic analysis, adapted and tuned largely by himself. The author explains in a foreword that, at the time his interest in development began, little had been done in this area by the academic centers of the world. As a matter of fact, even the analytic instruments he had been taught to play by French and British virtuosos failed to function properly against the preindustrialization acoustics of Latin American economic structures, and thus required some refurbishing or some special adaptors. He searched in the attic of the museum of economic analysis and found that (1) the classic reeds produced some of the sounds—division of labor and capital accumulation—that could be integrated, after some modification, into an economic development symphony; (2) of the Marxian percussion instruments, the surplus, without the earsplitting value attachments, would set the right rhythm for the development prelude; (3) the neoclassical equipment was not suitable here, for—excellent though it was for the precise performance of sophisticated partial and general equilibria—it had dropped the classic growth keynotes; and (4) from the Keynesian apparatus, with Harrod-Domar extensions, some rousing dynamic sequences could be elicited, especially if properly tuned to cope with difficulties of penetration in a world of structural and institutional obstacles. Of course, with a given set of instruments one could play optimistic or dismal variations of the Classics, revolutionary marches, abstract compositions emphasizing pure form, amorphic *Unterma-*

lungen of historical epics, etc. Furtado chooses to weave a rather simple theoretical motif into a variety of themes taken from economic history. These carry us through a wide range of time and space, sometimes with elegance, as the interpretations of the Greek and Roman slave systems and of the medieval city economies, sometimes less convincing, as the part on the industrial revolution.

It ought to be inserted here that all chapters of the book, except the last, are elaborations of articles by the author published in Spanish, Italian, and English journals over a span of ten years, beginning in 1952. However, this joining together of a chronologically disrupted effort is hardly noticeable; there are some repetitions, but the over-all impression is one of consistency and unity.

Furtado defines underdevelopment as the existence of enormous differences in capital accumulation per person between two or more sectors in an economy. He admits total income as an additional indicator of such differences but rejects income-per-capita as inadequate for comparative estimates. Whereas a developed economy grows through the impact of Schumpeterian innovations, an underdeveloped country typically moves ahead through progressive penetration of the backward area by the methods of production already applied in the relatively advanced sector. In Latin America a two-level underdeveloped economy usually consists of a large backward subsistence region with a small, often foreign-controlled, export industry. The Brazilian case is somewhat more complex insofar as three sectors exist side by side, agricultural commodities and ores for export, an industry geared to the domestic market, and subsistence agriculture.

The first prerequisite for development is the formation of an "excedente," or surplus of the value of export revenues over the outlay for subsistence wages, net of profit remissions by foreign investors. This surplus is usually appropriated by a minority group which will invest at least part of it, the rest being spent on imports or, worse, on foreign assets. A second important element is the diversification of consumption, for if the composition of imports becomes sufficiently complex, it will be more likely for interstices to open, frequently in the form of a demand for complementary goods which can be manufactured by domestic firms. High export earnings in conjunction with low import prices will retard such incipient formation of an industrial nucleus; the same negative effects will result from such policies as price supports for export commodities, since this will syphon the surplus off into the export sector at the expense of the manufactures. On the other hand, a drastic fall in export revenues, resulting in a curtailed capacity to import, will raise the prices of imports at home and, if accompanied by correct anticyclical policies, encourage investments in import-substituting activities. Again, should the terms of trade improve suddenly, some kind of quantitative import controls may have to be instituted in order to protect the newborn industries; this would also implicitly subsidize and further stimulate the purchase of capital items from abroad. Complications may arise due to the channeling of investments into activities that do not substitute for imports, since in this fashion it will not be possible to clear enough of the limited capacity to import for capital imports required by other growing industries. Some industries will stagnate and lay off employees as a consequence, unless this is counteracted by appropriate monetary policies; but in that case the ensuing scramble for the scarce foreign exchange will result in rising prices of imported investment goods, in terms of local currency, and initiate waves of inflation. The latter part is a summary of Furtado's explanation of the structural-disequilibrium nature of the type of inflation that often accompanies the effort of export-dependent economies to build up their own industries.

The author also mentions irreversibilities in productive methods and in demand composition—one due to rigid factor proportions dictated by the imported technology, the other a consequence of the effort to substitute for imports of high capital-input density, but both leading to an undesirably low employment-capital ratio. Curiously, no mention is made of the role foreign capital has played in economic development, with the exception of a cryptic reference to some international wire-pulling by petroleum companies. Whether some of the suggested controls over the flow of private investments are practicable will be demonstrated by the performance of Brazil's recently adopted three-year plan, conceived and supervised by Furtado.

This book presents the ideas of an important man in an important country, but it is also a valuable contribution to the literature on economic development.

HANS G. MUELLER

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Prospects for Indian Development. By WILFRED MALENBAUM. New York: Free Press of Glencoe, 1962. Pp. 325. $6.00.

Professor Malenbaum has participated in the discussion of Indian development planning almost from the beginning. For him and for some others in and out of India, this discussion has had a distinctive agenda and a certain methodological approach. The main question at issue has been the "strategy of development" with its problems of the proper role of government, the size of the investment program, and the relative emphasis to be given to different industrial sectors. Should capital-intensive or labor-intensive production methods be adopted; should domestic savings be sought through taxation, deficits, and borrowing or through purely private saving and investment processes; and should India plan for increasing or declining net imports?

The method has been to discuss these problems at a fairly general level, proposing plans consisting of a limited number of macromagnitudes and following events in the economy in the same terms. Materials for the discussion have come chiefly from the Planning Commission's own publications, but also from the statistical organizations of government. The analysis is generally quantitative in the sense that numbers are used to express plans and to appraise performance, but the numbers often are only illustrative, or at best crude guesses.

Within the framework of this type of analysis, Malenbaum's book is an extended and important contribution. An emphasis of his own is added in two ways. Problems of the current period are shown to be closely related to the history and inherited structure of the economy. And he is most persistent in discussing the weak spots of government policy. The successes are left to other reporters.

In the first chapter, the necessity of thinking in terms of massively underutilized resources, both physical and human, is underlined. Next, the principal macromagnitudes of the first two plans (1951-61) are presented and comparisons made. Part 2 presents the economic history of India since about 1850, stresses the importance of economic activity not requiring market transactions for its implementation, performs some cross-section analysis of consumer expenditures, and discusses India's leadership. Plans I and II are taken up again in Part 3, comparing actual developments with planned developments. Finally, in Part 4, the author's own plan is compared with the official Third Plan.

A main line of Malenbaum's criticism of the plans is that they strive for more rapid change in the industrial composition of output than he considers possible or desirable. Contrary to the planned transformation there has been a steady upward trend in the share of services, which should have fallen, and no change in the share of manufacturing and mining output, which should have risen rapidly. The agricultural share is declining about as intended. The statistical results are reinforced by pointing out a number of factors which could have limited the rate of structural change: slow change in the composition of investment; limited private saving when investment is to be carried out increasingly by the government; and finally Malenbaum's prime reason which is that Indian economic activity is mainly private and governed more by habits and institutions of the past than by the modernizing influences of the future.

The argument is difficult to evaluate because the data are admittedly weak, the notion that India and Indians are hard to change needs more precise formulation, and other explanations such as limited foreign assistance are not clearly tied into the analysis. Yet there is point to the questions raised and it is worth finding out how far one can go in answering them by this type of analysis.

Specifying his own "Third Plan," the author agrees with the official plan that it is possible to achieve nearly a 6 per cent rate of growth in total output. But he says that it will take 25 per cent more investment and should not be expected to achieve as much structural change as the planners indicate. Furthermore he recommends assigning a larger fraction of investment to labor-intensive sectors, which are identified as small-scale industry and agriculture. These sectors are strongholds of private activity, and more emphasis on private investment will encourage much needed private savings. They are also said to be "bargain" sectors having lower capital-output ratios (pp. 302-3). This point is troublesome, however, because it is surely wrong to argue the case without regard to the cost of other inputs and the longevity of capital which is the effect of proposing allocations on the basis of capital-output ratios.

Approaching the whole question of investment allocation as a problem of allocation among industrial sectors, as this type of planning analysis does, is really more confusing than helpful. Here I am objecting not only to the approach of this book but to more than a decade of debate in terms of agriculture versus industry that has preoccupied this group of analysts. It is better to think of allocating investment to the production of alternative commodities (among which some substitution is possible despite the rather rigid demand projections that are now popular) and to alternative processes of producing those commodities. The sectoral distribution of investment emerges as an end result of the analysis and so do the sectoral capital-output ratios. There's not much that can be done in the way of criticising or modifying these end results without going back into the commodity distribution of output and the processes of producing those commodities. So they might as well be the topics of discussion in the first place.

Consider, for example, an exploration of alternative production processes for adding to rice output. One alternative might be dams and canals plus farm labor using the water in crop production; investment would be required in the agricultural sector and would lead to more output by that sector. Another alternative might be chemical fertilizer factories plus labor on the farms applying fertilizer and harvesting the results; investment would be required in the manufacturing sector and the value added (at least that part captured in the sale price of the fertilizer) would be credited to manufacturing output. In both cases more rice is produced and more underutilized farm labor is employed. Does it matter whether the investment and subsequent output are credited to the agricultural or manufacturing sectors?

For literature on Indian planning, 1962 was a bumper year. W. B. Reddaway's *The Development of the Indian Economy* outlines a comprehensive, commodity-centered, rather than sector-centered, approach. J. P. Lewis in his *Quiet Crisis in India* examines sympathetically and positively the full range of critical problems and ways of meeting them. Together with Malenbaum's study they form an exciting and valuable trio.

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Growth and Cycles in the Japanese Economy. By MIYOHEI SHINOHARA. Tokyo: Kinokuniya, 1962. Pp. vii, 349. $8.00 or ¥ 2400.

A chronic complaint of Japanese social scientists has been neglect by the Western world. For one man after another, for one generation after another, paraphrasing Sorokin's comment about Russian sociologists in Czarist times, *Japonicus est, itaque non legitur.* It is the great accomplishment of various translation projects, supported by several associations and foundations, that this situation is gradually ending, at least for Japan's outstanding "modern" economists.[1]

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[1] Modern economics in Japan has improved sharply, both in intellectual vigor and in Western-language fluency, since the inauguration of GARIOA and Fulbright foreign-study programs in 1950. (At the same time, the continued flourishing of mathematical social science has lowered the minimal threshold of linguistic competence.) Translation facilities are also open to Japan's flourishing Marxian school of economists, but members have been unwilling to authorize English translations, possibly from a feeling of intellectual or at least technical inferiority.

Professor Shinohara (of Hitotsubashi University's Institute of Economic Research) is one such outstanding Japanese modern economist. He is a working econometrician with Keynesian leanings and Keynesian-type policy interests. I have thought of him as a Japanese Lawrence Klein, although he is hardly Klein's equal in mathematical economics or theoretical statistics. Shinohara is Japanese-trained, except for one postdoctoral year (1955-56), primarily in the United States. Since his English is labored, he has published little in Western journals; his domestic (Japanese) reputation outruns his external one. The present volume is a translation, expansion, and revision of Shinohara's *Nihon-Keizai no Seichō to Junkan* (Tokyo: Sōbunsha, 1961), and is his first large-scale introduction to the international scholarly community.

The thirteen chapters were originally separate essays, nine of them completely new to Western readers. The first two sections, dealing with "Growth" and "Cycles" respectively, are more integrated than the third section on "Other Related Topics," but internationally-interesting tests and hypotheses are scattered widely throughout the book.

In the "Growth" section, for example, Shinohara accumulates evidence of the variability of capital-output ratios (p. 13) and input-output coefficients (pp. 161 f.), which have been treated as constants in many theoretical growth models and practical planning projections. (Shinohara's data, incidentally, come from abroad as well as from Japan, belying the title of his book.) While many writers, particularly in Latin America, bewail alleged declining tendencies in the terms of trade for agricultural products and raw materials, Shinohara translates them into increasing relative cheapness of Japanese goods on the world market, and therefore increased export quantum and volume, with accelerated growth for Japan (pp. 63-75, 98-106). One of Shinohara's more controversial stands, ascribing a share of Japanese growth to chronic undervaluation of the yen, is explained in Chapter 2, but he does not translate his recent Japanese-language application to the current scene ("Jiyūka to 360-En Rēto" [Trade Liberalization and the 360-Yen Rate], *Ekonomisuto,* November 10, 1959).

Shinohara also outlines three novel hypotheses in growth theory. One (pp. 92-94) is a natural tendency toward balance by equalization of growth rates by major industrial groups. The second is a stress on the role of fluctuations, making the Japanese case "a high rate of growth by means of an unstable oscillation of investment and prices, and not as smooth-going a process as is formulated in modern economic growth theory" (p. 108). With special relevance to the period since 1945, Shinohara marshals statistical evidence to show that, at least in the capitalist world, the countries which had the most striking growth records for the decade beginning in 1948 were also those which suffered most severely in the preceding decade (pp. ii, 7-10). (This result, incidentally, confirms one of the several generalizations which contend for the title of "Friedman's Law.")

Chapter 5, dealing with the contemporary Japanese growth controversy, unfortunately assumes somewhat more background than Western readers will have, or can obtain easily. Shinohara, like most Japanese academic economists modern and Marxian alike, is pessimistic as to the staying-power and long-term significance of Japan's post-Korea "economic miracle." He speaks of "a reckless increase in investment, and the extraordinary rate of growth attained as a consequence," with firms seeking "to expand their investment beyond the target prescribed by the plan in order to increase their market share" (p. iii).[2] His leading optimistic opponent, Osamu Shimomura of the Japan Development Bank, believes he has shown (from "magic constants" *à la* Colin Clark or Sidney Weintraub) that the Japanese GNP can grow by at least 9 per cent per year until 1970 (p. 139, Table 4, Column 8). The official "Doubling-Income Plan" of the Japanese government and its Economic Planning Agency is a somewhat flabby and internally-inconsistent compromise between optimistic and pessimistic extremes, obviously closer to Shimomura than to the academics. Rather than attacking Shimomura on forthright methodological grounds (which would not be easy, since Shinohara occasionally resorts to magic constants himself), our author pecks away at minor statistical peccadillos. Although Shimomura is quoted both frequently and fairly, his position fails to come through (perhaps because of numerous modifications). It would be most desirable, both in view of Shimomura's status as economic "gray eminence" of the Ikeda government and of his numerous writings in Japanese[3] for some of his own works to become available to foreign critics and prospective imitators of the "economic miracle."

Passing to Part 2 (on cycles as such), Shinohara derives a promising forecasting device from the fact of dualism in the Japanese economy. He shows (in Chapter 8) that inventory and credit movements for small business lead those for large ones, and thence for the economy as a whole, so that the ratio of loans of small firms to loans to large ones is a useful economic barometer. Two of the "miscellaneous" essays of Part 3 will also repay careful study. The first of these is his explanation of Japan's high saving ratio (Chapter 10) on permanent-income-hypothesis grounds, citing the high proportion of entrepreneurs in the labor force and the payment of a large and uncertain fraction of wages in the form of mid-winter and mid-summer bonuses. The other is Shinohara's survey of Japanese production-function literature (Chapter 13), which gives the Cobb-Douglas function higher marks than are currently fashionable. This worries Shinohara, who, while following the data where they lead him, has a slightly irrational fear of being classified as "Douglasian."

Not the least valuable aspect of this book to the Western reader is the frequent reproduction of Japanese data *in extenso*. Both indexes and footnotes are usable, but a bibliography of Japanese-language sources would have been welcome. The English could have profited in spots by additional editorial revision, but is easily comprehensible as it stands.

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[2] Treatment in terms of an upward displacement from the unstable knife-edge equilibrium growth path of a Harrod or Domar model would seem to be in order here, but Shinohara, generally suspicious of aggregative growth models, does not provide it.

[3] I have two such works in mind, *Keizai-Seichō Jitsugen no tame ni* (*For the Realization of Economic Growth,* 1958) and *Nihon-Keizai no Seichō-Ryoku* (*The Growth Potential of the Japanese Economy,* 1960), the second being an extended controversy with a number of academic critics. The Indian Statistical Institute has, however, mimeographed an English translation of a shorter essay, *Keizai-Seichō Seisaku no Kihon-Mondai* (Basic Problems of Economic Growth Policy), as No. 37 of its Translation Series (Calcutta, 1961).

M. BRONFENBRENNER

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Japan—Wirtschaftsmacht des Fernen Ostens: Ein Beitrag zur Analyse des wirtschaftlichen Wachstums. By KARL HAX. Cologne and Opladen: Westdeutscher Verlag, 1961. Pp. 632. DM 56.

The author's intention was to describe and analyze the rapid economic (especially industrial) development in Japan in comparison with the "economic miracle" of Western Germany. His premise, expressed in the introductory note, is that the cause must be found in economic and social forces because in both countries political and military power were broken as a result of war.

As usual, one can trail the author's point of departure throughout the work. The influence of the American occupation is hardly mentioned; neither are the U.S. aid programs and investments, nor the profound effect on technology, productivity, and efficiency of wholesale after-war reconstruction of industries. The causes underlying the rapid recovery are sought by the author in sense of duty, hard work, efficient organization, and social control. And indeed, there are striking similarities in this respect between German and Japanese laborers and industrialists. If one wants to make a comparison as regards the physical environment, raw materials, and foreign markets, Japan had a much more arduous task throughout modern history, from 1870 onwards, than Germany. The achievements are all the more remarkable. It is again, I believe, a reflection of the impact of this busy country on a German mind that the book ends on a very hopeful note with regard to future development.

The major part of the book is a concise but epic description of nearly all sectors of Japanese national development. As an industrialist, the author, however, underrates agriculture, forestry, and fisheries in their primary function, considering them as a basis for industrial development. He hardly touches upon the interrelations between rural and urban development in the exchange of labor, services, and market opportunities. The enormous number of people working part-time in agriculture, part-time in industry or service-industries is of vital importance to aggregate production.

The great emphasis on banking and insurance is very commendable, and this part of the book reads very well. Similarly, the chapter on the interrelation between the state, large enterprise, and labor unions is very interesting. The revival of Zaibatsu is mentioned with obvious pleasure.

This very thorough treatment is a token of congeniality between author and the object of his research; its analysis, conclusions, and outlook reflect a common experience and a socio-economic, perhaps even political, ethos. This degree of identification is indeed remarkable; it makes the book very interesting as an object for analyzing the analysis. If one wants completely different interpretations, i.e., through British or U.S. eyes, one should compare the books of Allan and Dornithorne or E. E. Hagen. This reviewer has added a slight Dutch ingredient by way of further variety.

E. DE VRIES

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Viabilidad economica de America Latina. By VICTOR I. URQUIDI. Mexico, D.F.: Fondo de Cultura Economica, 1962. Pp. 205.

This book deals only indirectly with the problem of the economic viability of Latin America. In the introduction the author, a prominent Mexican economist, explains that the title is somewhat ambitious yet the study indicates his basic optimistic attitude concerning the economic future of Latin America. He states that there are more positive than negative factors present and that most of the negative ones can be changed through actions of the people of Latin America. Thus it is up to the reader to assess the positive factors and to answer the question in the title. The study covers a very wide range of topics concerning both domestic and international problems.

In the chapter on some structural problems of Latin America the author manages to present well the features common to most of the countries reviewed, in an area where substantial differences of economic and social development exist. In Latin America commercial, financial, government, and other services, plus transport, account on the average for 47 per cent of GNP (the author's figure). It would be interesting to know the share of the tertiary industry in the rate of growth in countries like Brazil which have gone through a high degree of inflation so that one could determine the real meaning of this rate of growth. One certainly agrees with the author's urging that in order to increase the standard of living the countries must step up agricultural production—advice which has not been popular.

One of the best chapters is on the financial and monetary difficulties of Latin America. While Urquidi is critical of government finance, and particularly of the deficits of public enterprises and of their monetary policies, and while he points out that countries with acute inflation are not always the ones which have a high degree of development, he also refers to many noneconomic factors as responsible for inflation. This reviewer wishes Urquidi had taken a clearer stand on this vital and much-disputed issue.

Urquidi is at his best when he discusses the political and institutional changes required for development. His views concerning the needed reforms in taxation, the maldistribution of national income, low-cost housing, agrarian reform, and a whole range of social services are—as might be expected—very much in line with the Alliance for Progress programs. And he states bluntly that the system of property now prevailing does not make accelerated development possible. Because of the need for change the author is convinced that more general and intensive government intervention is required to carry out an adequate rate of development. He recommends well-prepared planning for development based on thorough knowledge of available resources and including the private sector of the economy; he rightly concludes that various political and institutional obstructions must be overcome to achieve economic progress. Urquidi realizes the many difficulties inherent in the present setup, particularly the lack of experienced, capable, and well-paid public servants.

Somewhat less balanced are the parts dealing with international problems, especially the U.S. policy toward Latin America. Before the discussion of the Alliance for Progress agreement a brief review of this policy is given. The author mentions that it was thought Latin America would be able to return to normalcy on its own after World War II because it had emerged from the war with substantial foreign exchange holdings—a view that was strengthened again when Latin America, as a result of high prices of its main exports, benefited greatly from the Korean boom. However, the substantial foreign assets were often not used to finance imports needed for development.

Urquidi is critical of a statement made by the U.S. delegation at the Bogotá conference in 1948 that no kind of Marshall Plan was possible for Latin America, and that it was important to create a climate that would be favorable for private investment. But he admits that the Latin American countries at that time had no well-prepared programs that could have served as a basis for a Marshall-Plan approach, had such an approach been possible.

Although Latin America was the largest recipient of U.S. private investment next to Canada, the author does not attribute any important role to this investment, pointing out that it went largely into mining and petroleum. He realizes the need of modern industrial technology for the development of Latin America and suggests that the industrial countries should arrange for free access to modern technology, but he stresses that the Latin American industries should not become dependent on foreign enterprises and foreign capital participation so that technical colonialism might be avoided.

According to Urquidi, ideological and political prejudices were generally the reasons why U.S. policy always preferred direct investment and did not pay proper attention to the needs of Latin America. The author maintains that the same line was followed by the World Bank.

The situation changed only at the conference in Bogotá in 1960, which can be regarded as the predecessor of the Alliance for Progress agreement of 1961. Earlier, in 1958, a Pan-American operation requiring vast sums was proclaimed in Brazilia. In Urquidi's opinion the significance of this new development lies in the fact that the United States accepted for the first time the concept that economic and social development must be pursued simultaneously.

The question arises: could any policy or action of the United States or an international agency substitute for determined domestic action on which the eventual success of economic development depends, without that country or agency being blamed for interfering in the nation's internal policies? And one may ask: why had the Latin American countries themselves not agreed on programs and actions to be carried out earlier? The study provides the reader with an answer. The need for a change has been growing gradually, but in many countries political forces have not been equal to their task.

There is no doubt about the viability of the Latin American economy, but to carry on continuous progress more must be done mainly in internal policies, without regard to the growing external assistance.

This book contains a wealth of information and stimulating thought and gives a good review of economic problems of Latin America. It should be read widely and particularly by those who have the important say in politics in this part of the world.

ANTONIN BASCH

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Small-Scale Industry in the Soviet Union. NBER Occasional Paper No. 80. By ADAM KAUFMAN. New York: National Bureau of Economic Research, 1962. Pp. xvi, 95. $2.00.

In preindustrial Russia, as in all nonindustrial societies, the products of handicraft, cottage, and other small-scale industry made up the bulk of fabricated consumers' goods. As industrialization proceeded, large-scale industry took over the production of most of these goods. A large portion of small-scale industrial output went unmeasured, but once that output had been shifted to large-scale enterprises, it was measured with gusto. The implications for statistics of industrial output are clear. Much of the growth reflected in those statistics may be a mirage, particularly when such data are deliberately restricted to a coverage of large-scale industry.

Few would deny that early Soviet measures of industrial output were distorted by the inclusion of previously unmeasured production. Scrupulous souls should similarly concede that this phenomenon is not unique to the Soviet Union. Though there may be agreement on the existence of distortion, honest men may quarrel about its extent. Adam Kaufman has made a heroic attempt to assemble data on the magnitude of small-scale industrial output and employment in Tsarist Russia and the Soviet Union. On the strength of these diggings, he has tried to assay the extent of the aforesaid distortion.

Kaufman's research was handicapped by the fact that no official statistical investigations of small-scale industry were carried out in Tsarist Russia. Several official and unofficial studies of the prerevolutionary situation were made in the 1920's, but they differed substantially in their coverage and conclusions. After digesting these findings, Kaufman concludes that, on the eve of the Revolution, small-scale industry had about half of the employment and a third of the value of output of all industry. It was concentrated in the textile and foods industries and was mainly rural in location.

Small-scale industry recovered quickly from its Civil War depths, and during the New Economic Policy it assumed great importance. Fortunately the quantity and quality of data improved in the late 1920's; significant gaps remained in the statistics, but Kaufman was able to use the 1929-30 general census of small-scale industry. At its zenith in the preplan period, Kaufman estimates that small-scale industry accounted for 43 per cent of total industrial employment and 31 per cent of the value of all industrial output. Although Kaufman has tried to correct the official data for certain omissions, I think that he may still underestimate the true size of small-scale production.

Collectivization and forced industrialization doomed small-scale industry in the USSR. Death was partly real and partly statistical. Many private producers were swallowed by producer cooperatives. The definition of large-scale industry was steadily broadened to include many enterprises formerly counted as small-scale. Many small-scale private producers ceased production altogether under the pressures of collectivization and ideological hostility. Employment in small-scale industry dropped from 45 per cent of total industrial employment in 1927-28 to 8 per cent of the total in 1933. The absolute number of workers in small-scale industry in 1933 was only a third of the 1927-28 level. Kaufman estimates that the net value of small-scale output in 1932 was about 37 per cent of its 1927-28 level.

Despite a dearth of data, Kaufman has constructed an index of small-scale industrial output for the period 1913-33. The index is based on physical output series for 27 industrial materials covering the years 1913, 1927-28, and 1933. Of these, data were available on only 19 for 1933. Aggregation is by 1926-28 "unit value" weights.[1]

Given the limitations of the data, Kaufman's index seems to me to be a very worth-while measure. In particular, it appears to be immune to some of the criticisms that have been aimed at Warren Nutter's index of industrial materials. For example, the degree of fabrication of goods produced by handicraftsmen probably did not increase appreciably between 1913 and 1933. For this reason, Kaufman's index of materials produced by small-scale industry may approximate the growth of total net output of that sector more closely than Nutter's more comprehensive index of industrial materials approximates the growth of total industrial output.

Kaufman has factored the total growth of industrial materials production between 1913 and 1933 into parts contributed by large-scale and small-scale industry. He estimates that the 37 per cent increase in total industrial materials production over the period was composed of an increase of 83 per cent in the large-scale sector and a decline of 68 per cent in the small-scale sector.

This study, undertaken as a part of the NBER's larger research project on Soviet economic growth, is a valuable contribution to our knowledge of a complex subject.

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[1] These are a mixture of estimated prices and values added that have been concocted by the NBER and are described in detail in Table D-8, *The Growth of Industrial Production in the Soviet Union* by G. Warren Nutter, NBER, 1962, pp. 530-567.

Der Wachstumsprozess in der Sowjetwirtschaft: Eine Untersuchung der Produktionsstruktur, des Lenkungsprozesses und des Volkseinkommens. By FRANZ J. HINKELAMMERT. Berlin: Duncker & Humblot, 1961. Pp. 114. DM 16.

Hinkelammert's monograph on the theory of Soviet economic growth has four chapters. The first discusses the history of Soviet doctrines of economic development, starting with Marx, Tugan-Baranovsky, and Hilferding in the background and covering Lenin, Trotsky, Bukharin, and Stalin, up to the recent debates on value and the efficiency of capital investments. The second chapter compares the two-sector growth models of Böhm-Bawerk, Marx, Domar, and Feldman and supplements them with the author's own model of the Soviet-type "equilibriumless" growth. The third treats the main elements and criteria of the Soviet theory of development planning; and the last chapter contemplates the Soviet conception of the structure of national income as an indicator of the results of economic growth.

With such a broad historicotheoretical approach Hinkelammert's dissertation arrives almost as the first-born into the conceptually rather infertile family of Western studies of Soviet growth—a point aptly noted by Karl Thalheim, who wrote the introduction. For this reason, Hinkelammert's work is indeed welcome. However, the "baby" is not very heavy, and it still has much to learn. Hinkelammert's treatment of the history of Soviet economic thought, as it relates to his subject, is no more than a draft outline, all too sketchy and incomplete up to the brink of error.

It is, for example, hardly sufficient to declare merely that "in Marxist tradition, equilibrium has always been understood as the goal of Socialism," and not to explain that Marx's socialist equilibrium consisted of (a) a notion of full employment, (b) the production of what was "socially necessary" according to a master plan, and (c) implied technical, rather than economic, equilibria within the plan. Having insufficiently developed this point, Hinkelammert missed an important link between the Stalinist theory of planning, which he has otherwise adequately described and criticized, and the original Marxist theory. If "social necessity" is in one way or another given in the plan and described by its goals and priorities (whether in terms of money or *in natura*), or, for that matter, if the necessary value data in a two-sector model are somehow determined exogenously (by the "Law of Value," for example), the economic equilibria inside the plan, or inside the two-sector model, inevitably become but derived functions of the given data. Moreover, these functions are overdetermined because they all inevitably reduce to the technical coefficients and physical input-output ratios required to fulfill the plan. Hence objective, independent economic equilibria cease to exist inside a given plan and inside an economy that works according to such plan; unless, of course, the latter condition is not obliging, the economy works independently of the plan, and the plan itself is not a plan but a mere sheet of paper, a *l'art pour l'art* model, to which the economy is compared *ex post* rather than *ex ante*. Hinkelammert thus missed the point that the decisive choice, which has made Soviet planning what it is, has really been not the choice between "planning in kind" and "planning in money," but rather whether or not plans should be carried out at all, whether or not their fulfillment should be enforced and made obligatory.

Why Stalin made his plans compulsory, Hinkelammert clearly explains in his own model of the Soviet economic growth: the planners had to hold consumption down to a minimum in order to divert resources into capital goods and arms production. This type of economic development Hinkelammert calls "equilibriumless," although he admits that such a label sticks to it merely because this type of growth is incompatible with the notion of equilibrium peculiar to a consumer-oriented economy, and because this type of growth in a competitive economy is impossible. Following, in a way, the fitting ideas of Tugan-Baranovsky (to which he could have added Marx's little-known, but in this peculiar connection even more fitting, *Grundrisse der Kritik der Politischen Oekonomie* [Ch. 3, Sec. 1]), Hinkelammert has developed his model to show how the Soviet economy, at a certain level of capital accumulation, may become capable of simultaneously increasing the production of both capital goods (including weapons) and consumer goods.

Among other things of interest is Hinkelammert's conclusion that, although Soviet economy could be guided by the principle of profit equalization without abandoning its present centralized system of allocations, this principle per se would prevent the maximization of over-all growth. This, then, again suggests why the Soviets refuse to accept such a principle.

VSEVOLOD HOLUBNYCHY

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Dimensions of Soviet Economic Power. Hearings Together with Compilation of Studies Prepared for the Joint Economic Committee, December 10 and 11, 1962. Washington: Superintendent of Documents, 1962. Pp. iii, 185; xix, 744. $2.75.

As the title may suggest, this (intellectually as well as physically) weighty volume contains statements before the Joint Economic Committee by Soviet specialists on the growth of the Russian economy since roughly World War I, and detailed comparisons of its strength with that of the U.S. economy as of mid-1962. In terms of both the quality and the quantity of the material, the volume clearly warrants description as a major contribution to the literature, a source likely to be referred to frequently for many years by Soviet specialists. It is also likely to be very useful to nonspecialists who, on occasion, may need to dip into its wealth of statistical data on a wide variety of topics,

A sketch of the contents will underscore the volume's encyclopedic nature. In the hearings section, there is a statement by Paul G. Hoffman on "Competition in Aid to Underdeveloped Countries," and a discussion of "Dimensions of the Soviet Economy and Trade Offensive" by Warren W. Eason, Willard L. Thorp, and Vermont Royster. These are followed by discussions of the "Policy Aspects of the Soviet Economic Offensive" by Roger Hilsman, Holland Hunter, Warren G. Nutter, and John Scott. The foregoing, comprising 67 pages of layman's prose and other material intended for the nonspecialist, is supported by a 118-page appendix consisting of three excellent papers complete with statistical data.

The hearings are followed by 744 pages of studies prepared for the Committee by the staff of the Research Analysis Corporation and by a large number of academic and governmental economists. The papers deal with "The Policy Framework," "The Measure of Production," "The Strategy of

Production," "The Development of Human Resources," "The Share of the Citizen," "The External Impact," "Demographic Trends and Population Policy," and "Employment." The volume concludes with a 20-page "Selected Bibliography of Recent Soviet Monographs" and a 55-page "Statistical Appendix." Unfortunately, space does not permit the reviewer even to list the titles and authors of the 23 studies, much less review all of their individual contributions. However, brief consideration of a few is possible.

John P. Hardt in a well-written, stimulating paper, "Strategic Alternatives in Soviet Resource Allocation Policy," states that increasingly severe strains have occurred and will continue to occur in the Soviet economy as it enters a new phase of competition with the West wherein the number of economic, military, and political claimants to its resource base is proliferated. While this reviewer agrees, it should be observed, as Hardt himself does, that strict rationing of resources even among high priority programs is not a new problem in the Soviet Union. While it is undoubtedly true that the Soviets may find it increasingly difficult to keep up simultaneously the power struggle with the West, the modernization of their economy, and the improvement of living standards, this reviewer is not as certain as Hardt that the new experience may require an accommodation that they are not geared to make. For all of the Russian economy's well-publicized difficulties, and recent studies by some Western economists which have led them to conclude that its growth and performance have been overstated, it remains true that the Russians have shown long-term agility in surmounting major planning problems in the past; and, as Messrs. Hunter, Nutter, and Scott point out, "more pragmatic figures" and programs in the future are always possible. Herbert Levine concludes his article on planning in the volume with essentially the same notion: that it would be prudent to expect improvements in the planning system and hence a better performance in the future. Lest readers think that Hardt is alone on a limb, it should be observed that there is considerable room for difference of opinion.

Turning to less speculative matters, Nicholas De Witt in "Education and the Development of Human Resources: Soviet and American Effort" concludes that the quality of Soviet professional training in scientific, engineering, and applied fields today, on substantive grounds, is comparable to that offered in the West. He finds that the planners have succeeded in increasing "spectacularly" the rate at which specialists are produced. He predicts that if the USSR's targets are met, by 1980, the Soviet Union will probably have caught up with the United States in total enrollments in higher education. To those of us who have long been aghast at the incredible waste of human resources in both absolute and relative terms in the United States, this hardly comes as a surprise. What may be surprising to many economists is that the closing of the gap by the Soviets may occur at a time when their lead in population size has disappeared; so that in both countries the percentage of the total population engaged in higher education would then be roughly equal! This possibility emerges from a combination of De Witt's findings with those of James W. Brackett in "Demographic Trends and Population

Policy in the Soviet Union." Brackett observes that while, since 1913, the USSR has had a larger population than the United States, the successive annual differences between the sizes of the two have been decreasing in both relative and absolute terms and by 1980 the gap is likely to be no more than 22 million in absolute terms and 8.1 per cent in relative terms. However, if fertility in the Soviet Union declines, as it presently seems likely to do, and if fertility in the United States remains constant, according to Brackett, by 1980 the population of the United States may be greater than that of the Soviet Union. In any case, after 1980 as before it, lead in population size is not likely to be a major determinant of lead in total enrollment in higher education.

It would be easy to criticize the volume on a wide variety of petty grounds: duplication of material in some papers, lack of uniformity of organization among others, etc. What would ordinarily be minor vices are, in this case, major virtues. Close collaboration among the contributors would undoubtedly have resulted in watered-down, conservative "committee-type" conclusions instead of the independent, sharply written, sometimes conflicting ones contained herein. The product as published has all of the communicable excitement and variability of a "live" TV program as compared to the glossy, patent smoothness, and mechanical perfection of the same program on tape.

SIDNEY KLEIN

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Material'noe stimulirovanie v narodnom khoziaistve Vengrii (Material Incentives in the National Economy of Hungary). By ISHTVAN FRISCH AND OTHERS. Budapest: Institute of Economics, Hungarian Academy of Sciences, 1962. Pp. 100. $2.00.

In this collection of five articles the director of the Hungarian Institute of Economics and four of his associates discuss some of the problems encountered in introducing personal economic incentives into the Hungarian economy after the "counter-revolution" of 1956. Academician Frisch first quotes Marx and Lenin to justify the payment of workers in accordance with their individual productivity in a socialist economy, then distinguishes between the function of profits under capitalism and the proletarian state, and finally analyzes some of the "contradictions that create confusion in economic life."

These contradictions often arise, he says, "out of the two-sided nature of personal interest." For example:

> If material encouragement is stipulated for fulfillment of a certain planned goal, the workers are interested, on the one hand, in fulfilling the plan and, on the other hand, in fulfilling it in the easiest way, with the least expenditure of energy. In such cases the interested workers will direct their efforts not only to fulfilling or overfulfilling the target but also so that the given index will serve their needs best, irrespective of the actual possibilities or needs. If fulfillment of the production plan is tied in with material benefits, they will strive to have a lower volume of production

> prescribed, and in the interests of this they will be inclined to conceal internal reserves of a further rise in production; if material encouragement is tied in with curtailment of production expenditures, they will be inclined to conceal possibilities of further cost cutting; seeking in both cases the easiest unimpeded achievement of the established goal.

Frisch finds that these human attitudes that have been carried over into the new economic system constitute obstacles to technical progress similar, it would appear, to the situation with regard to automation in the United States today. He writes:

> Technical progress is often accompanied by a reduction or losses in productive output and involves an increase in production cost. In the future, however, possibly within a year or two, production will rise significantly and costs will fall correspondingly. Nevertheless, in consequence of the loss in production and the rise in costs, a firm's labor, especially its supervisory labor, will suffer a material loss in the current year, a decline in earnings. The workers do not know whether they will be able to obtain in the future adequate compensation for these losses. Their personal interest thus conflicts with the interests of technical progress. In such cases it is difficult to count on their enthusiasm and full utilization of their productive powers in the struggle for technical progress. Sometimes technical progress is impeded or delayed also by less important causes. For example, when workers of great skill are required for the mastery of some kind of progressive technological process, the level of average wages may rise. The index of average wages is not only strictly ordered by the firm and controlled from above, but its increase brings about a definite material loss for the engineering-technical workers, depriving them of bonuses.
>
> In such cases we should not expect resolution of the contradictions only from a proper realization of the principle of personal interest, although naturally we should do everything possible so that the workers, and first of all the engineering-technical workers who can contribute most to technical progress, should be materially interested in the mastery of more progressive and modern productive methods. Along with this we should take corresponding measures to influence the consciousness of the workers, and by no means least, the control organ that stands above the enterprise should manifest a certain flexibility, taking into consideration that what is on the whole a good rule—that a condition for awarding bonuses is fulfillment of the plan for production and cost or maintenance of the prescribed level of average wages—in a given case may impede or prevent the achievement of a more important goal. In such a case, therefore, application of the general rule should be avoided.

Despite these problems and others that are discussed frankly by the authors, they find that the introduction of incentive methods of payment has increased production in industry, agricultural producers' cooperatives, and retail trade. In the socialized sector of industry, the Central Statistical Bureau reports, the percentage of labor hours paid by the piece increased from 48.2 in 1957 to 58.9 in 1960. In retail trade premium wage payments for fulfillment of monthly sales norms are reported to comprise one-fifth of total labor payments.

Methods of rewarding engineering-technical personnel are treated in a separate chapter. They include profit-sharing, bonuses, and special premiums. It is noted that a shift toward annual planning and the determination of material incentives on an annual basis have produced better results.

This book reveals ample evidence of the kinds of human resistance to any movement toward truly "communist" methods of labor payment with which Communist planners and ideologists are having to contend increasingly, not only in backward Communist countries like Hungary but also in the advanced Soviet Union.

ROLAND GIBSON

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The Third Five Year Plan & India's Economic Growth. Edited by B. C. TANDON. Allahabad: Chaitanya Publishing House, 1962. Pp. 324. $3.50.

This book consists of 27 articles written by 26 authors, among whom are some of India's most distinguished economists. The essays deal with various problems of economic development in India at the time the third five-year plan was being formalized. More than half of the essays have been previously published in various popular and semipopular Indian journals and all of them are oriented to informing the intelligent layman. A wide variety of issues is covered—regional economic development, inequality of incomes, resource mobilization via monetary and fiscal devices, pricing policies, consumption growth, foreign trade and aid. The essays are uneven in quality but some are quite useful. For example, the editor's article on "Inequality of Incomes" brings together information that is not otherwise easily accessible. All the articles were written before the end of 1961 and therefore do not consider the impact on development of the new emphasis on defense expenditures.

While the scholarly apparatus is limited, the volume might have considerable use for those who are teaching courses in economic development where summary essays on the Indian economy would be of help.

MORRIS DAVID MORRIS

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Talks on Planning. By P. C. MAHALANOBIS. New York: Asia Publishing House, 1961. Pp. vi, 159. $7.00.

One must conclude after reading the speeches and articles on planning of P. C. Mahalanobis, the great organizer and idea man of India's second and third five-year plans, that here is a man of determination, imagination, intellectual competence, and also the possessor of a great zest for action within a given theoretical pattern. However, Free World leaders would feel more comfortable in working with India if the author's theories and plans for industrial development, which are so well presented in this book, had failed to gain unqualified government support.

Mahalanobis, the physicist, was able to convince the government economists and others that: (1) economic growth was taking place much more rapidly in the planned economies of the socialist nations than in the unplanned capitalistic economies and that this was largely true because the planned economies placed great emphasis upon developing heavy industry first; and (2) Indian agriculture and general consumer goods industries would have to stay in the handicraft stage until India had developed her basic industries, which were defined as producers of industrial raw materials and heavy machinery and equipment of all types.

India's first five-year plan (1951-52 to 1955-56) emphasized agriculture and village industries with little provision for basic industries, at least according to Mahalanobis's definition. The draft plan outline which was developed in 1955 became the second five-year plan. "Emphasis was placed on heavy machine building, heavy electrical, steel and non-ferrous metals and energy to supply a sound foundation for economic self-reliance." Also, in December 1954, the Congress Party and the Indian government decided the time had come for a formal declaration that the socialistic approach would be used in the pursuit of economic development. Over and over in his speeches Mahalanobis quotes this basic decision and what it means to him: that investment capital is to be largely controlled by the government, and that the government will use it in the way specified in the above quotation.

From Mahalanobis's speeches one gains the impression that steel production was his guiding star and basic measurement of economic progress. In "Approach to Planning in India," the author states that "For a single factor, the production of steel has the highest correlation with national income in different countries." Elsewhere, the author talks as though the expansion in Russian steel production is the measure of relative success of the Russian and U.S. economies. This need to expand India's steel production for a social economic development program seems to have become very nearly an obsession with Mahalanobis and, through him, with India's economic planners.

When the accomplishments of the second five-year plan began to fall short of prior forecasts, Mahalanobis pointed to the great need for scientists in India. The fact that Russia was training more engineers than was the United States prompted him to advocate the Russian approach to economic development—this time in the field of technical education. His general advice for the third five-year plan was more of the same including a great deal more steel, plus the establishment of an educational system along Russian lines. He recommended 10 million tons of steel capacity and not the "six or eight million tons" others advocated. Market demand was alleged to be "scarcely relevant" in a socialist economy.

Mahalanobis and his followers have been able to give the industrial sector of the Indian economy a wrench. The testing time is near and the United States and Great Britain find themselves deeply involved in assuring the success of an economic development program possessing a Russian-type planned industrial growth pattern. Neither India's nor China's record nor the world's record of economic development demonstrates that "This is the only way to secure increasing supplies of machinery and energy, with which we can create more employment and also can give the workers more machinery so that the total production in the country can rise rapidly" (p. 95). But Mahalanobis believes this.

Talks on Planning should be included in the reading list of every graduate or undergraduate course on economic development. I recommend this because the book gives a real feel for the stuff out of which economic programs are made and sold.

R. W. Lindholm

University of Oregon

Economic Systems; Planning and Reform; Cooperation

Problem zakona vrijednosti u prelaznom periodu (The Problem of the Law of Value in Socialism). By Ivan Vrancić. Zagreb: Ekonomski Pregled, 1961. Pp. 133. Din 600.

Since the early 1950's Yugoslav economists have been engaged in attempting to adjust Marx's economic model to present socioeconomic conditions and to build a Marxian economic theory of socialism. Vrancić's book represents an analytical endeavor to achieve both these aims.

The author analyzes the problem of equilibrium allocation of resources in classical and developed capitalism and in socialism. The major analytical tool of the author is Marx's law of value. His analysis rests on three basic assumptions: the labor theory of value, the law of average rate of profit, and the classical concept of money.

Because of the law of value, industry will tend toward the following equilibrium relationship: $Yn = d\Sigma n$, where Yn is the number of man-hours allocated to the industry, d is the price, and n is the product of the industry. According to the author, this relationship holds for classical capitalism, developed capitalism, and socialism. The author asserts, however, that the qualitative content of this relationship is different for each of these three stages of economic development. Therefore analysis of the qualitative content of the law of value fosters our understanding of the basic properties of different socioeconomic systems.

In classical capitalism, competition tends to establish the following equilibrium relationship: $d = c + v + m$, where c is depreciation of fixed capital, v is variable capital, and m is surplus value. In developed capitalism, the capital intensive industries will sell their products at an equilibrium price of $d > c + v + m$, while labor intensive industries will sell their products at $d < c + v + m$. Since in the Marxian model $\Sigma d = \Sigma(c + v + m)$ the equilibrium relationship $Yn = d\Sigma n$ indicates the fundamental characteristics of developed capitalism: the inherent degree of exploitation of labor intensive industries by capital intensive industries. This basic characteristic of developed capitalism creates a tendency toward labor-saving innovations and a rise in the ratio of labor to capital.

In socialism, according to the author, the law of value cannot be disregarded. Contrary to the opinion of a number of Soviet economists, Vrancić seems to believe that the law of value must exist under the conditions of scarcity, otherwise the allocation of resources (Yn) becomes necessarily arbitrary.

Vrancić maintains that the Yugoslav "experiment" has indicated the nature and scope of the law of value in socialism. The Yugoslav economic plan determines only such quantities as the sum total of disposable income and the sum total of gross investment for the economy as a whole and for each industry, while the market mechanism is expected to establish the equilibrium relationship $Yn = d\Sigma n$ within each industry, i.e. the quality, quantity, and price of each product.

Vrancić's book suffers from the same shortcomings as the majority of other studies written by East European economists. Their adherence to the classical concept of value renders their work lacking with respect to a valid analytical apparatus. Yet Vrancić's book deserves our attention because it is a serious attempt to prove—on the basis of purely Marxian analysis—that centralized economic planning which goes further than planning the aggregates is contrary to the law of value of Karl Marx and that, as long as the element of scarcity in the production of material goods is present, microeconomic decisions must be left to the market mechanism.

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

The Flow-of-Funds Approach to Social Accounting: Appraisals, Analysis and Applications. Studies in Income and Wealth, Vol. 26. Princeton; Princeton University Press for the National Bureau of Economic Research, 1962. Pp. x, 487. $10.00.

This volume is in four parts. Part I, entitled "The Integration of Social Accounting Systems," contains three essays: "An Approach to the Integration of Income and Product and Flow-of-Funds National Accounting Systems: A Progress Report," by Stanley J. Sigel; "The Canadian National Transactions Accounts," by S. J. Handfield-Jones; and "Financial Accounts in Countries Other Than the United States and Canada," by Graeme S. Dorrance. Part II, which carries the heading "Analysis and Applications of Flow-of-Funds Data," has four essays in it: "A Process Approach to Flow-of-Funds Analysis," by James S. Duesenberry; "Some Illustrative Analytical Uses of Flow-of-Funds Data," by Morris A. Copeland; "The Impact of Severe Monetary Restraint on Money Flows," by Emanuel T. Weiler; and "Applications of Flow-of-Funds Data to Capital Market Analysis," by James J. O'Leary. And Part III, which is called "Sources of Information and Technical Problems," is made up of five essays: "Institutional Data as a Source of New Information for Use in Social Accounting Systems," by Irwin Friend; "Small and Unincorporated Businesses as a Source of Information for Social Accounting Purposes," by Daniel H. Brill; "Consumer Surveys as a Source of Information for Social Accounting: The Problems," by Arthur L. Broida; "Consumer Surveys as a Source of Information for Social Accounting: The Prospects," by Robert Ferber; "The Optimum of Grossness in Flow-of-Funds

Accounts," by Morris Mendelson; and "The Float in Flow-of-Funds Accounts," by George Garvy. Part IV, which concludes this Conference on Research in Income and Wealth volume, contains two brief "General Comments," one by John C. Dawson and one by John S. Atlee. There are finally the usual specific comments; in time-honored conference tradition, each of the essays is followed by a "Comment" or two or three. (I should perhaps note in passing that some of these comments—for example, those by Read, Taylor, and Tobin—are quite as valuable as any of the essays.)

Sigel is mainly occupied in his lengthy essay with presenting a unified or integrated set of national accounts—a single set of accounts from which both the national income and product and the flow-of-funds accounts can be derived. His essay would thus seem to be a testimonial to U.S. affluence, for the Handfield-Jones and Dorrance essays indicate that no country has two sets of independently developed national accounts, nor a need to reconcile or integrate accounts which, as Natrella, speaking of ours, says in his Introduction "touch at many places with what appear to be resounding clashes." Not everyone is agreed, however, that Sigel has found the best way of integrating our two sets of accounts. Possibly his format is too imperialistic for some—too much, that is, like the present flow-of-funds format. One cannot be sure, but certainly I got the impression when reading Part I of this volume that integrating our "rival" accounts is more than an intellectual problem.

If one of the purposes of Part II of this book, "Analysis and Applications of Flow-of-Funds Data," was to demonstrate the usefulness of these accounts, it will strike some, I think, as a failure. Not that the papers it contains are uninteresting. Quite the contrary. Duesenberry's, for example, is rich in plausible hypotheses. But then he makes explicit use of flow-of-funds data only once (in an appendix). Weiler's paper and Gurley's discussion of it are devoted to discerning cyclical financial patterns, although to what end is not clear. For instance, Weiler's problem is to determine whether the financial patterns of high interest rate periods differ from those of low interest rate periods. Yet he is not clear how his concern bears on the issue of monetary policy's effectiveness, so one can reasonably confront his effort, and to a lesser extent Gurley's as well, with an inevitably (and unfortunately) pungent "So what?" Does it make a difference for economic welfare that, say, the amount of financial intermediation varies cyclically? It can be argued that understanding the financial process, and particularly the short-run adjustment process, which no doubt is greatly aided by the availability of flow-of-funds data as well as by papers like those contained in Part II of this Conference volume, is in and of itself worthwhile. Still, I would have been happier with a statement by Federal Reserve people explaining why the System finds flow-of-funds tables valuable and worth what it cost to develop them.

Part III will greatly interest economists occupied with generating numbers. The essays by Friend, Brill, Broida, and Ferber—all of which struck me, a novice, as extremely informative—and the comments by Lindow, McHugh, and Morgan, will be of particular interest to those who struggle to provide data on the consumer sector of the economy. Actually, they should be of even greater interest to empirical researchers who test hypotheses with other economists' numbers. Reading them, one gets a healthier regard for the printed number. (When a hypothesis does not fit the data, should it or the data be rejected?) And similarly for Garvy's valuable essay on float; it too gives another perspective on the numbers which when all done up in print look so respectable and reliable. Mendelson, in his essay, the remaining one of Part III, is as much concerned with how flow-of-funds accounts should be used as with problems of data collection.

It is fitting, I think, that this book concludes as it does with brief notes by Dawson and Atlee, for the question to which they address themselves—namely, "Now that we have the flow-of-funds accounts, what do we do with them?"—is one asked, if in different ways, by many other contributors. That their answers are unconvincing is too bad, for presumably the underlying issue is of some importance.

What do flow-of-funds totals and subtotals mean, if anything? And of what possible significance can all the splendid detail of flow-of-funds accounts be for the making of economic policy? These are the questions implicit in the painful, oft-repeated lament "Where oh where is the Keynes of flow-of-funds?"

For myself, I am not overly concerned that flow-of-funds accounting will turn out to have been a monstrous hoax. Granted, the tide does seem to be running with the quantity theorists these days, but this may be in large part because they are, to a far greater extent than their rivals, actually doing empirical work. Nor can I be comforted by those who, in defending flow-of-funds accounting or directly attacking the quantity theorists, take refuge in that wonderfully meaningless word "liquidity." No, I shall wait uncomfortably until my view—that effective policy formulation requires an understanding of the path by which our economy adjusts to a change in monetary variables—begins getting some empirical support. And some of this empirical support is to be found, I suspect, in flow-of-funds tables and, more importantly, in sector balance sheets.

John Kareken

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Free Reserves and the Money Supply. By A. James Meigs. Chicago: University of Chicago Press, 1962. Pp. xvi, 118. $4.00.

Financial writers frequently regard the amount of "free reserves"—reserve balances of member banks at the Federal Reserve banks in excess of legal requirements minus their borrowings from the Reserve banks—as the most important indicator of Federal Reserve policy and of credit conditions.[1] It is the concept of the role of free reserves—its origin, its meaning with respect to the money supply, and its validity—that James Meigs examines in this book.

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[1] For example, Edward T. O'Toole in *The New York Times*, October 21, 1962, section 3, p. 1: "The level of free reserves in the commercial banking system—a fairly reliable barometer of credit conditions over the long term—has been well above $400,000,000 on average for the last two years. This means there has been plenty of money available in the banking system since November, 1960, for those who would borrow it."

Meigs traces the emphasis on free reserves to the 1920's when Federal Reserve officials, after discovering that member banks often tended to offset open-market operations by changing the amount of their indebtedness to Federal Reserve banks, developed what is called "reserve position theory . . . as the rationale for attempting to control member-bank behavior through influencing the proportion of borrowed reserves in total reserves" (p. 2). More explicitly, the hypothesis Meigs examines is a modified reserve-position theory, namely, "that banks seek to maintain certain desired ratios of excess reserves and borrowings (or free reserves) to total deposits and that these desired ratios are functionally related to market interest rates and the discount rate. In attempting to adjust the actual reserve ratios to the desired ratios, the banks increase or decrease their holdings of earning assets, thus causing deposits to change" (pp. 3-4). His study includes an examination of the correlative "proposition that when free reserves are large the rate of growth of deposits tends to be high and that when free reserves are negative, or 'net borrowed,' the rate of change of deposits is small or negative" (p. 66), and of its implication that the central bank, in attempting to control the money supply, should focus its attention on free reserves rather than on total reserves.

Meigs subjects this hypothesis and correlative propositions to a sophisticated statistical analysis covering the years 1947-58. He concludes that while the theory helps to explain the reaction of member banks to prevailing interest rates and Federal Reserve discount policy and open-market operations, it does not provide a good clue to the character of Federal Reserve policy and is not the best focal point for Federal Reserve open-market operations. He concludes, on the contrary, "that use of total member-bank reserves as a proximate goal for open-market operations would afford a more precise control over the rate of bank-deposit expansion or contraction than would use of free reserves" (p. 1). Likewise, he implies that commentators on Federal Reserve policy would be more discerning if they concentrated their attention on changes in total member-bank reserves rather than free reserves.

All students of monetary policy will be greatly indebted to Meigs for his painstaking and elaborate analysis of the hypothesis which he examines, and for his conclusion regarding its degree of conformance with the factual record, which seems to the reviewer to be unexceptionable. However, it may be suggested that he might well have examined more critically the basic logic of the modified reserve position theory that he tests statistically. In particular, the second of his twin assumptions about what banks seek to maintain may be questioned. The first, that they seek to maintain certain desired ratios of excess reserves to total deposits, seems reasonable in view of the fact that excess reserves are the member banks' free cash available for meeting the adverse balances of payments that frequently occur in connection with the ebb and flow of funds. (Banks can, of course, dip into their required reserves for such payments, but over comparatively short periods of time must have average reserve holdings equal to or exceeding the required percentages.) But that the banks seek to maintain certain desired ratios of borrowings to total deposits seems incredible. Isn't it much more reasonable to suppose that under changing interest-rate conditions, prevailing Federal Reserve bank attitudes, and member-bank traditions about borrowing, member banks at some times are much more willing to borrow from the Federal Reserve than at others, and that for particular banks there is far more necessity or pressure to make such borrowings at some times than at others? Desired ratios of borrowings to total deposits, and hence of free reserves to total deposits, would seem to be highly changeable and not something that the banks seek to maintain. Consequently, Meigs' "principal conclusion . . . that what really matters is the difference between *actual* free reserves and their *desired* free reserves" (p. 87) has comparatively little explanatory significance with respect to member-bank behavior.

CLARK WARBURTON

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Theory of Money. By W. T. NEWLYN. Oxford: Clarendon Press, 1962. Pp. vi, 171. $2.90.

This is a brief review of monetary theory in a Keynesian framework and a British setting. It misses the U.S. market, lacking the sophistication and breadth needed for graduate students and being too abstract for undergraduates. The author acknowledges the influence of the Radcliffe Report and has put emphasis on the problem of money substitutes and the great limitations of monetary policy. The book largely ignores developments outside Britain and so should provide students an interesting contrast to Patinkin's treatment; it may also help to prepare them for the point of view, hard at first to pin down, of the Radcliffe Report.

Three introductory chapters on the demand for money guide beginners to the heart of the book. These begin with transactions demand à la Angell and Ellis (a good presentation of this neglected topic) and proceed directly to liquidity preference and then the question of money substitutes. A subsequent chapter on the rate of interest describes the place of liquidity preference in the Keynesian determination of interest and income. A slim chapter on foreign exchange then sets the stage for a theoretical discussion of monetary policy. The author is slightly less pessimistic than the Radcliffe Report about the usefulness of monetary measures.

As a brief review of selected areas of monetary theory, the book is fairly successful. Its chief shortcomings are omissions of competing theoretical developments and empirical tests. Although some data are presented, they are of questionable relevance, and the conclusions are overconfidently based on a limited and largely unexamined empirical foundation. This is more serious today than ever. In the life of every topic in economics there is a time for theoretical analysis and a time for empirical work to test the proliferating hypotheses. Surely, in the monetary area, theory has monopolized attention too long, and statistical testing is long overdue.

The lack of an empirical outlook leads to annoying dictums like (p. 78): "The variability in the velocity of circulation of bank money is the most effective refutation of the quantity theory of money." On this level of sophistication one could also say that the Keynesian system falls to the ground because the marginal propensity to consume is observed to change over time. Is it not relevant how great is the variability compared with alternative systems and whether it can be explained within the framework of the theory?

The author also treads uncautiously where careful empirical probing should go first. He argues that the speculative demand for money imparts a high interest-elasticity to the total demand curve, because small movements above or below the expected "normal" rate bring massive shifts of funds into and out of bonds, respectively. Such an expectational theory is troublesome, however, for two reasons. First, a persistent movement of the interest rate away from the "normal" level will gradually alter expectations. Hence the entire schedule showing the speculative demand for money itself depends on what happens to the interest rate; it cannot therefore be used in the analysis of comparative statics. Second, even if we use it only for dynamic analysis, it is based on the special hypothesis that the "normal" rate is estimated by past *levels* of the rate. This is plausible, but so are many alternatives—for example, that any changes in the rate are expected to continue. The question basically is, "If the rate changes, will speculators view this as a temporary movement soon to be reversed, or the beginning of a larger, permanent movement in the same direction?" The two responses give quite different results. It is odd that elsewhere (p. 136) changes in the foreign exchange rate are viewed as often, or frequently, destabilizing; whereas the speculative demand for money is treated, in these terms, as stabilizing the interest rate. In reality, expectations are no doubt sometimes stabilizing and sometimes destabilizing. A priori theory here is apt to lead us far off the track. How much better it would be to tell the student we do not really know which of the important alternatives is correct, and to discuss instead how we might go about finding out.

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Public Finance; Fiscal Policy

Techniques of Treasury Debt Management. By TILFORD G. GAINES. New York: Free Press of Glencoe, 1962. Pp. xii, 317. $7.50.

This is the first of a series of doctoral dissertations to be published by the Graduate School of Business at Columbia University, and it sets a high standard. The book falls into two parts: (a) a brief historical survey of debt management in the United States since the Civil War; and (b) postwar problems of debt management. The historical portion of the book is neither persuasive nor interesting. The record of debt management from the Civil War through 1916 was "quite satisfactory." The records of Secretary Mellon during the 1920's and Secretary Morganthau during the 1930's (and even during World War II) are praised. Gaines is even mildly apologetic concerning the financing done by Secretary Chase during the Civil War and that done by Secretary McAdoo during World War I. These charitable judgments can be reached only by suspending the critical standards used by Gaines in his appraisal of the years 1946-57. But the pages devoted to the pre- 1946 years are few. The rest of the book presents the results of instructive research in a field where contributions have been rare.

In his examination of postwar debt management—really the years since 1951—Gaines argues that demonetization and lengthening of the public debt are not "appropriate or meaningful objectives of Treasury debt management" (p. 3). Responsibility for policies to regulate monetization of financial assets (of which federal securities are one type) should rest with the Federal Reserve. Changes in maturity structure, as attempted by the Treasury in the postwar period, have been disturbing to capital markets and ineffective in accomplishment. The first task of the Treasury should be to "direct its operations toward preventing further changes in the maturity structure of the debt." Until this neutral objective is achieved, more ambitious objectives should be eschewed.

In pursuit of the aim of avoiding monetization of the federal debt, the Treasury has limited subscriptions, made preferential allotments, and, in general, discriminated against the commercial banks. But restrictive conditions imposed by the Treasury when securities are issued cannot determine where the securities will come to rest. In a free market the Treasury can have little control over the ownership of the federal debt (except for debt in its trust accounts). Attempts at debt monetization are, therefore, "pointless" and they may interfere with the monetary policies of the Federal Reserve.

Changes in the maturity structure of the federal debt have important effects "upon the availability of funds at different maturities, upon the structure (and perhaps the level) of interest rates, and, in a setting of excess liquidity such as has characterized the postwar years, upon the income velocity of money" (pp. 251-2). Lengthening or shortening the debt is not an objective in itself; it is "the *means* employed to pre-empt or supply capital funds, to reduce or increase income velocity, to lessen or add to the liquidity of financial assets" (p. 252). An important shift in the maturity structure of federal debt requires either release or absorption of funds elsewhere in the relevant maturity sectors. Unless moves by the Treasury take account of Federal Reserve policy, the result will be contradictory pressures on the money market. In the postwar period this has often happened. The Treasury has been unable to market longer-term securities in periods of recession. Both courses of action were in conflict with Federal Reserve policy.

Why has the Treasury been unable to sell more long-term bonds? Because its techniques have been defective. It has sold irregularly and in large blocks, thereby failing to gear sale to the rate of accrual of long-term funds. Another fault has been that longer-term debt was replaced as it reached maturity—a mistaken method since current holders of maturing long-term bonds are unlikely to be interested in new long-term bonds. Gaines believes that the Treasury should routinize its financing in order to reduce uncertainty concerning the maturities it will offer; it should make the size of its offerings at any time consistent with the absorptive capacity of the money market. In short, the key objective of present debt management should be "orderliness" (p. 298).

In a perceptive preface, Robert V. Roosa, Undersecretary of the Treasury, suggests that this recommendation is inconsistent with development of imaginative programs required by the newer theories of debt management. Gaines would, I presume, retort that use of debt management as an instrument for economic policy must rest upon prior development of techniques which will bring the debt under the administrative control of the Treasury. Theories have run ahead of management procedures. After orderliness has been achieved, debt management may be developed into a significant instrument of economic policy.

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Offentlig økonomikk. By LEIF JOHANSEN. Oslo: Universitetsforlaget, 1962. Pp. ix, 108.

This monograph, written in Norwegian, is based on a series of lectures to students of economics at the University of Oslo. Its emphasis is on the use of theory in dealing with problems of fiscal and monetary policy. According to its author, it does not put particular weight on Norwegian experience.

Johansen begins by pointing out that in most nations, Norway as well, government's role in economic life has increased in importance if for no other reason than simply because the proportion of public expenditures to total national product is greater than it was a half century ago. Whether planned or not, government today has great influence on the course of the economy. He proceeds then in Chapter 2 to inquire into the ways and means by which the government may, through its economic policies, contribute to public goals such as full employment, economic growth, and the level of prices. In distinguishing policy means available to the public sector for achieving the goals of the community, he sets up a series of implicit functions relating appropriate means to particular ends. Johansen points out that in this way contradictions and/or inadequacies between ends and means are explicitly revealed. Some means are more effective than others in relation to the objective sought, but the matter is complicated by the fact that a given means may influence more than one objective of society. This often, then, requires centralized control over the means of economic policy, a point which he discusses at some length. This chapter may be of special interest to U.S. readers because of the effective and uninhibited statement of the ends-means dichotomy. Norway, it may be suggested, has gone beyond the United States not only in experience with government economic policy but also in public and professional acceptance of the management of the public sector to attain widely accepted goals of the economy.

Chapter 3, though it is the heart of the work, contains little if anything that is not generally available in any standard English language text in public finance or macrotheory. Johansen develops an orthodox, highly aggregated income-expenditure model to illustrate the role of the public sector and the impact of changes in tax and expenditure parameters, first in a closed and then in an open economy. Static multipliers indicate the impact on total output of changes in the level of government expenditure, in direct and indirect tax rates, and of alterations in the degree of liquidity in the private sector owing to shifts in the government's fiscal position. The author deals first with a model of a closed economy in which prices are stable, and then proceeds to the case of an open economy.

In the latter part of Chapter 3 the case of a variable price level is incorporated within the closed economy model. Special attention is given to upward pressures on the price level under conditions of high level resource utilization. Johansen emphasizes that if reasonable price stability is an objective of society, then discretionary increases in tax rates should only be used as a relatively short-run expedient. Their use as a means over an extended time period may be self-defeating by bringing the ends of price stability and high employment into conflict. Upward pressures on the price level can be satisfactorily met in the long run only from the supply side by increasing factor supplies.

In Chapter 4 Johansen discusses public debt, considering various kinds of debt, public attitudes toward debt, the effects of the debt on income distribution, and its long-run effects on economic growth. The treatment, though clear and careful, is standard.

Chapter 5 briefly discusses the state budget and state accounting. U.S. readers might wish for more illumination about the state's economic budget, particularly about its preparation and the political process by which its objectives are given parliamentary approval. But the book was not written for U.S. readers.

The work may not be easy for those for whom Norwegian is a second language. It roams over contemporary technical usages, the "new" Norwegian, and frequently lapses into language usages of the past. (A typographical deficiency is that the type face of several symbols changes so that it is uncertain whether a new notation is being used or not. See pp. 36-37, 42-43, and 53).

EDWARD J. CHAMBERS
ROBERT A. NELSON

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Reappraisal of Business Taxation. Symposium Conducted by the Tax Institute, December 7-8, 1961. By H. M. GROVES, S. BARKIN, AND OTHERS. Princeton: Tax Institute, 1962. Pp. xii, 242. $6.00.

This symposium examines the subject matter in five parts, all except the second inclusive of varied viewpoints. In Part I two economists, a labor union research director, and a corporate attorney consider the role of business taxation. In Part II a tax practitioner examines business forms in relation to federal income taxation. In Part III a National Association of Manufacturers' tax counsel, a state tax researcher, a university man, and an active state tax administrator examine with very little overlapping some business tax problems of states and local governments. In Part IV two manufacturers' representatives, two professors, and a member of the Committee on Ways and Means take a look at tax policies in relation to investment and other aspects of economic growth. In the final section an economist and three tax practitioners consider the reconstruction of business tax policies.

The volume, though addressed in part to professional tax people, offers on the whole a remarkably good opportunity for the economist who is not a tax specialist to review the business tax issues most in the limelight during the past year. This coverage results from the careful planning which is antecedent to such a presentation as well as from the individual speakers' effective popularization of most of the technical subject matter. With few exceptions, the papers in Parts IV and V are more incisive than the earlier ones;, but few of the papers suffer from the carelessness sometimes characteristic of symposium presentations.

Perhaps it is significant that in the discussion of reform most of the speakers conclude that no over-all reconstitution in their particular areas is required. This is especially arresting in two of the best papers, one by Professor E. Gordon Keith on "Flat Rate Corporation Tax versus Present Tax" and the other by Mr. Norman A. Sugarman on "Business Income of Exempt Organizations." (The latter is somewhat short of a categorical conclusion, but the implications seem clear.)

In fact, from the volume as a whole one gains the impression that, although the speakers on various phases of the tax program believe the federal government confronts bristling difficulties of a specific character, they also think the general outline of policy is satisfactory. Thus, not only do the over-all rate structure and "charitable" exemption policy seem to meet reserved approval, as exemplified by the Keith and Sugarman papers, but corporation income tax policy in general appears acceptable to a majority of the speakers. Nearly all symposium members imply the wisdom of working on particular details. Professor Lowell Harriss, presiding at the first session, remarked that "I would not . . . support our present system of taxing business. . . . I constantly criticize our present system of taxing business. Or can we call it a system?" However, even Harriss also comments that "Although revolutionary and sweeping changes are unlikely to be made this year or next, the clearer the goal which we would like to reach, the better will be the individual changes that can be made from time to time. The better, too, they will be as elements of a coherent, consistent, constructive program for the long run."

The business tax situation in the states, although less critically discussed, seems not to receive uniform general acquiescence. In fact, there appears to be some question as to exactly what is meant by state business taxation (e.g., p. 76). There is serious issue, too, as to federal-state relationships. Possibly state business taxation requires attention not only to technicalities but also to basic concepts.

JAMES W. MARTIN

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International Economics

Shaping the World Economy—Suggestions for an International Economy Policy. By JAN TINBERGEN. New York: Twentieth Century Fund, 1962. Pp. xvii, 330. $4.00; paper, $2.25.

Looking at the world and its problems, Professor Tinbergen finds four outstanding features: a group of countries ruled by Communist parties, a division of the world into developed and "developing" countries, a rapidly disappearing colonialism, and a tendency toward integration by some groups of nations. The first three—the divisive factors—produce political and economic tensions.

Of the problems created by these tensions, underdevelopment is the most important.[1] It is also persistent as indicated by the increasing gap in real income per capita between developed and underdeveloped countries. The solution to this problem, which involves the "basic human needs" of an enormous number of people, is mainly an economic matter in a field where much progress has been made, and it could contribute to the solution of other problems as well.

"Two doctors," to use Tinbergen's expression, offer to help underdeveloped countries overcome their poverty—the West and the Communists. Unfortunately, we cannot determine which doctor is better because we lack adequate information about relevant output—capital ratios. The Communists command a larger number of instruments of economic policy which, in general, permits them to reach their goals with greater precision than can the West with its smaller arsenal. Fewer instruments, however, give the West the advantage of simpler administration and therefore greater efficiency. Tinbergen concludes:

> An appraisal of the relative effectiveness of the two systems turns on estimates of the strength of these opposed forces: more precise guidance with perhaps less stimulus in the Communist system, less precise guidance with perhaps more stimulus in the mixed system.

In response to the oft-heard statement that the greater scope for individual freedom, implicit in the West's prescription for improving the lot of underdeveloped countries, may appear more attractive to developed nations than to some underdeveloped ones, Tinbergen pleads for an undoctrinaire approach to economic policy:

> The most important contribution the economist can make is to point out that the choice is not necessarily a choice between the extremes, rather it is one of an optimum combination of institutions and instruments in every case.

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[1] Readers interested in economic cooperation and development in specific regions will find the first four appendices useful. They are brief surveys written by other members of Tinbergen's team—Africa (F. Hartog), Middle East (A. van Oven), Southeast Asia (H. Linnemann), and Latin America (H. Rijken van Olst). They vary in approach and style but are uniformly interesting.

In his appraisal of the external economic environment of the developing countries, Tinbergen notes that, since the end of World War II, developed nations have participated in a "crusade against protection," on occasion oddly coupled with quantitative restrictions against imports from low-wage countries. Condemning the discrimination against outside countries inherent in the trade policy of the E.E.C., Tinbergen estimates that for the developing countries as a whole this unfavorable impact will be quite small. The increased productivity of the E.E.C. will make its exports cheaper and its higher income will result in greater imports so that "the outside world as a whole may not be unfavorably affected," although "some individual countries will suffer."

We must hope for a reduction in the common external tariff of the E.E.C. and for economic integration elsewhere. Praiseworthy steps toward the latter have been taken by the Economic Commission for Latin American in a "remarkable example of continental thinking."

Next Tinbergen examines the structure of world trade. Admittedly crude econometric studies (described in detail in an appendix) support the conclusion that, in general, the "normal," "theoretical" or "ideal" trade flow between two countries is proportional to the gross national products of the countries and inversely proportional to the distance between them.

Comparing actual trade with the computed "normal," Tinbergen found that only the group of large industrial countries traded with each other at levels persistently below "normal." Having found a proportionate relationship between size and trade, despite the expectation that greater size would lead to a somewhat smaller relative volume of international trade because of the greater diversity of larger economics, he assigns much responsibility for the subnormal trade of these nations to their restrictive trade practices.

For the smaller industrial countries, including Japan, the deviations of actual trade from computed trade are not large enough to warrant the inference that they are adversely affected by trade restrictions. Tinbergen also reports: "We do not find clear examples of trade victims among developing countries either." Then, cautioning that more research is needed, he states: "For what our conclusions may be worth, it follows that trade policies should not be overemphasized as an instrument in development; thus we do not think the slogan 'Trade, not aid' a proper guide to action."

After commenting briefly on the problems created by fluctuations in income and in the prices of primary products, Tinbergen concludes his survey of the world economy with a series of vignettes on some of the leading commodity markets. He decries the slow progress in concluding international commodity agreements.

The interrelationship of the tensions, and their effects upon the world today, call for policies shaped by national interest broadly conceived. Tinbergen asserts "that we can no longer afford the luxury of national autonomy." He would seek agreement on a one-world economic policy including the Communists or, failing that, a common policy for the non-Communist countries as a whole.

Policy goals should be set by the United Nations. They should include: (1) development targets for national income over a period of five years, (2) equilibrium in balances of payments, (3) price stability, and (4) income distribution targets which call for convergence of real incomes per capita.

Production in developing nations must be increased by at least two per cent and preferably four per cent per annum more than it is now rising if we are to achieve the goal of converging incomes per capita. A two per cent increase in production would require $7 billion to $8 billion more investment than now takes place in these countries. It is about double the amount they now get from developed countries. We should not be deterred by the low short-run absorptive capacity of some areas but should seek to increase that capacity as rapidly as possible.

Referring to trade policy as "a monster with a multitude of dimensions or degrees of freedom," Tinbergen, as might be suspected, advocates reduction in trade barriers except when their temporary imposition will aid economic development. He views a world of trade areas as desirable. Internal trade and development will be encouraged. If, subsequently, external trade restrictions are also reduced, widespread benefits will result. For territories associated with the E.E.C. and likely to suffer from reductions in the common external tariff, Tinbergen recommends an interesting program of investment outlays to stimulate establishment of businesses suited to the newer competitive conditions.

The international effects of domestic monetary and financial policies are so great that "a considerable degree of centralization in the world's (or at least the non-Communist world's) financial and monetary policies is indispensable." At this time, Tinbergen supports the Triffin Plan as appropriate in a "natural progression" of monetary systems. He hopes that in due course the central bank managers will accept J. E. Meade's proposals, which include introduction of floating exchange rates.

This long review of a relatively short book is partly a consequence of the wealth of information and ideas compressed in its few pages. And partly, it reflects this reviewer's pleasure from reading a sophisticated work of political economy. Having clearly revealed his value preferences, Tinbergen has asked important questions and given the answers of a disciplined economist.

Certainly there will be much disagreement with some of Tinbergen's policy recommendations. His vision is broad and his approach bold. This alone will no doubt stimulate criticism and discussion by widely disparate groups. However, it will not be surprising if they agree that fuzzy, emotion-eroded reasoning played no part in leading Tinbergen to his positions on policy.

As the work of an excellently qualified advocate of an undoctrinaire approach to world economic problems, this book deserves widespread reading.

WYTZE GORTER

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U.S. Private and Government Investment Abroad. Edited by RAYMOND F. MIKESELL. Eugene: University of Oregon Books, 1962. Pp. xiv, 599. $10.00.

This volume is a collection of essays by seven authors, including the editor, dealing with U.S. private and government investment abroad and the analytical and policy problems and issues which they pose. It is the most comprehensive survey of this subject that has yet appeared and is, therefore, likely to be regarded as a principal reference source. The study contains nine chapters by Mikesell (about one-third of the book), which both introduce and conclude the volume, as well as dealing with a range of statistical, analytical, and policy issues; five chapters by J. N. Behrman on the nature, effects, and methods of promotion of direct private foreign investment (another one-quarter of the book); and one chapter each by A. G. Gols on postwar U.S. foreign petroleum investment, P. Meek on U.S. portfolio investment in foreign securities, D. C. North on historical statistics and trends in world capital movements, primarily before 1914, P. B. Simpson on a theoretical analysis of foreign investment and national economic advantage, and D. A. Wells on an economic analysis of attitudes of host countries towards direct private investment.

The complexity of the subject matter should be readily apparent. It ranges over the whole of economics and involves political as well as economic issues. This is political economy in the grand style. Despite the range of topics covered, the book is not exhaustive, as the authors themselves recognize (p. 8). But it certainly touches on all of the relevant questions and, in this respect, could be characterized as an agenda for the study of the problems of public and private investment. In view of the range of topics covered, I shall single out only a few matters that have struck me as of particular interest.

The chapters contributed by Mikesell are especially notable for their intimate knowledge of current developments and institutional arrangements, revealing, undoubtedly, Mikesell's active role as a consultant to the U.S. government and to several international organizations. There is much good sense in his judgments as to the feasibility of alternative courses of policy action. The most original contribution and, in my judgment, the one most likely to be cited from this volume, is that of Behrman, who summarizes and analyzes the results of an elaborate questionnaire and interview procedure under which U.S. firms operating abroad reported on various aspects of their foreign activities. This represents an important addition to the work already undertaken by the Commerce Department, and casts light upon many aspects of the foreign operations of U.S. firms. The chapter by Meek on the revival of portfolio securities trading in the United States since the war also seems quite informative.

There are some matters of policy that are touched upon in this volume, but not, in my opinion, adequately explored. Mikesell states (p. 7) that the analysis emphasizes the importance of policies favoring an increase in direct investment to poor countries of the free world. And Behrman (p. 171) asserts, among other virtues of private direct investment abroad, that it will lead to political stability which will, in turn, lead to independence of host countries from the Sino-Soviet bloc. I find little in the volume to support these assertions; they may be correct, but they have not been argued. The omission is curious in the light of the fact that there is rather sophisticated discussion of the politics of public lending—for example, in a discussion by Mikesell of how the IBRD and IMF became enmeshed in the local politics of Bolivia and Argentina.

A second point of policy that is of some interest is the proposal by Behrman that American firms operating abroad gradually transfer ownership to nationals. The idea is gradually to separate the transfer of organization, enterprise, and skills which accompany direct investment abroad from ownership, the latter being the principal point of contention with host countries and their nationals. The suggestion is an interesting one, but one wonders about its feasibility. If one were to consider the various reasons for engaging in subsidiary and branch plant activities abroad, it seems to me highly doubtful that such a scheme would commend itself readily to many American firms.

While the volume contains a wealth of useful materials, it is quite evident that much more work on the problems raised remains to be done. We need much better statistics on international investment variously defined and its relationship to other economic magnitudes. We also need much more information about international firms and their activities. The volume under review is, nonetheless, an important contribution to the literature on international investment in the postwar period.

BERNARD GOODMAN

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The United States and the Common Market—A Background Study. By DON D. HUMPHREY. New York: Frederick A. Praeger, 1962. Pp. xvi, 176. $4.50.

Economic relations between the United States and the European Economic Community are likely to generate front-page news for a long time to come. The failure of Britain's bid for admission has cast a long shadow over prospects for an outward-looking Community and has crippled President Kennedy's authority to negotiate lower duties by almost eliminating most items on the 80 per cent dominant-supplier list. The high hopes that attended the passage of the Trade Expansion Act last year must now give way to a sober reappraisal of the Community's willingness to undertake a massive reduction or removal of external tariffs. At the very least, it now appears that tariff negotiations with the Common Market are going to be much more difficult (and probably less fruitful) than most of us expected. Very little is likely to come out of such negotiations unless the United States is willing to make concessions that exceed any in the past.

Professor Humphrey (with the assistance of William P. Kelley, Jr.) has written a small book that provides a great deal of the background essential to an understanding of U.S. trade policy. The title is somewhat misleading because only the first of twelve chapters focuses on Western Europe although references to the Common Market are made here and there throughout the book. The strength of the book lies in its clear, succinct statement and analysis of various aspects of U.S. trade policy. Humphrey has succeeded in stripping away the excess verbiage that characterizes too many discussions of policy issues by sticking to the essentials only. The result is a book that will please students because of its brevity, and instructors because of its hard core of fact and analysis.

The author manages to touch upon all the major aspects of U.S. trade policy: past tariff reductions under the Reciprocal Trade Agreements Act and their effects on trade, characteristics of protected industries, adjustment, balance of payments, wages, "buy American," agriculture, escape clause, peril points, etc. More than once Humphrey makes observations on topics that are often neglected. For example, he points out that the smaller the value added by manufacture in relation to the cost of materials, the higher the protection provided by a given duty. Although we may quarrel with his use of "incidence" to describe this relationship, the concept is helpful when comparing duties that protect different industries.

When discussing adjustments to foreign competition, Humphrey asserts that the first prerequisite is a gradual reduction of tariffs. Later on he uses as an example a 50 per cent tariff reduction for 155 manufacturing industries spread over a ten-year period. In the opinion of this writer, ten years is much too gradual. Probably Humphrey would agree that the Common Market has demonstrated both the practicality and desirability of rapid reductions (50 per cent in 3½ years). It can be argued, of course, that rapid economic growth has made it easier for European producers to adjust than for U.S. producers operating in a far less buoyant economy. On the other hand, adjustments in the Common Market have been of a magnitude far greater than those likely to be imposed on U.S. producers by lower duties. Moreover, substantial tariff reductions within a period of five years or so would probably induce a more energetic adaptation by affected industries than more gradual reductions.

Humphrey properly views the Common Market as an instrument for political integration, but his position on British entry is ambiguous. He says that membership in the Common Market is unlikely to restore the British economy "except as competition stimulates massive labor-saving investments" and Britain could get the benefit of this "cold shower of competition" by negotiating free trade on the pattern now proposed by the United States. It seems to me that Britain tried something like the latter in her unsuccessful negotiations with the Common Market in 1958. More importantly, it is difficult to see how the political integration of a little Europe that excludes Britain can promote the grand design of an Atlantic partnership that President Kennedy talked about so eloquently in his Fourth of July speech.

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Regional Payments Mechanisms: The Case of Puerto Rico. By JAMES C. INGRAM. Chapel Hill: University of North Carolina Press, 1962. Pp. vii, 152. $4.00.

This short, interesting book deals with a number of topics related to balance of payments adjustment in Puerto Rico. In order, the subjects are estimates of the flow of funds, 1958-60; the process of adjustment to a large capital inflow; a proposal for financial integration of common markets suggested by the island's experience; and some lessons for other underdeveloped regions.

Nearly the first third of the book is devoted to a statement of the region's gross flow of funds and the sources of the estimates. Professor Ingram argues that the estimates for Puerto Rico are superior to the statements of the flow of funds among Federal Reserve districts because balance of payments data exist which allow a classification of moneyflows into types of transactions. The data are used to illustrate some of the financial characteristics of Puerto Rico: the sharp rise in moneyflows, the overwhelming importance of external transactions, the autonomous nature of Federal government transfers, and the falling share of commercial transactions.

The bulk of the work is an examination of the process of adjustment to the large and growing capital inflow. While Puerto Rico is a part of the United States, the theory of balance of payments adjustment can be used by treating Puerto Rico as a separate nation whose reserves are demand balances in New York banks and whose exchange rate is fixed at par with the U.S. dollar. Ingram uses both the classical gold-standard transfer model and the income approach. The monetary model presents difficulties in defining and measuring the money supply and bank reserves as well as in interpreting sectoral price changes. Although prices rose more for domestic goods than for exports and more for exports than for imports (as predicted by the theory), Ingram rightly suggests that this may simply reflect U.S. experience where the price for services (i.e., domestic goods) has risen more than for staples.

The income approach is given much more extended treatment. Balance of payments and national income data for the region exist; they are used to derive least-squares estimates of marginal propensities. These propensities are then used in multiplier models where changes in the capacity to export as well as domestic investment are determinants of income and imports. In one model the change in exports and domestic investment is viewed as autonomous; in another an output-capital ratio is used to derive the changes in export capacity. By and large the projections resulting from both models fit the actual income and import data well enough for Ingram to conclude that (a) income theory is satisfactory to explain Puerto Rican adjustments, and (b) international balance of payments theory can be used to explain interregional adjustments. I have no quarrel with these conclusions. It should be noted that the marginal propensities are computed from 1947-60 data and the projected series derived from the models cover the same period. The income models are thus used as "good explainers of data to date, and one is uncertain about whether they will continue to explain future data" in Carl Christ's phrase [1, p. 402].

Ingram's discussion of the implications of this case study for international monetary policy is extremely interesting. Because of the thorough integration of Puerto Rico's financial market with that of the United States, instability and panic movements of funds do not occur. Puerto Ricans hold many U.S. assets that are readily salable; Puerto Rican financial assets are readily marketable in the United States. Hence reserves are readily replenished and interruptions of payments do not occur. A further factor is that Puerto Rico does not pursue an independent monetary policy. Unlike the typical international case, foreign exchange reserves are not a sharply separate type of asset: a wide variety of claims may be sold to obtain foreign exchange. The Puerto Rican experience is generalized with the proposition that schemes to increase the size of foreign exchange reserves (e.g., the Triffin plan) would be less effective in minimizing payments pressures than would the integration of long-term assets markets. Removing obstacles to transactions in longer-term assets, of course, gives similar interest rates in the countries concerned. This idea, which is called a "one price for claims" proposal, has particular application to highly integrated groups on fixed exchange rates. The whole idea is spelled out in much greater detail in Ingram's paper in the Joint Economic Committee compilation [2], which should be read with this section of the book. The discussion may be regarded as working out the implications for one particular market of the general proposition that with fixed exchange rates the domestic economy must adjust to the international.

In some final comments about lessons for other underdeveloped countries to be drawn from Puerto Rican experience, Ingram states that the alleged advantages of the island's unique ties to the United States are usually overstressed. They are relative, not unique, for other countries could approach Puerto Rico's policies if they would—but they are warned that Puerto Rico's experience gives only a pessimistic outlook. Little domestic saving has been induced and in spite of the huge capital inflow, employment has actually fallen. Countries without an outlet for surplus population may have more trouble than they expect in absorbing labor via more investment, particularly in view of the fact that with rising incomes the Puerto Rican population did not stop growing.

Altogether, this is a stimulating monograph.

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International Economic Instability—The Experience After World War II. By JOSEPH D. COPPOCK. New York: McGraw-Hill Book Co., 1962. Pp. xiv, 184. $7.95.

Since 1945, UN-affiliated organizations have assembled a vast array of international statistics. Their availability has spurred intensive empirical investigations both of the strategic movements in the world economy and of the international economic performance of individual nations. It becomes possible to employ a "global approach" to study specific aspects of international economic relations. Coppock's work on international economic instability illustrates this approach.

In this statistical treatment of short-term fluctuations in the world economy for the postwar epoch 1947-58, the author attempts to define, measure, analyze, and devise policy recommendations to mitigate international economic instability. He adopts "external variable analysis" through an intensive examination of a major segment of the balance of payments of each of 83 nations as the focus of the investigation.

Coppock selects annual variations in the money value of exported goods and services as the principal indicator of international economic instability. The analysis of the invisible segment of the current account is confined to one allusion to the magnitude of world exports of services as providing "another $10 to $15 billion" in 1957 to an aggregate value of world commodity exports exceeding $100 billion (p. 32). The lack of additional explicit references to this issue leads one to conclude that Coppock's skepticism about the quality of the annual services export data precluded their use either on a global or partial basis for the analysis of short-term fluctuations in the world economy.

Moreover, I find the logical basis of the case Coppock develops for selecting current account export proceeds unconvincing. He contends that since capital imports, in most cases, give rise to subsequent capital exports, they are claims on future export proceeds and therefore, "export proceeds remain the fundamental variable" (p. 18). This formulation overlooks the fact that short-term fluctuations in the capital account are produced by a different set of forces than those operating on the current account. In short, the study of cyclical fluctuations in international capital movements is a distinct and essential aspect of the problem of international economic instability. If Coppock had merely stated that he selected commodity exports as the appropriate measure, due to the availability of a better quality and more extensive variety of data, he would have been on more solid ground.

The substance of Coppock's investigation consists of an impressive presentation of the statistical results which emerge from the processing of a vast array of commodity statistics. He constructs logarithmic variance indices to measure the degree of instability for aggregate world exports and for the major commodity and geographical breakdowns. He finds that export proceeds exhibited greater stability for primary goods than for manufactured goods between 1947 and 1958. The United States had a relatively low instability index, ranking 49th among 83 nations. He develops a useful measure of the geographic distribution of export instability by weighting each country's share of world exports by its export instability index. The United States, with 20 per cent of world exports in 1957, thus accounted for less than 17 per cent of world export stability.

Simple correlation analysis is employed to explain variations in export instability among nations. Of the forty variables analyzed, only a few, such as instability of export quantum, yield reasonably high positive correlation coefficients. One interesting finding is the rather high negative coefficients obtained for foreign trade as a per cent of GNP, foreign trade per capita, and per cent of exports going to the United States. The tendency for lower values for these variables to be associated with higher export instability values casts doubt on the effectiveness of autarchical measures employed by individual nations to insulate themselves against the uncertainties of external involvement.

Coppock is justifiably cautious about the conclusiveness of either single-factor analysis or multiple-correlation techniques in the examination of the complex character of international fluctuations. He ackowledges that "measures for reducing international economic instability do not flow from this study" (p. 144). Moreover, the measure he provides for the instability index of world export volume for 1925-38 was 7.8, substantially higher than the 4.2 for 1947-58, which suggests that the extent and character of international economic instability in the latter period was the product of unique historical circumstances.

In the concluding chapter, Coppock advocates the establishment of an International Short-Term Credit Corporation as an affiliate to the International Monetary Fund to provide small nations suffering from a deficiency in export earnings with short-term credits. Although he alluded to the connection between balance of payments disequilibrum and international economic instability (pp. 14-15), he fails to bring this relationship to bear in his analysis of the limitations of "the methods of operation" of the International Monetary Fund, an organization largely concerned with problems of payments imbalance.

Immense and challenging opportunities exist for further empirical investigations, analysis, and consideration of policy issues in the field of international economics. Viewed in this context, Coppock's formidable pioneering statistical research should be a major point of departure for the continued study of the international aspects of cyclical fluctuations and economic growth.

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United Nations and U.S. Foreign Economic Policy. By BENJAMIN HIGGINS. Homewood: Richard D. Irwin, Inc., 1962. Pp. viii, 235. $3.75.

The United States in carrying out its foreign economic policy toward the underdeveloped countries has the choice of dealing directly with individual countries through its own agencies or of using the various United Nations organizations. In practice, of course, the problem is the best combination of the two. The policy goals may be taken to be independent of the combinations of means utilized. This book investigates the relative merits of the two approaches with respect to technical and capital assistance and economic stabilization. U.S. foreign economic policy toward the developed countries is mentioned only *en passant*.

The book begins with a superficial examination of the objectives of U.S.

foreign economic policy in the underdeveloped countries. Higgins takes the objective to be pretty much what Presidents Eisenhower and Kennedy in their public speeches have said it is, i.e., to accelerate economic growth and establish stability in the underdeveloped countries. Since this, however true, is far from the whole of the matter, there follows a discussion of political and military objectives as well as such related matters as the necessity for our intervention in the internal affairs of other countries and the dangers of resentment by the governments of those countries impeding the attainment of the U.S. goals. Whatever the validity of much of this discussion, a lack of context made it unconvincing to this reader.

The most interesting part of the review of the theory of development and its relation to foreign aid is the discussion of absorptive capacity. This is "the amount of investment that can be undertaken (say, within a five-year program) without reducing the marginal contribution of the last block of capital below x." The "ideal" capital assistance program would fill the gap between maximum domestic effort plus foreign private investment and the level of investment required to bring the incremental capital-output ratio (ICOR) to $1/x$. Higgins thinks that x should perhaps equal the rate of interest at which the lending country can borrow internally. Technical assistance is primarily designed to raise the maximum domestic effort and absorptive capacity, particularly to raise the latter if it is less than the former.

The usefulness of this way of thinking about aid seems limited where the maximum domestic effort and absorptive capacity are given for a specific economic structure but where the aid program is designed to change elements of the structure, e.g., the Alliance for Progress and the agricultural systems of Latin America. Furthermore, as Higgins observes, "the limits to absorptive capacity . . . can be virtually removed if enough technical assistance is available and acceptable" (p. 28).

Having defined the goals and made a bow to theories of development, the book sets about its central task of assessing the relative efficiency of the alternative vehicles of U.S. aid. An examination of the record of each yields a capsule history of the banks, funds, departments, regional commissions, and special agencies that have been concerned with aid. In the nature of the case no generalization as to relative effectiveness emerges. On the score of "dilution of objectives" (Chap. 6), it is roughly a draw or perhaps the U.N. is the winner by a few points. The membership of Communist countries in some of the U.N. programs is held not likely to frustrate the achievement of the goals of the United States. In fact, the U.S. Congress is a greater threat to aid through U.S. agencies than the Communist countries to U.N. programs. Finally, the book compares the actual carrying out of aid policy in the field. This section is a combination of short cases drawn from the author's experience, and from other studies of aid programs, plus conclusions based on the differing character of the two groups of agencies.

The major conclusions and recommendations of the study are not novel. Their general tenor is to call for an increasing use by the United States of U.N. agencies—IDA, the Technical Assistance Program, and the IMF.

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Espace économique et intégration européenne—essai de détermination de l'incidence du Marché Commun sur les inégalités régionales de développement. By HENRI BOURGUINAT. Paris: Société d'Edition d'Enseignement Supérieur, 1962. Pp. ix, 343. NF 28.

Entire libraries already have been written in various languages on the European Common Market, and thus one would think that scarcely anything new could be added. However a reading of this book is most rewarding, for it provides many interesting insights to the background problems and development of the Common Market. Those appreciating the language will find the French is written in a beautiful and interesting style, while the subject is treated in a most stimulating manner.

Writing during the first years of the existence of the European Economic Community (1958-59), the author attempts to forecast its area impact. He takes the position that the Common Market will aggravate the regional disequilibrium with which France is plagued "in the absence of an energetic policy for the correction of regional disparities upon a European scale." The demonstration of this risk is developed throughout the work, as the author substantiates his arguments along three different levels, all based upon extensive research.

Applying the lessons of history, the author proceeds to examine past experiences of integration, partial or general, which have preceded the formation of the European Economic Community. He devotes four chapters to the accomplishments and effects of the Cobden-Chevalier treaty of 1860 between France and Great Britain, the Zollverein, the Unification of Italy, the European Coal and Steel Community, and Benelux. Bourguinat points out the pertinent facts which are well documented and underscores the effects of economic integration policies upon national economic areas, and the effects of free exchange upon localization of economic activity, except that of Benelux. He describes the consequences of the Franco-British treaty of 1860 upon the various elements (sectors, branches, regions) composing the French economy of that period. The general opinion prevalent in his country was that the treaty had an unfortunate effect, which led to its abandonment in 1881. However the author does not mention that this was in line with the trend that appeared during the last quarter of the nineteenth century, when a strong drift toward higher duties and away from free trade set in, and that this movement began in France. He also does not say that the French government needed large sums to pay the indemnity and the interest on the large debt incurred in the Franco-Prussian War of 1870, or that most branches of manufacturing in France had always been protectionist.

In the 19th century, Germany and Italy were able to realize their economic unity; in the former country economic unification preceded the political, while in the latter the economic unification resulted from the political. He treats the impact on the different regions of the Zollverein which benefited some and hurt others. The author quotes several Italian economists indicating that the unification of Italy, through its abolishing the existing customs barriers and opening its gates to foreign industry, led to a condition of growing unemployment and economic underdevelopment in the south of Italy although prosperity resulted in the north.

Bourguinat passes from the historical diagnosis to a theoretical analysis of the degree of incompatibility between the French economic area and the European area. In the first two of the four chapters his analysis is on specialization, while the latter two are on interdependence. He shows how customs unions generate inequalities of development among the regions.

The third part of this study is devoted to the correction of regional disparities within the European framework. He analyzes French regional expansion when faced with new exigencies resulting from the European Common Market. He suggests that the energies and resources of each French region be mobilized; and that for this purpose necessary economic and financial organs be established, implemented by regional programs of development.

In his conclusion, Bourguinat writes that if one takes into consideration all the possibilities afforded by the Treaty of Rome, not only the influences from the fusion of the markets but also the opportunities of a common policy, there is no room for pessimism. The Common Market can eventually be compatible with a harmonious development of the French regions, but it must face the various problems of regional economics and endeavor to solve them simultaneously.

The European Common Market is considered by many as the single most exciting economic development in the western world during the past decade. This pooling of economic interests and prosperity among the nations comprising the European Economic Community, with other nations clamoring to join, poses a variety of problems which will have to be solved. This book, although written in the early years of the European Common Market, is highly recommended for bringing some of these problems to our attention.

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Business Finance; Investment and Security Markets; Insurance

Life Insurance Companies in the Capital Market. By ANDREW F. BRIMMER. East Lansing: Bureau of Business and Economic Research, Graduate School of Business Administration, Michigan State University, 1962. Pp. xxiv, 394. $7.50.

This is, first a book of facts and figures. It reports in detail life insurers' participation in the various segments of the investment market since World War II, with some data going back to 1917 and 1920. The material was brought together from across the spectrum of published authorities, plus a questionnaire survey of life insurance investment practices and policies conducted in 1959 by Andrew Brimmer.

After three background chapters on life insurers and the behavior of capital markets, Chapter 4 takes up life insurers' investment criteria. The discussion is based on deduction from the characteristics of the life insurance business, induction from companies' past investment behavior, and replies to the questionnaires. Each of the next five chapters analyzes life insurers' historic behavior in a particular segment of the investment market: U.S. government securities, corporate bonds, mortgages, state and local government securities, and small business and equity financing. Chapter 10 is "Summary and Conclusions."

Brimmer accepted the scholar's responsibility to seek conclusions from facts gathered. But it is notoriously difficult to extract valid generalizations from empirical data, and he does not overcome the difficulties. Data often suggest conflicting hypotheses, and these appear in this book. A major conclusion (p. 372) is: "It was demonstrated that the quantity of funds supplied by life insurance companies to the capital market is primarily a function of the structure of interest rates. As a rule, in a period of rising interest rates, life insurance companies, while first attracted by the higher yield available on assets of greater credit risk, gradually become more restrictive and selective in their investment activities." Although the discussion that immediately follows deals with the effect of interest rates on holdings of U.S. government bonds, other statements on the same and the next page clearly show the point was intended to apply to investment practices in general. But in the succeeding discussion, limitations on the original, straightforward statement begin to appear: "New commitments to buy nonresidential mortgages show a mixed pattern with respect to business cycles and interest rate trends." With respect to securities, "the pattern is somewhat blurred," although it can be "detected." In mortgages, the relationship is "somewhat weaker than that outlined for securities." As a matter of fact, "turning points in interest rates and commitments for new mortgages seem to occur simultaneously and to move in opposite directions." This is *not* the pattern described in the first conclusion quoted, which placed increases in demand for mortgages at times of increasing, not decreasing, interest rates.

Difficulties appear at some other points. For example, the following reasons are advanced (p. 259) to explain "the sharply reduced role of life insurance" in the mortgage market during the housing boom of 1958-59 as compared with the booms of 1949-50 and 1953-55: (1) the total value of life insurers' assets was growing at a slower rate; (2) companies' holdings of U.S. bonds were low enough that they had little desire to exchange them for other assets; and (3) there was more severe competition from other lenders in the mortgage market.

These facts could be causes for reduced *dollar* investment in new mortgages. But the conclusion stated is: "Reflecting these diverse factors, real estate loans made during the most recent housing boom accounted *for a smaller proportion of total lending by life insurance companies* than they did in either of the two previous expansions" (italics added). Of the three "diverse factors" mentioned, only the third has apparent relevance to the conclusion quoted. A few sentences later, the following appears: "Furthermore, while they [life insurers] channeled *slightly more funds per month into the capital market as a whole* in 1958-59 than they did in 1953-55, the rate of investment in mortgages fell by more than two-fifths" (again, italics added). This last statement leaves the role of the first two stated "causes" even more of a mystery. (It may be further noted that none of this discussion makes any direct reference to interest rates as a cause, and of the three causes which are suggested, only the third would support the proposition that interest rates are the major determinant of life insurer's investment practices.)

The collection of facts in the study is so complete that all students of the subject have good cause to be grateful. Sufficient bases for conclusions about the behavior of life insurers in the investment markets are quite possibly here. But other students may want to draw different conclusions from this material than Brimmer did.

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Business Organization; Management Economics; Marketing; Accounting

Advertising and the Public. By RALPH HARRIS AND ARTHUR SELDEN. London: Andre Deutsch for Institute for Economic Affairs, 1962. Pp. xxiii, 304. 30s.

This is a companion book to the authors' *Advertising in Action.* The latter is mainly a collection of case histories, while the present book presents a discussion of the business, economic, and sociologic pros and cons of advertising. While this is definitely a British book, it would fit the U.S. scene well with merely changes in company names and pounds to dollars. The experiences with advertising and the arguments pro and con in England evidently are similar to those here.

Actually, while much has been added to recorded experience, not much new is cited for or against advertising than was suggested in my own little book, *Economics of Advertising* (New York, 1927), and spelled out in much more detail by Borden in *The Economic Effects of Advertising* (Homewood, Ill., 1942), nor are conclusions much more definitive. Nevertheless, the present book offers an excellent review and analysis and could well be "must" reading for students of business both in and out of college classes.

The book is divided into three main parts: Advertising in Practice; The Economics of Advertising; Advertising and Everyman. The impossibility is stressed of distinguishing between advertising and either some other tools of marketing, or propaganda and other forms of persuasion. The authors claim that advertising, or something like it, is essential in the conduct of a free, private-enterprise economy. Advertising results in speeding change in the scale and content of individual living and thus adds to the dynamics of the economy.

The authors make clear that, as economists, they are concerned with the basic questions of who shall make the decisions about the uses of resources or individual consumption programs, and under what laws, restrictions, and persuasions the decisions shall be made. Incidentally, the companion question of who gets how much of the rewards of industry is, of course, also raised. The authors point out that the persuasive efforts of advertising are similar, after all, to those used in politics and even in religion. They seem to favor leaving a wide field of persuasive action to private enterprise.

Advertising, the authors say, may be and is being used in connection with voting bonds for school purposes and similar public undertakings. This permits active competition between the public and the private segments of the economy and might well be encouraged. Moreover, advertising for savings for investment purposes also may tend to equalize competition among the various possible uses of consumer income. This, the authors believe, is all to the good.

This leads to the general conclusion that it is not advertising per se that is on trial, but the way it sometimes is used. On this point the authors seem to feel the need only for stricter control by government agencies over exaggeration. Even here, they point out, advertisements are so openly used that they are likely to be at least as truthful as human beings elsewhere. And, after all, it is stated, unless a product lives up to the advertised claims it will not long be repurchased.

The book contains three useful appendices on the law, codes of ethics, and advertising organizations. An excellent bibliography and an index of sources are included.

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Industrial Organization; Government and Business; Industry Studies

Air Transport and its Regulators—An Industry Study. By RICHARD E. CAVES. Cambridge: Harvard University Press, 1963. Pp. x, 479. $10.00.

This book is the most comprehensive study of the economics of U.S. domestic air transportation which has come to the attention of this reviewer. The work is well-organized, well-reasoned, and well-written, and should be extremely useful to both specialists and interested outsiders. It is to be hoped that many of the latter will avail themselves of the opportunity that Professor Caves affords them to become familiar with major facts and issues in the air transport field, which has not in the past received its due share of objective and scholarly attention.

The book consists of four parts, concerned, respectively, with market structure, public regulation, market conduct, and market performance. Part I takes up the number and size distribution of sellers, in both the country as a whole and individual city-pair markets; significant demand characteristics; determinants of costs and their variation with firm output; entry conditions that would obtain in the absence of regulatory barriers; and important features of input markets. Part II is a survey of economic regulation by the Civil Aeronautics Board, and includes a chapter on political environment which gives a realistic picture of the effect of outside pressures on the Board's treatment of the non-scheduled carriers, the general fare level, and the local-service airlines.

Part III deals with airline policies toward investment and expansion, pricing, and various characteristics of the product, and concludes with a valuable discussion of the influence of regulation on structure and conduct. It is pointed out that the Board's exclusion of new carriers from trunkline transportation, together with attrition of the number of old carriers by merger, has inevitably resulted in fewer trunkline firms, and has excluded from interstate markets the sort of specialist carrier that flourishes on the Los Angeles-San Francisco route. The major effect of regulation on conduct seems to have been deflection of management attention from pricing to other competitive dimensions. In this connection, Caves disposes once for all of the "prevailing myths which hold that, without the Board's restraining hand, the airlines would collapse in a Hobbesian war of destructive competition and that something like this actually occurred before the passage of the Civil Aeronautics Act in 1938" (p. 382).

Direct evaluation of "market performance" is notoriously difficult. Caves is to be commended for bringing together (in Part IV) the relevant evidence rather than blamed for the inconclusiveness of much of the result. The author's excellent analysis of Board arguments for "reasonable" returns on equity capital of 16 and 18 per cent, for large and small trunklines respectively, should be required reading for regulators as well as students of economic regulation. Pointing out that concern for the financial welfare of individual firms has been and is a major determinant of the Board's policies, Caves concludes that "as long as the Board keeps its present set of objectives and the certification machinery remains in effect, no major improvements [in economic regulation] are possible" (p. 446). The evidence appears to show "that the air transport industry has characteristics of market structure that would bring market performance of reasonable quality without any economic regulation"; therefore it would seem desirable to move "from the present system to one of relatively free competition" (p. 447).

With so much to praise, it may seem ungrateful to criticize. But for the sake of newcomers to the field, it is appropriate to warn against some possibly misleading impressions. It is going too far to say that air transportation should be subjected to *no* economic regulation. As long as the Air Traffic Conference continues to function, as function it must, some governmental supervision will have to be provided. Second, provision of below-cost service to some communities at the expense of others was not a declared objective of early regulation, and would have been contrary to the Board's policy of certificating only services that would not raise subsidy rates and showed promise of early self-support. "Internal subsidization" was promoted by Board spokesmen to the status of guiding principle when non-scheduled carriers were attempting to compete effectively with the "grandfathers," and old arguments for protective entry control had become out-of-date or obviously false.

Third, the fact that local-service airlines secured Congressional support for their permanent certification does not absolve the Board from responsibility for initiating this extremely costly "experiment"—still with us after eighteen years. The whole episode strongly suggests that the Act be amended to make it impossible for the Board ever to repeat this sort of error. Fortunately, the President has already directed the agency "to develop by June 30, 1963, a step-by-step program, with specific annual targets, to assure sharp reduction of operating subsidies" to local-service and other domestic airlines. (Message on Transportation, April 4, 1962.) It would, of course, be wasteful to force liquidation of experienced enterprises if a useful place can be found for them in the nation's transportation system. The opening up to them of markets now closed by regulation (as is tentatively suggested by Caves) would be the logical way to give these airlines a chance for self-support.

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Diversification and Integration in American Industry. NBER General Series No. 77. By MICHAEL GORT. Princeton: Princeton University Press for National Bureau of Economic Research, 1962. Pp. xxi, 238. $5.00.

With this interesting and stimulating book the National Bureau has added to its studies of industrial organization an inquiry into interindustry structure based primarily on a sample of 111 large, diversified enterprises. Working with Census data (some heretofore unpublished, supplemented by data from published sources), Gort compiled the product structures of the sample firms in 1929, 1939, 1947, 1950, and 1954. His analysis of the changes in product structure over time is centered around the measurement of trends in diversification and the identification of the economic characteristics of both diversifying firms and the industries frequently entered by such firms.

The book contains a brief foreword by George J. Stigler, an adequate index, 143 pages of text with 60 tables, and 88 pages of appendices with 29 tables. The first of eight chapters summarizes the most important conclusions of the author. The second chapter explains and defends some of the concepts and methods used. A multitude of variables are tested for statistical interrelationships in an attempt to find explanations for the observed changes over time in the product structure of large manufacturing firms.

The definition of diversification is, of course, crucial to the usefulness of conclusions drawn from such an empirical study. Gort defines diversification as "an increase in the heterogeneity of output from the point of view of the number of markets served by that output" (p. 9). Thus, to say that diversification has occurred requires that one define the markets in which a firm's output is sold. Gort raises the question and briefly discusses two theoretical bases for distinguishing between two separate markets. He concludes that two products are sold in separate markets either if cross-elasticity of demand between them is low or if resources cannot be shifted rapidly from the production of one to the other. The second criterion, frequently overlooked in theoretical discussions of market definition, is important because supply as well as demand interdependence results in interrelated prices, earnings, and output.

In view of the current increased interest in questions of public policy on market structure and mergers—horizontal, vertical, and conglomerate—increased knowledge of changes in diversification and its causes and effects on the economy would certainly be welcomed. The reader looking for such new knowledge may be disappointed in this book. Although it contains many facts, Gort's study suffers much from the lack of empirical information that corresponds closely to the variables defined and used in economic theory. Gort apparently subscribes to the not uncommon view that, on questions of data, something is better than nothing. For example, he identifies products as belonging to separate markets on the basis of their classification in the 1945 Standard Industrial Classification Code. He says (p. 9): "As a practical matter, there was little choice since most of the data available were cast in the mold of the Classification Code." Thus, geographical diversification is explicitly ruled out of the study.

Within the limits of Census data nothing could be done about the geographical problem. Gort, however, had some freedom in making the product classification decisions. He judged the 4-digit level of industry detail to be an appropriate compromise between too fine and too coarse a breakdown, since he felt the most appropriate level given in the Code not to be the same for each industry. He checked his results by some use of 2- and 3-digit levels, but made no attempt to work with the finest breakdown and reconstruct the broader classifications to fit more closely the requirements of market analysis. The SIC code, for example, treats rolling, drawing, and extruding of copper products as a 4-digit industry distinct from rolling, drawing, and extruding of aluminum products, but rolling, drawing, and extruding of magnesium, lead, nickel, zinc, and titanium are lumped together for no reason related to demand or resource mobility. Since his sample of 111 firms, on which much of the book is based, was not randomly selected, the author might have picked the industries and firms to be included with consideration given to the adequacy of the Census classifications. Instead he picked them primarily to include the largest manufacturing firms and to reflect the heterogeneity in the various industries, while keeping the number down to a manageable size.

In spite of the limitations stemming from the nature of the sample, and the dangers inherent in inferring cause and effect relationships from statistical correlations, Gort has presented a number of interesting findings. For example, vertical integration appears to compete with diversification for capital or managerial resources. The sample firms diversified into high growth industries with rapid technological change. The trend from 1929 to 1954 was toward more diversification for the 111 sample firms. Size of firm appeared to be unrelated to measures of vertical integration. Diversification was positively correlated with the proportion of total shipments made by the largest four producers in a firm's primary industry. Most changes in product structure did not take place through merger. Neither profit rates nor dividend pay-out ratios appear to be correlated with diversification.

The title of the book is somewhat misleading, since "integration" is used by Gort to mean only vertical integration and his coverage is not adequate to warrant generalizations about all of U.S. industry. But as long as the reader recognizes the tentative nature and limited range of applicability of such research findings, this book will provide a large mass of useful information on conglomerate enterprises.

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Market Structure, Organization and Performance—An Essay on Price Fixing and Combinations in Restraint of Trade. By ALMARIN PHILLIPS. Cambridge: Harvard University Press, 1962. Pp. xiii, 257. $5.50.

The major emphasis of this book, as its title indicates, is on the theory of rivalry among firms and the implication of that theory for policy. Professor Phillips suggests a new theoretical approach to market analysis based on the behavioral sciences and organizational theory. The author wisely states in his preface, "Conversations and correspondence with colleagues and friends have made me quite aware that neither the policy suggestions nor the theoretical framework presented in this book is likely to win easy acceptance. The theory has foundations in organizational and group behavioral principles and, with these principles and the expected reception of my theses in mind, I have sometimes wanted to abandon my heterodoxy and to adhere more closely to the norms of my profession."

The author seems to feel that present economic thought is still dominated by nineteenth century liberal economics which emphasizes among other things the need of complete independent action among sellers and among buyers for the existence of "free" competition. The interpretation of these premises, the author feels, resulted in economists advocating antitrust policies "which were directed at making the real world more like their assumptions rather than changing their assumptions to afford a better analysis of the real world" (p. 221).

To support his position the author seems to use the old argument against economic theory that firms really do not set their prices where marginal cost is equal to marginal revenue. They seem to have a target profit where managements' best guesses about the most they can make and their behavior and performance are influenced by members of the market group. For example, Phillips states ". . . firms appear to conform to the canons of profit maximization only if long run and dynamic considerations are allowed into the maximization concept, and sometimes perhaps not even then" (p. 5). This statement suggests the principal defect of the work; in short, a synthesis of the author's thoughts with dynamic oligopoly analysis and the treatment of effective and workable competition would have been more desirable. Many economists, J. M. Clark one of the first, recognizing the imperfections of competition and the many patterns into which it falls, have sought to develop a concept of workable competition which would differentiate between socially acceptable and unacceptable situations. In determining workability economists such as George W. Stocking examined three factors: industrial structure, business conduct, and economic performance. Although the economic standards of workability have been debated and cannot solely be used to determine the legality of business arrangements under the antitrust statutes, the author should have shown how the principle of workability can be utilized to show the existence of situations which he describes in his book.

Much of Phillips' book is devoted to the study of particular markets and the application of his hypothesis. In Chapter 2 the theory of interfirm organization is presented. This theory is based in part on the developments in behavioral sciences and here the author indicates that "in the absence of a formal interfirm organization a high degree of rivalry tends to develop in markets characterized by large numbers of sellers with heterogeneous value systems, especially if these sellers are confronted with powerful buyers or suppliers" (p. 74).

The ensuing 7 chapters are devoted to the study of alternative structural and behavioral market situations and the resulting market performance. The cases the authors has chosen have value in illustrating the application of his theory. Chapter 3 surveys markets, the outstanding characteristic of which is the small number of directly competing firms. Chapter 4 notes the frequency with which markets in which a large number of sellers appear have been accorded some sort of exemption or immunity from the antitrust laws. Chapter 5 illustrates the need for a market organization to maximize independent competitive behavior and indicates interrelationships among market organization, market structure, and market performance. The Addyston case provides some important lessons.

Each of the following four chapters treats particular markets in greater depth. The bituminous coal industry is surveyed because of the lack of responsiveness its structure has demonstrated in the face of decades of poor performance and because of the illustration it provides for government regulation or immunities. In Chapter 7, the American Column and Lumber case is compared with the Maple Flooring case to show the ease with which nonanalytical performance tests can mislead antitrust policy and the need for theoretical explanations rather than just descriptions of performance.

Chapter 8 uses the Trenton Potteries case to illustrate the influence of leadership in the market group and also to indicate some of the difficulties that may be encouraged in effecting cohesive, parallel behavior within the market group, with more than a few sellers. The Plate Glass Mirror case, presented in Chapter 9, treats the problems inherent in effecting a working conspiracy in a market with more than a few sellers and a modest degree of heterogeneity in value systems in greater detail. Chapter 10 traces the development of per se illegalities in order to provide a chronological background for the policy conclusion of the next chapter.

The final chapter presents the policy implications of the analysis used by the author. The appropriate antitrust policy toward business, according to the author, is a proposed rule of reason for both per se legalities and per se illegalities. This conclusion was answered by Justice Douglas, speaking for the Court in the Madison Oil case, who stated that the reasonableness of prices cannot be left to a value judgment. "The reasonableness of prices," he declared, "has no constancy due to the dynamic quality of business facts underlying price structures. Those who fixed reasonable prices today would perpetuate unreasonable prices tomorrow, since those prices would not be subject to continuous administrative supervision and readjustment in the light of changed conditions."

In the reviewer's judgment, the book presents a unique approach to public policy questions. In general, the book is well done, the inquiry scholarly and a contribution to the theory for public policy literature.

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Comptes nationaux et régionaux de l'énergie. Recherches sur l'économie française, No. 7. By JANINE CAPRONNIER-SPIELHAGEN. Paris: Armand Colin, 1962. Pp. 345, plus unpaginated maps and tables. NF 35.

The role of energy in economic activities has become a subject of an increasing number of studies. And although energy is, as Schurr and Netschert observe [1, p. 16], a pervasive ingredient in the production of all goods, the question of its part in economic development defies a simple answer. Although the approaches vary, a possible avenue is to explore the interindustry structure of energy supplies.

This is the method adopted by Janine Capronnier-Spielhagen for her inquiry which, in the words of Jean-Marcel Jeanneny, an eminent French economist who directed her research, was designed to obtain a better insight into functional interindustry relations concerning energy and their regional variations in France. Moreover, Mrs. Capronnier-Spielhagen's analysis was designed to furnish a more reliable basis for formulating goals in French economic development plans.

The author sets her inquiry on two different, though interconnected, levels. Part I concentrates on flows of energy from energy-producing and energy-processing industries to all other sectors of the French economy, including households and the government. By means of an adjusted Leontief model, she presents, in specific units and hard-coal equivalents, interindustry sales and purchases (rather than inputs and outputs) of primary and secondary energy. Although this procedure involves a certain degree of double-counting, it permits the author to construct a comprehensive picture of available energy supplies and their functional distribution throughout the industries of France. Initially, Mrs. Capronnier-Spielhagen constructed such flow-matrices for 1954, 1955, and 1956.

The 1954 picture is further elaborated by the presentation of an energy-value balance sheet which analyzes the monetary aspects of inputs and outputs of the energy industries. Due to the particular institutional structure of the French energy sector this inquiry is limited only to the sectors dominated by nationalized industries: that is, coal, gas, and electricity.

Mrs. Capronnier-Spielhagen used the delay between the completion of the original research in 1958 and its ultimate publication in 1962 to construct supplemental energy flow-tables for 1957, 1958, 1959, and 1960. Vexing as the delay might have been to the author, these additional flow-tables are extremely valuable instruments of analysis. They permit one to explore changes in interindustry energy relations over a period of seven years during which major structural modifications occurred in the French energy economy.

Part II, the regional accounts of energy, occupies by far the largest part of the book. Insufficiency of statistical data forced the author to abandon the approach of interindustry flows. Instead, she explores the regional origin of each fuel, showing its purchases by selected economic organizations or groups for each of the sixteen regions into which she divided France for this purpose. This analysis is carried out in physical and monetary terms and necessitated exploring institutional and economic conditions of the regional structure of energy prices. The result is presented in a complex, but rather interesting, matrix which indicates not only the total interregional expenses for the respective fuel by its purchasers but also the shares received by producers, transport, commerce, and the government (in taxes).

The extent of the work Mrs. Capronnier-Spielhagen undertook in this connection is best reflected by the more than 150 matrices, in addition to maps and other forms of tabulated data, required to present all the aspects of each fuel's physical and commercial intra- and interregional physiognomy. Moreover, another 80 triangular matrices summarize each region's energy balance sheet indicating intraregional production, outflow, and inflow of fuels to and from the rest of France and third countries.

The complexity of this kind of inquiry precluded its ready extension to other years. That is even more unfortunate since the interregional flows of fuels in France underwent considerable change in the intervening years under the impact of exploitation of natural gas from Lacq and oil from the Paris region.

By way of criticism, albeit moderate, it may also be pointed out that the first part of the book lacks the conceptual refinement of, for instance, Schurr and Netschert's work or Teitelbaum's analysis of energy flows in the United States [2]. On the other hand, Mrs. Capronnier-Spielhagen's study, particularly in the analysis of the regional aspects of energy flows, goes far beyond the scope of the latter inquiry.

Also her ready and unqualified acceptance of conclusions of others cannot be passed here without objections. Thus her much-too-sweeping statement about a close relationship between GNP and energy consumption cannot be generally upheld in view of other studies [1, pp. 13-17], and it is even contradicted in the Coal and Steel Community Study [3, pp. 44-46], which she quotes in another context.

In some regards, technical aspects of exposition might also have been improved. It is annoying to have to search for an explanatory note to a table throughout the text and footnotes. A rather rudimentary index further compounds the discomfort of the reader and seriously affects the practical value of the tremendous wealth of data generously spread throughout the main body of discussion as well as under the margin. Some matrices lack proper headings to be easily understood, and others are not found where the uninitiated would expect them to be, as, for instance, Table 1-33, which follows Table 1-37.

Nevertheless, these are minor points in an otherwise solid piece of research which easily accomplishes the stated objectives of clarifying functional relations of energy in French economy. And although one might wish that she had elaborated implications of her analysis beyond a brief résumé of the results, the usefulness of her inquiry for further exploration of the impact of energy on economic growth can hardly be questioned.

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Price Discrimination Under the Robinson-Patman Act. By Frederick M. Rowe. Boston: Little, Brown and Company, 1962. Pp. xxx, 675. $22.50.

Price discrimination under the Robinson-Patman Act is a complex event whose legal status depends upon the prices charged by a company, the reasons for the prices, and their competitive effects. Unsatisfactory as this may seem to those seeking simple and definite rules, it reflects an attempt by Congress to vary the impact of regulation of price discrimination with the gravity of the effects upon competition. Fortunately for the economist, the very complexities of the law that have given rise to an extensive line of litigation have also generated a wealth of information on the effects of price differentials under widely varying conditions. Indeed, economists will find Rowe's analysis of the issues arising under the Robinson-Patman Act an indispensable source of information on price structures and on the pricing practices of individual companies.

Short of restating the text of the Act, there is no way of outlining its requirements without doing violence to the shadings of company behavior and market consequences of which it seeks to take account. In essence, the Act prohibits an individual seller from charging different prices for homogenous commodities, if these differentials *may* contribute to a substantial lessening of competition. Price differentials do not violate the law if the seller can show that (1) the differentials make only due allowance for differences in his cost of manufacture, sale, or delivery, or (2) his prices have changed in response to changing market conditions, or (3) the low price is designed, in good faith, to meet the equally low price of a competitor.

Although each step in determining whether a given price differential is a prohibited form of discrimination requires economic analysis, the analysis must be carried through in terms of established standards for legal evidence. Rowe's examination of the appropriate steps is a model of analytic acumen and factual precision. He systematically lays out the interrelated business and legal considerations surrounding the words of the law and illustrates each point with relevant facts from the cases.

Rowe notes that the Federal Trade Commission and the courts have examined the competitive effects of price discrimination at three different market levels: at the primary level (if competition between the original seller and his competitors is affected), at the secondary level (if competition among customers of the original seller is affected), and at the third level (if competition among customers of favored and disfavored customers is affected).

Major questions at the primary level concern the possibility of predatory pricing designed to destroy a competitor or collusive or parallel pricing where discrimination is embedded in a pricing system that is adopted by two or more sellers. At the secondary level, Rowe notes that the effects of discrimination will vary with competitive relations among the customers who are charged different prices and upon these customers' cost and price structures. The law is not violated when customers who do not compete with each other are charged different prices. Arbitrary classification of customers on a functional or a geographic basis cannot, however, be used to justify discrimination that generates anticompetitive effects. Case experience with competitive effects at the third level is limited. The most critical problems have turned upon the situation of the buyer who functions in a dual capacity, for example, as a wholesaler and a retailer. Such a buyer can, for example, pay a wholesale price and then resell at retail at a lower price or at a higher margin than competitive retailers who buy through wholesalers. In either case, the reseller who operates in a dual capacity has an advantage over single-level competitors. The implications of this problem are far-reaching, since strict interpretation of the law might require a type of resale price surveillance by the original seller which is not compatible with the canon of free enterprise.

Unlike Robinson-Patman Act issues involving the effects of price discrimination on competition, those involving cost justifications have yielded meager data for economic analysis. Congress wrote the cost justification provisions on the theory that the price differentials resulting from cost differentials should not be penalized. But companies whose price differentials have been challenged have not found it very rewarding to attempt to allocate overhead costs so as to justify price differentials. Labyrinthine interpretations, costly cost studies, and repeated rejection of such studies by the Federal Trade Commission on the ground that they did not reflect recognizable cost differences have marked the history of this section of the law. Rowe patiently examines the conceptual and factual difficulties facing a company that seeks to set up a cost justification defense, penetrating deeply into the rationale of the law's attempt to force a company to relate price differences to cost differences—as if cost were the sole determinant of price.

Perhaps of greatest interest to economists is Rowe's examination of the "good faith" defense: under what conditions is a seller free to lower his price to some customers, but not to others, in order to meet the equally low price of a competitor? At issue, for example, in cases where the "good faith" defense has been challenged by the Federal Trade Commission have been the following questions. Is the price that is met nondiscriminatory? Has the seller arrived at his prices independently or through collusion? Does the lower price represent an adjustment by the seller to a specific competitive problem or is it part of a rigid pricing system? Does the lower price merely meet a competitor's price or does it fall below it? Is a lower price permissible only to keep "old" customers or may it be used to attract "new" customers? May a seller charge a higher price for his own brand than for a customer's private brand? May a price concession be granted to help a customer meet the price of one of his own competitors?

In his final chapter, Rowe examines the basic conflict between the type of Robinson-Patman Act enforcement which puts a premium on price inflexibility and a strong antitrust policy. He also notes critically the heavy impact that enforcement has had on small concerns and the relative scarcity of cases against large buyers.

The economist who works through Rowe's study will find a penetrating analysis of price behavior combined with a broad-gauge appraisal of the competitive consequences of different forms of pricing. The text and reference apparatus open up a wealth of information concerning pricing practices and their effects in different market settings. Regardless of whether the reader has specialized in pricing practices or comes to those questions for the first time, he will find this volume an indispensable guide to the effects of pricing practices on competition in specific markets, the relation between company behavior and the structure of markets, and the serious difficulties in legislating concerning pricing behavior while seeking to maintain freedom of pricing.

BETTY BOCK

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Competition and Monopoly in the British Soap Industry. By H. R. EDWARDS. New York: Oxford University Press, 1962. Pp. xi, 270. $5.60.

The first half of the book is an attempted "reformulation of the theory of price and output formation in manufacturing industry" (p. 3), which is used as the basis for a relatively brief study of the British soap and detergents industry. The core of the analysis is good will in its two-fold role as determinant of stable market shares and deterrent to entry. The author's basic conclusion, developed in Chapter 3, is that "in most cases the industry price will be set at that level which will just suffice to deter the entry of new competition" (p. 58).

In Chapter 2, Edwards makes the somewhat conflicting claims that good will will "sustain for long periods an unequal division of the market at the one price" (p. 49) but, contrary to Chamberlin and others, will not permit unequal prices for products of substantially equal physical quality. The argument seems to involve a shift of emphasis if not of assumption. The complexity of product, lack of knowledge, and wisdom of sticking with the known, adduced early in the chapter, would support price differentials. Conversely, the later assumption that consumers "can and do recognize equivalence in size/quality of product where it exists" (p. 41) erodes away much of the case for stable market shares based on irrational preferences. The valid content of the chapter appears to be a restatement of the kink with respect to good will, which is built up with difficulty because efforts are matched (pp. 32-37) but lost relatively easily by unilateral action (pp. 41-52).

It is difficult to say precisely what the author's reformulation finally turns out to be. The first presentation is simply the joint maximum on a demand curve cut off horizontally by a uniformly recognized threat of entry. This is essentially Bain's theory of the limit price developed over fifteen years ago and published in a textbook[1] over ten years ago but not acknowledged by Edwards.

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[1] Joe S. Bain, *Price Theory* (New York: Henry Holt, 1952).

Obviously such a model needs many qualifications, but they are introduced in such a way as to leave little precise content. As an example, a markup over his own full cost is first suggested as the method by which an entrepreneur calculates the ceiling price. Then follows the qualification: "Secondly, then, and overriding the firm's own costing, the enterpreneur will look to what his rivals are doing in both his own and adjacent lines of production (potential competitors), and adjust his policy accordingly" (p. 74). Since his rivals are also looking to what he is doing, it is hard to predict what any of them will do.

After introducing numerous qualifications Edwards, in effect, abandons the attempt at reformulating theory and concludes that "the line of action which holds out the most promise of further progress is the detailed empirical study of particular industries" (p. 122). The book would have been much more effective if he had begun here and devoted more effort to a theoretical model of his own industry.

The study of the British soap industry is an interestingly written, competently assembled historical narrative. To an American reviewer, there is a striking coincidence between William H. Lever in the British soap industry and James B. Duke in the American cigarette industry. In the same decade of the 1880's, in similar conditions of comfortable oligopoly, each entered the industry and quickly fought into a position of dominance by aggressive marketing methods. The reactions of existing manufacturers are surprisingly similar. Lever's methods were apparently not so ruthless as those of Duke; Edwards makes no mention of local price cutting, which would have been an effective weapon in the sharply segmented geographical market. In any event, while Duke was able to drive the industry into combination within a few years, Lever followed a gradual policy of acquiring firms through stock purchase and did not attain a 70 per cent control of the industry until 1920. Both Lever and Duke found their dominant positions threatened by competitive encroachment when selling efforts were relaxed, bought out rivals, and discontinued their brands. Both industries ultimately become oligopolies —essentially duopoly in the British soap industry—though by different routes. It would be interesting to have a careful comparative analysis made.

What is lacking in the study is a rigorous crosscut analysis to supplement the historical narrative. Edwards simply does not devote sufficient effort to the fashioning of a theoretical model for the industry. Also, with respect to competitive and collusive strategies, the account gives the appearance of being written solely from the outside, presumably because there was not available the kind of data turned up by our antitrust prosecutions.

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Essays on Some Unsettled Questions in the Economics of Transportation. By George W. Wilson. Bloomington: Foundation for Economic and Business Studies, Indiana University, 1962. Pp. ix, 182.

George W. Wilson asserts in the introduction that the contributions of economic theory to the field of transportation have been "relatively meager" during the period running roughly from 1920 to 1950, but he believes that there is presently a "substantial revival" of interest in the economics of transportation which will be characterized by "much more analysis and much less description" in the treatment of transportation problems. In the light of such a forecast or appraisal of the situation, Wilson believes that now is an appropriate time to "raise" some unsettled questions "needing further analysis and solution."

Six major problem areas are "raised" by the author, with one chapter devoted to each of them. The approach in each chapter is largely one of presenting the problem and of reviewing, with thorough documentation, some of the most important contributions made in the literature to the analysis and solution of the stated problem.

The first chapter considers the concept of inherent advantages in general terms, with primary attention being directed to a definition of the concept itself and to an examination of the nature of the output unit in transportation. In the second chapter, various problems of costs in transportation are investigated, with major emphasis being given to out-of-pocket costs, indirect costs, and the findings of selected empirical cost studies. The third chapter investigates in detail the inherent advantages—cost and quality—of rail and truck transportation.

In the fourth chapter, the subject matter is the economics of intercarrier competition, and the analysis relates to the factors that must be considered in arriving at the optimal intercarrier equilibrium. The fifth chapter looks at value-of-service pricing from the standpoints of both the "old view" which emphasized the necessity for and desirability of value-of-service pricing and the "new orthodoxy" which disparges value-of-service pricing and praises the use of cost-of-service pricing. The author's contention is that the decline in favor of value-of-service pricing is attributable largely to misapplication of the value-of-service principle. The final chapter analyzes competition among common and unregulated carriers, with particular attention being devoted to the relative costs of common and private carriage.

The author has done a good job of defining the key problem areas and of critically reviewing the literature pertinent to these areas. One might question, however, his omission of the declaration of national transportation policy of the Interstate Commerce Act from the list of problem areas in transportation. Some of the major problems today might be less severe if it were not necessary to operate our transportation system within the limitations imposed by the present declaration of policy. As a final general comment, it might be argued that this book will be of greater value to the inquirer having only a rudimentary understanding of the economics of transportation than to the student who is well versed in transportation economics and is familiar with the literature in the field. To the former, the book would probably serve as an effective revelation and description of some of the unsettled questions in transportation; to the latter, it would likely represent an equally effective, although familiar, description of some transportation problems whose existence had been acknowledged but whose solutions remained largely unrevealed.

In terms of specific contributions, Wilson urges greater use of the tools of economic analysis in the solution of transportation problems and insists that more attention should be paid to the clarification of definitions, such as those of the important cost categories, as a prerequisite to intelligent policy formulation. Of particular interest from the public-policy standpoint are the fears which seem to be implied in the sixth chapter for the future health of common carriage vis-à-vis private carriage under present regulatory policy. The author maintains that the common carriers suffer from a regulatory handicap which does not impede the development of private carriers. This handicap he would partially remove through such policy changes as revision of the law governing personal discrimination, the removal of back-haul restrictions on common carriers, the granting of permission to common carriers to reduce rates "where costs justify such actions," and, more controversial, the amending of the Reed-Bulwinkle Act to permit common carriers of different types to join the same rate bureau.

Finally, the author may have been too much engrossed in the statement and analytical review of existing transportation problems and too little occupied with their solutions; but the book does serve as a valuable bench mark to indicate the progress of economic theory in the resolution of some of the most troublesome transportation problems.

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The Supply and Price of Natural Gas. Supplement to *Journal of Industrial Economics.* By M. A. ADELMAN. Oxford: Basil Blackwell, 1962. Pp. 116. $4.20; $2.10, Subscribers.

To the increasing flow of literature on the long-neglected natural gas industry, M. A. Adelman has added this brief monograph, published by the *Journal of Industrial Economics* in the hope that it will be the first in a series dealing with problems facing modern industry.

Adelman proceeds from a consideration of petroleum production costs and the problem of joint cost incurrence in oil-gas production to an attempted determination of the shape of the natural gas supply function and a description of conditions affecting the demand side of the market. He concludes with an interesting chapter entitled "Natural Gas and the World Petroleum Market" and an appendix discussion of Soviet reserve statistics.

Adelman freely admits that the brevity of his monograph is due in part to the fact that "Some earlier work of mine on this subject [competition in natural gas markets] has been largely superseded" by others.[1] Unfortunately, he uses this fact as a basis for the sweeping conclusion that "There is . . . no question but that the field price of gas in the United States is competitively determined" (p. 39). This statement suffers from three errors. First, as the analyses of Paul Douglas,[2] Joel B. Dirlam[3] and Alfred E. Kahn[4] indicate, there remains a very substantial question, indeed, concerning the workability of competition in natural gas markets. Second, the implication that the cited sources support the Adelman view is incorrect. Neuner, for example, carefully points out (*op. cit.*, pp. 280-85) that the absence of structural monopoly does not, because of the long-term contracts and escalation clauses characteristic of the industry, prevent "a level of field prices higher than that which would prevail in the competitive field market." (Cf. Kahn, *loc. cit.*, p. 509). Third, the few comments offered by Adelman are insufficient to support his thesis. For example, his low concentration ratio in sales under existing contracts is irrelevant; the important figure would be one showing concentration of holdings of uncommitted reserves, which represent the supply available to meet current and prospective demand. And it is in this area that Adelman's plea for better reserve data (p. 59) is best taken; we have available only Kahn's admittedly horseback estimate that the eight largest sellers controlled 60 to 70 per cent of the uncommitted reserves in 1955 and the knowledge of Humble's huge King Ranch holdings as indicia of this concentration ratio.[5]

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[1] P. 39 n. Those cited are Leslie Cookenboo, Jr., *Competition in the Field Market for Natural Gas* (Rice Institute Pamphlet, Vol. 19, 1958); James W. McKie, *The Regulation of Natural Gas* (Washington, 1957); Edward J. Neuner, *The Natural Gas Industry* (Norman, Oklahoma, 1960); and Paul W. MacAvoy, the then ms. of *Price Formulation in Natural Gas Fields* (New Haven, 1962).

This reviewer also has difficulty with Adelman's analysis of petroleum production costs, in which he seems repeatedly to confuse enterprise with industry economics. From the fact that a characteristic of exploration "is the unpredictable relation between outlay and receipts ***in any given effort***" (p. 6, emphasis added), Adelman concludes that in petroleum production it is impossible to predict "how much capacity can be secured for any given amount of money investment . . . for any given project, company, area, or even the whole industry" (p. 7). Yet, as we move from the single-well producer to the industry aggregate, predictability increases; the probability of success of a single well is, indeed, difficult to determine, but the probability that the proportion of successful ventures to total ventures will approach some predictable value is quite high, given the year-in and year-out stability of the industry's success rate.

This same confusion of individual ventures with industry experience leads Adelman to criticize with undue severity an important U.N. study (*The Price of Oil,* 1955). In acidly rebutting that work's conclusion that the petroleum industry is one characterized by decreasing costs, Adelman points out that in an individual reservoir or field a point is eventually reached at which inputs per unit of additional output increase (p. 13); but he does not adequately analyze the aggregate industry situation, in which production from new and often substantial discoveries (e.g., Fairway field, discovered in Texas in 1960, with estimated reserves of 500 million barrels) is constantly being phased-in, with a downward pressure on costs at least equal to the upward thrust exerted by the individual reservoir cost pattern he cites.

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[2] Congressional Record, Jan. 20-25, 1957.

[3] J. B. Dirlam, "Natural Gas: Cost, Conservation and Pricing," *Am. Econ. Rev., Proc.,* May 1958, *48,* 491-501.

[4] A. E. Kahn, "Economic Issues in Regulating the Field Price of Natural Gas," *Am. Econ. Rev., Proc.,* May 1960, 506-17.

[5] This reviewer, for one, is inclined by the industry's unwillingness to rebut Kahn's data, despite the importance to them of so doing, to give his estimate considerable weight.

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Labor Economics

Labor in Developing Economies. Edited by WALTER GALENSON. Berkeley and Los Angeles, University of California Press, 1962, Pp. x, 299. $6.00.

This interesting book contains studies of seven countries by five authors and an introduction by Professor Walter Galenson. As the editor notes, the countries—Pakistan, Indonesia, Brazil, Argentina, Chile, Israel, and Turkey—are all underdeveloped economically by the standards of Europe and the United States, but the causes and problems which confront them are quite different. One of the results of reading these detail-packed pages is to become aware that low economic development is not the consequence of a specific problem, but a result traceable to a variety of causes—historical, political, and economic. The authors have therefore sought to place the problems of the respective labor movements in a broad framework.

Professor Willis D. Weatherford's study of Pakistan is devoted to exploring the relation of people to resources, the significance of agriculture, the high labor turnover in industry, and the low honorific value placed upon material accumulation by Pakistani workers. Given the general background and the struggle of a newly organized country to develop a viable economy, the difficulties encountered by labor organizations can be clearly understood. In addition, the labor unions are confronted by competing ideologies, which adds to confusion and uncertainty. Finally, labor is a minority in a predominantly agricultural country.

As a leader in the fight for independence, the Indonesian labor movement, according to Professor Everett D. Hawkins, has retained public esteem. It is, however, also plagued by internal differences, uncertainties, and the low educational levels of potential members. In analyzing the economy and history of the country, Hawkins shows the industrial part of the labor force is a relatively small group, it is unevenly distributed within the country, and it is sorely lacking in training and education. One notices that the removal of colonialism, necessary for future progress and independence, has not resulted in sensational rises in labor productivity or increases in the low incomes of the working population.

The three countries—Brazil, Argentina, and Chile—dealt with by Professor Robert J. Alexander, appear to have the common attributes of proneness to revolution, exporting of raw materials, and a failure or reluctance of rural landowners to invest in industrial enterprises even at a higher return than could be obtained in agriculture. Alexander discusses the influence of government, especially that of the dictatorships, on the labor movements, but it would appear that dictatorships also introduce an added uncertainty into private investment which may account for some of the lower economic levels in the above countries. Alexander discusses the differences between the impoverished countryside and the industrial cities in which much of the manufacturing industry is concentrated, and describes the structure and operation of labor organizations, their bargaining with employers, and the predominant influence of the governments in these areas.

Professor Irvin Sobel's excellent study of Israel is a highly informative essay on a unique labor movement—the Histadrut. The problems facing the Israeli movement are so different that their analysis can scarcely serve as guide lines to nations such as Pakistan and Indonesia, Turkey or Argentina. For example, the costs of developing human capital—professional and skilled labor—were not appreciably borne by the Israeli economy. Nor has there been the same lack of entrepreneurial spirit or a "flight from industry." Israelis, in contrast to the Pakistanis, do not need the Protestant ethic for justifying trade, hard work, and economic gain. Not only scientific and entrepreneurial skills and large private donations, but the advanced philosophies of European labor have been brought in and sown in Israel. Consequently, the essay with its wealth of details must stand on its own.

Like the others, the essay on Turkey by Professor Sumner M. Rosen is a well-documented description of the birth pangs of a labor movement struggling against inertia and opposition. As shown by the author, the labor organizations in Turkey are by no means secure, even though the author believes they are necessary in a developing economy.

A reading of these excellent essays leads to a number of questions. Could a better understanding of the labor movements described be gained by comparing them with those in Western Europe and the United States in the late eighteenth and early nineteenth centuries? Shortages of capital existed in all the countries on the verge of industrialization. The socialist and labor leaders of European countries and the United States denounced the exploitation of the early factory worker, and no one is more eloquent than Marx in his description of "primitive accumulation." Why has concentration upon development at the expense of worker consumption become more of a virtue than it was in early European capitalism? It may be that colonialism has hampered growth of industry, and there exists a natural desire to catch up. Finally, the limited influence of the skilled artisan who in the West frequently became an entrepreneur but also established the early trade unions may account for the political direction of some of labor in underdeveloped areas. Unless a trade union performs some type of service, it is difficult to envisage it as anything but a means for mobilizing labor behind some political organization.

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NOTES

A nominating committee consisting of Paul Samuelson, chairman, Robert Baldwin, Harry Eastman, Edgar Edwards, Frank Fetter, and Ilse Mintz has submitted the following slate of nominees for 1964 officers of the American Economic Assocation.

President:

George J. Stigler

President-Elect:

Joseph J. Spengler

Vice President:

James S. Earley
Bernard F. Haley
Oskar Morgenstern
James Tobin

Executive Committee:

Joseph D. Coppock
Harry L. Johnson
Douglass C. North
Robert M. Solow

Representative, Social Science Research Council:

George H. Hildebrand

The annual meeting of the Association will be held in Boston, Massachusetts, December 27-29, 1963, with headquarters at the Statler Hilton.

GROUP FLIGHTS TO ANNUAL MEETING

The American Economic Association is developing plans for group flights to the Boston meeting, December 27-29, from the Chicago, Cincinnati, Denver, Los Angeles, Pittsburgh, San Francisco, and St. Louis areas. These flights will be scheduled to arrive in Boston the afternoon of December 26, with departure either the evening of December 29 or the morning of December 30. Potential savings amounting to 20 per cent of round trip air fare are possible by traveling *as a group* of 25 or more (members and their families). Per person group fares, reflecting the 20 per cent reduction and using jet tourist round trip to Boston, are Chicago $81.40, Cincinnati $71.70, Denver $163.70, Los Angeles $243.60, Pittsburgh $53.80, San Francisco $243.60, and St. Louis $100.00 (a 5 per cent transportation tax to be added to all fares).

Members interested in the group flight plan should write to the Secretary, American Economic Association, 629 Noyes Street, Evanston, Illinois before August 1, 1963.

Announcements

Radio Free Europe announces the establishment of a visiting professorship at its Munich headquarters. This position will make it possible for a highly qualified professor to spend six months to one full year in Munich for research on internal Eastern European and Soviet affairs, foreign relations concerned with Eastern Europe, or studies in the application of the methodological and analytical techniques of the social sciences to Eastern Europe.

Applicants are invited to present a specific program for research either in a substantitive topic dealing with one or several of the nations of East Europe or the Soviet Union or for making use of methodological and analytical techniques which may be usefully applied to the work of Radio Free Europe. Other things being equal, research topics dealing with contemporary matters of direct interest to the broadcasting operations of Radio Free Europe will be accorded preference. Application forms and further information may be obtained from Mr. Charles B. Kaufmann, Jr., Radio Free Europe, 2 Park Avenue, New York, or Dr. R. V. Burks, Radio Free Europe, 1 Englishcher Garten, Munich 22, Germany.

The National Science Foundation announces that the next closing date for receipt of basic research proposals in the social sciences is October 1. The closing date for proposals for renovation and construction of graduate-level research facilities is August 1. Inquiries or proposals requesting support should be addressed to the National Science Foundation, Washington 25, D.C.

The National Institute of Social and Behavioral Science, in collaboration with the Section on Social and Economic Sciences of the American Association for the Advancement of Science, will hold sessions for contributed papers at the annual meeting of the AAAS in Cleveland, December 26-31, 1963. Association members interested in presenting a paper at these sessions should forward titles and abstracts not later than September 10 to Donald P. Ray, Director, National Institute of Social and Behavioral Science, 863 Benjamin Franklin Station, Washington 4, D.C. Papers should be based on research current or recently completed by the author. The American Economic Association is an affiliate member of the Section on Social and Economic Sciences of the AAAS.

The Industrial College of the Armed Forces extends an invitation to qualified civilians in business, industry, and the professions to enroll in its graduate-level correspondence course, "The Economics of National Security."

Operating under the direction of the Joint Chiefs of Staff, the Industrial College conducts courses of study in the economic and industrial aspects of national security and in the management of resources. The correspondence course extends the facilities and program of the College to those who are unable to receive resident instruction. Qualified persons interested in taking the course may apply directly to the Commandant, Industrial College of the Armed Forces, Fort Lesley J. McNair, Washington 25, D.C., Attention: Correspondence Course Division.

The Office of the Comptroller of the Currency announces the forthcoming publication of a new journal of banking and monetary economics, *The National Banking Review*. Manuscripts will be accepted on the basis of traditional standards of scholarship and in no case will bear the endorsement of the Comptroller of the Currency. Address all correspondence to Deane Carson, Consultant to the Comptroller, Office of the Comptroller of the Currency, Treasury Department, Washington 25, D.C.

Deaths

Clyde Aitchison, Washington D.C., January 16, 1962.

James A. Fitzgerald, professor emeritus of banking and insurance, dean emeritus, College of Business Administration, University of Texas, January 11, 1963.

Virgil L. Hurlburt, professor and USDA collaborator, department of economics and sociology, Iowa State University, November 30, 1962.

H. P. B. Jenkins, professor of economics, College of Business Administration, University of Arkansas, January 26, 1963.

Maurice Leven, McLean, Virginia, May 27, 1962.

Robert C. Shackelford, instructor, department of economics, University of Illinois, February 23, 1963.

Retirements

John C. Clendenin, professor emeritus, School of Business Administration, University of California, Los Angeles, January 1963.

Don M. Dailey, director of research, United States Savings and Loan League, Chicago, December 1962.

Roy L. Garis, emeritus professor of economics, University of Southern California, June 1963.

Charles A. Gulick, University of California, Berkeley, July 1963.

Miles Jones, professor emeritus, Graduate School of Business, University of Pittsburgh, April 1963.

C. L. Van Sickle, professor emeritus, Graduate School of Business, University of Pittsburgh, April 1963.

Visiting Foreign Scholars

Sukhamoy Chakravarty, Presidency College, Calcutta, India: visiting associate professor of economics, Massachusetts Institute of Technology.

Jung-Chao Liu, Taiwan National University: visiting professor of economics, Southern Methodist University, 1963-64.

Leonard Minkes, University of Birmingham: visiting lecturer, San Diego State College, summer 1963.

Jan Sandee, Central Planning Bureau, The Hague, Netherlands: visiting professor of economics, Massachusetts Institute of Technology.

Alan H. Williams, University of Exeter, England: visiting lecturer, Princeton University, 1963-64.

Taro Yamane, Aoyama Gakuin University: visiting lecturer in economics, San Diego State College, summer and fall 1963.

Promotions

Albert K. Ando: associate professor of economics, Massachusetts Institute of Technology.

Ronald G. Bodkin: assistant professor of economics, Yale University.

J. Howard Craven: vice president, Bank of America.

Emmanuel M. Drandakis: assistant professor of economics, Yale University.

Charles F. Eyre: assistant professor of finance, School of Business Administration, University of Miami.

James W. Friedman: assistant professor of economics, Yale University.

Edward Y. George: associate professor of economics, Bentley College of Accounting and Finance.

Challis A. Hall: professor of economics, Yale University.

Joseph E. Haring: associate professor of economics, Occidental College.

George Macesich: professor of economics, Florida State University.

Wilbur R. Maki: professor, department of economics, Iowa State University.

Edwin S. Mills: professor of political economy, Johns Hopkins University.

John M. Montias: associate professor, department of economics, Yale University.

Lawrence Nabers: professor of economics, University of Utah.

Arthur M. Okun: professor of economics, Yale University.

Edmund S. Phelps: associate professor, department of economics, Yale University.

Richard L. Porter: professor of economics, Marquette Unversity.

Reed C. Richardson: professor of economics, University of Utah.

Barnard Seligman: asistant professor of finance, New York University.

Robert E. Smith: associate professor of economics, University of Utah.

Larkin B. Warner: associate professor of economics, Oklahoma State University.

Stanislaw Wasowski: associate professor, department of economics, Georgetown University.

Robert D. Weintraub: associate professor, department of economics, The City College of New York.

Administrative Appointments

Mark Blaug, Yale University: senior lecturer and director, Research Unit in the Economics of Education, Institute of Education, London University.

Raymond W. Coleman, West Virginia University: dean, College of Business Administration, University of Illinois, Chicago.

Arthur S. Fefferman, Office of Tax Analysis, U.S. Treasury Department: director, economic analysis, American Life Convention, Washington, D.C.

Edward Y. George: director, Bureau of Business and Economic Research, Bentley College of Accounting and Finance.

Meredith B. Givens, State College, Pennsylvania: director, Office of Research in Economics and Science, Bureau of Intelligence and Research, U.S. State Department, Washington, D.C.

W. Summers Johnson, Joint Economic Committee: staff director and clerk, Committee on Banking and Currency, House of Representatives.

Heinz Kohler: chairman, department of economics, Amherst College.

Cyril A. Lindquist, Rider College: professor and chairman, division of economics and business, Ouachita Baptist College, Arkadelphis, Arkansas.

John W. McConnell, New York State School of Industrial and Labor Relations, Cornell University: President, University of New Hampshire, February 1963.

Robert F. Risley: acting dean, School of Industrial and Labor Relations, Cornell University.

H. Edward Wrapp, Harvard University: professor of business policy and director, Executive Program, Graduate School of Business, University of Chicago, beginning fall quarter 1963.

Appointments

Earl Adams: assistant professor of economics, Amherst College.

Robert Adams: staff, Center for Regional Economic Studies and department of economics, University of Pittsburgh.

Albert Y. Badre, American University, Beirut: professor of economics, University of Iowa.

Solomon Barkin: deputy to director, Manpower and Social Affairs Directorate and director, Social Affairs Division, OECD, Paris.

C. Phillip Baumel: assistant professor, department of economics, Iowa State University.

David S. Ball, University of North Carolina: assistant professor of economics, Vanderbilt University.

Nicholas Baloff: assistant professor of production management, Graduate School of Business, University of Chicago.

Ragnar Bentzel: Irving Fisher research professor of economics, Yale University.

Sylvester E. Berki: assistant professor, department of economics, Cornell University.

G. Sadler Bridges, Southern Methodist University: assistant professor of economics, Texas A. and M. College.

Harold E. Bronson: instructor in economics, University of Saskatchewan.

Alan A. Brown: assistant professor of economics, University of Southern California.

John W. Browning: assistant extension specialist in marketing, department of agricultural economics, Rutgers—The State University.

Glen G. Cain, University of Chicago: assistant professor, department of economics, University of Wisconsin.

Frank W. Carlborg: instructor in statistics, Graduate School of Business, University of Chicago.

Willard T. Carleton, Federal Reserve Bank of St. Louis: assistant professor of finance, Graduate School of Business Administration, New York University.

Samuel B. Chase, Jr.: associate professor, department of economics, University of Maryland.

Herbert Chesler: staff, Center for Regional Economic Studies and department of economics, University of Pittsburgh.

Chulsoon Khang: assistant professor of economics, San Diego State College.

Roberto G. Cofino, University of Miami: economic consultant, Committee of Nine, Alliance for Progress Program, Pan American Union.

Charles L. Cole: assistant professor of economics, Occidental College.

Richard N. Cooper: assistant professor of economics, Yale University.

Jon Cunnyngham: assistant professor of economics, Columbia University.

Harry L. Davis: instructor in marketing, Graduate School of Business, University of Chicago.

Patrick B. de Fontenay: instructor in economics, Vassar College.

Bruce Duncombe: instructor, department of economics, Georgetown University.

Corwin D. Edwards: professor of economics, University of Oregon.

Alan Entine: assistant professor of economics, Columbia University.

Edward W. Erickson: instructor in economics, Vanderbilt University.

Morris Fabian: research associate, department of agricultural economics, Rutgers—The State University.

Louis M. Falkson: assistant professor, department of economics, Cornell University.

William C. Frederick: associate dean and associate professor of business administration, Graduate School of Business, University of Pittsburgh.

Erik Furubotn, Emory University: associate professor of economics, Harpur College.

Richard J. Gelson: instructor, department of economics, Georgetown University.

John M. Glasgow, University of Colorado: assistant professor of economics, Texas A. and M. College.

Michael D. Godfrey: research associate, Econometric Research Program, Princeton University.

Stephen M. Goldfeld: assistant professor of economics, Princeton University.

C. Hunter Green: director, Louisville Branch Federal Reserve Bank of St. Louis.

George Green: staff, Center for Regional Economic Studies and department of economics, University of Pittsburgh.

Herbert Grubel: assistant professor of economics, University of Chicago.

Lyle M. Hansen: visiting associate professor of economics, Williams College.

James A. Harris, Sr., Bureau of Economics, U.S. Federal Trade Commission: lecturer in economics, Howard University.

Walter G. Harter: extension farm management specialist, Ohio State University.

Michio Hatanaka, Princeton University: associate professor, department of economics, University of Rochester.

Ronald F. Hoffman, Johns Hopkins University: assistant professor, department of economics, University of Hawaii.

Paul Hohenberg, Massachusetts Institute of Technology: assistant professor of economics, Stanford University.

E. Philip Howrey: assistant professor of economics, Princeton University.

Robert M. Hurt: assistant professor of economics, Princeton University.

Donald J. Igo, RAND Corporation: economics and costing division, Research Analysis Corporation, Bethesda.

Sydney C. James: assistant professor, department of economics, Iowa State University.

Harry G. Johnson: visiting professor of economics, Indiana University, second semester 1962-63.

George M. Jones, Western Pacific Railroad: associate administrative analyst, staff, Office of the Legislative Analyst, State of California.

Richard D. Karfunkle, SKF Industries: economist, business economics section, textile fibers department, E. I. du Pont de Nemours, Wilmington, Delaware.

David Kaun: staff, Center for Regional Economic Studies and department of economics, University of Pittsburgh.

Robert Kautz: associate professor of economics, University of Saskatchewan.

Lawrence B. Krause, Yale University: senior staff, Brookings Institution, Washington.

Jerome R. LaPittus: acting assistant professor of economics, University of California, Los Angeles.

Richard I. Leighton, College of William and Mary: assistant professor of economics, Western Reserve University.

Melvin Leiman: assistant professor of economics, Harpur College.

Wolfram Liepe, W. R. Grace and Company: associate professor of economics, University of New Mexico.

Eugene L. Loren: instructor, Princeton University.

Patrick J. Lynch, Saint Joseph's College, Indiana: economics and costing division, Research Analysis Corporation, Bethesda.

Paul W. MacAvoy, Graduate School of Business, University of Chicago: assistant professor of economics, Massachusetts Institute of Technology.

Michael D. McCarthy: member, research staff, Institute for Defense Analysis.

Ruth P. Mack: visiting professor of economics, Yale University.

Robert R. McKenzie, University of California, Los Angeles: assistant professor of finance, Graduate School of Business Administration, New York University.

G. S. Maddala: assistant professor of economics, Stanford University.

Burton G. Malkiel: assistant professor of economics, Princeton University.

Allan B. Mandelstamm, Vanderbilt University: associate professor of economics, Michigan State University.

David Martin: assistant professor of economics, Harpur College.

Robert Melville, Ohio Wesleyan and U.S. House Committee on State Taxation: associate professor of economics, Harpur College.

John E. Moore: extension farm management specialist, Ohio State University.

James E. Mullins: instructor in economics, Marquette University.

Roy E. Murphy, Jr.: Varian assistant professor, department of economics, Stanford University.

Richard R. Nelson: economics department, RAND Corporation.

David E. Novack: assistant professor, department of economics, Cornell University.

Robert E. Olley: assistant professor of economics, University of Saskatchewan.

John M. Owen, Jr.: assistant professor of political economy, Johns Hopkins University.

Frederick A. Perkins: associate extension specialist in marketing, department of agricultural economics, Rutgers—The State University.

James L. Pierce: research staff economist, Yale University.

Murray E. Polakoff, University of Rochester: professor of finance, Graduate School of Business Administration, New York University.

Louis R. Pondy: assistant professor, Graduate School of Business, University of Pittsburgh.

William Poole: assistant professor of political economy, Johns Hopkins University.

Robert W. Rafuse: instructor, department of economics, University of Illinois.

John E. Rapp: assistant professor of economics, University of South Dakota.

James Robinson: assistant professor of economics, University of Missouri.

Hugh Rose, University of Rochester: associate professor, University of Toronto.

Robert L. Sandmeyer: assistant professor of economics, Oklahoma State University.

John C. Sawhill, New York University: assistant professor, department of economics and School of Commerce, University of Wisconsin.

Herbert E. Scarf: professor of economics, Yale University.

Frederic M. Scherer: assistant professor of economics, Princeton University.

James R. Schlesinger: economics department, RAND Corporation.

Richard T. Selden, Graduate School of Business, Columbia University: professor of economics, Cornell University.

Nelson L. Smith, Federal Power Commission and American Airlines: professor of business, Graduate School of Business, Columbia University.

Aubrey N. Snellings; research economics, Federal Reserve Bank of Richmond, Virginia.

Dufferin S. Spafford: instructor in economics, University of Saskatchewan.

Robert Sterling: assistant professor of accounting, Harpur College.

Louis H. Stettler, III: assistant professor of political economy, Johns Hopkins University.

Leon P. Sydor: instructor, Princeton University.

Norman W. Taylor: associate professor of economics, Franklin and Marshall College, Lancaster, Pennsylvania.

Henri Thiel: Ford Foundation visiting professor of econometrics and management science, Graduate School of Business, University of Chicago, spring quarter 1964.

Robert J. Vanhandel: instructor in business enterprise, Harpur College.

George J. Viksnins: instructor, department of economics, Georgetown University.

Hugh D. Walker: instructor in economics, Vanderbilt University.

Gerald I. Weber: instructor, department of economics, University of Illinois.

Jacob Weissman, University of Chicago: associate professor of economics, Hofstra University.

Morris Weisz, American University: Deputy Assistant Secretary of Labor, May 1962.

Joseph T. White: instructor, department of economics, Georgetown University.

Jeffry G. Williamson, Vanderbilt University: assistant professor, department of economics, University of Wisconsin.

Gordon C. Winston: assistant professor of economics, Williams College.

Robert W. Wolfson: logistics department, RAND Corporation.

Basil S. Yamey: Ford Foundation visiting professor of business economics and accounting, Graduate School of Business, University of Chicago, autumn quarter 1963.

Victor Zarnowitz, University of Chicago: member, research staff, National Bureau of Economic Research.

Leaves for Special Appointments

Martin J. Bailey, University of Chicago: Institute for Defense Analysis, 1963-64.

Deane C. Carson, Brown University: consultant to Comptroller of the Currency, Washington, D.C.

Paul T. Ellsworth, University of Wisconsin: Carnegie visiting professor, department of economics, University of Hawaii, 1963-64.

Stanley Engerman, Yale University: visiting assistant professor, department of economics, University of Rochester.

Karl A. Fox, Iowa State University: Iowa-Peru project on agrarian reform, AID, January-March, 1963.

John P. Henderson, Michigan State University: adviser in economics, University of Nigeria, Nsukka, academic year 1963-64.

James B. Hendry, Michigan State University: economist, Harvard University/Ford Foundation Advisory Group, East Pakistan, 1963-64.

John M. Hunter, Michigan State University: adviser, Institute of Economic Research, University of Cordoba, Argentina, academic year 1963-64.

Clarence D. Long, Johns Hopkins University: U.S. House of Representatives.

Millard F. Long, Vanderbilt University: visiting assistant professor of economics Kesatsart University, Bangkok, Thailand, academic year 1962-63; Vanderbilt University's Overseas Professorship Program, fall semester 1963.

Joseph P. McKenna, Boston College: School of International Studies, Johns Hopkins University, Bologna, Italy, until September, 1963.

James W. McKie, Vanderbilt University: visiting professor of economics, University of California, Berkeley.

G. David Quirin, University of British Columbia: visiting lecturer in business administration, University of Malaya, May-August, 1963; University of Singapore, September 1963-May 1964.

Sidney M. Robbins, Columbia University: chief economist, Securities and Exchange Commission, special assignment.

Bernard Schwartz, New York University: visiting professor of economics, Michigan State University, spring term 1963.

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Erik Thorbecke, Iowa State University: Iowa-Peru project on agrarian reform, AID, January-March 1963.

Willard L. Thorp, Amherst College: chairman, Development Assistance Committee, OECD, Paris, beginning June 1963.

William O. Thweatt, representative, Ford Foundation, Nepal: visiting associate professor of economics, Vanderbilt University, 1963-64.

John F. Timmons, Iowa State University: Iowa-Peru project on agrarian reform, AID, January-March 1963.

Alfred N. Watson, United States Rubber Company: visiting professor of marketing, Graduate School of Business, Columbia University.

Resignations

Robert L. Basmann: University of Chicago.

Gregory Chow: Cornell University.

Robert J. Connor: Graduate School of Business, University of Chicago.

Charles L. Cooper: University of Pittsburgh.

Raymond Franklin: Vassar College.

Michael C. Lovell: Yale University.

William A. Mauer: Tulane University.

Stephen W. Rousseas: Cornell University.

George Thatcher: chairman, department of economics, Miami University.

Gerhard Tintner: Iowa State University.

Miscellaneous

Frank H. Knight was named distinguished visiting scholar, Thomas Jefferson Center for Studies in Political Economy, University of Virginia, April 21—May 11, 1963.

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THE AMERICAN ECONOMIC REVIEW

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SURVEY OF INFLATION THEORY

By MARTIN BRONFENBRENNER AND FRANKLYN D. HOLZMAN*

> The economic stalactite of inflated demand has met a sociological stalagmite of upthrusting claims; and when stalactite and stalagmite meet and fuse in an icy kiss—I hope there is no geologist present to tell me I am talking through my hat—nobody on earth can be quite sure where the one ends and the other begins.
>
> —SIR DENNIS ROBERTSON [145]

Roll back the prices,
We'll save a barrel of mon',
We'll lick the prices,
Inflation's on the run,
Boom, boom, tarara,
Housewives, don't sigh,
Roll back the prices
In the pig's left eye!

American, *circa* 1943

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* The authors are, respectively, professors of economics at Carnegie Institute of Technology and Tufts University. Although they consulted freely with each other on all sections of the manuscript, Bronfenbrenner accepts primary responsibility for Parts I, II, and IV; Holzman for Parts III, V, and VI. The authors wish to express their appreciation to colleagues who commented on various parts of this manuscript, especially G. L. Bach, Harry Johnson, Allan Meltzer, Edmund Phelps, A. W. Phillips, and Robert M. Solow. They also offer grateful thanks to Judy Brown, who drew the charts, and to secretarial victims over successive drafts, Mrs. Alice Gallo and Mrs. Harriet Schweinsberg. Holzman would like to express his appreciation to the Social Science Research Council and to the Tufts University Faculty Research Fund for financial assistance in the writing of this paper. [This is the seventh in a series of survey articles for which the Rockefeller Foundation has provided support.—*Editor.*]

Since 1945 the geographical extent and temporal stubbornness of *The Great Inflation*[1] have shaken many economists' faith in the orthodoxies of preceding generations. Neo-orthodoxies, including restatements, are only dimly in evidence. Our survey is accordingly more of a guide through chaos than a history of received doctrine or a systematic critique of a few rival positions.

We limit the subject in several ways, apologizing to those with interests amputated by the limitations. (1) We are concerned with the positive theory of open inflation, excluding such policy issues as whether a given degree of inflation is a Good Thing or not, what measures should be taken to bring it about (if a Good Thing) or to prevent it (if it is not). At the same time, we consider at least peripherally the relationships between inflation on the one hand and other economic quantities such as the level of employment, the rate of economic growth, and the functional distribution of income. (2) We omit the international issues involved in trade, payments, and exchange rates between countries inflating at different velocities and countries with different policies toward inflation. (3) We concentrate primarily on developments in the postwar period.[2] (4) Finally, we recognize that many developments in both monetary and expenditure theory, not specifically concerned with price level changes, have had implications for inflation theory. Some of these developments are taken into account in what follows, but we shall concentrate our attention on writings which center on questions of rising wages and prices.[3]

The first section will be devoted to definitions of inflation. Section II will consider types of inflation theory which emphasize demand-pull (buyers' inflation). In Section III theories which emphasize cost-push (sellers' inflation, administered inflation, wage-price spiral, etc.) are discussed. Section IV is devoted to quantitative testing of inflation theories in the postwar period. Section V outlines briefly the stabilizing and destabilizing factors in an inflation and factors that determine the "speed" of inflation. In Section VI the effects of inflation on the distribution of wealth and income are considered.

I. *Sources and Definitions of Inflation*

A. *Sources of Inflation*

One key issue is the identification of the fundamental source or sources of inflationary pressure. Does inflation arise from the demand side of the goods, factor, and asset markets, from the supply side, or from some combination of the two—the so-called mixed inflation? Does the answer differ in the short and the long run, in developed and underdeveloped economies, with differences in countries' economic structures, in creeping, trotting, or galloping inflations, in the 1950's and the 1940's?

---

[1] Brown's volume of this title [24] covers the period 1939-51 on a world-wide basis.

[2] For brief surveys of earlier theories, see monographs by Biacabe, B. Hansen, and Paunio [15] [68] [70] [126a].

[3] On monetary theory, see Johnson [81]; on consumption theory, see Ferber [52].

Answers to such questions are not only academic, but have important policy implications. Thus, if an inflation is due to excess demand, it is generally considered controllable by demand-reducing fiscal and monetary policies. If it is due largely to forces of cost and supply, it may not yield to such easy therapy. Fiscal and monetary policies, that is to say, may cure a cost inflation only at the price of unemployment and slower growth, either temporary or permanent. Many cost-inflation theorists therefore propose mitigation rather than elimination of inflation. Others rely on voluntary restraints in pricing and bargaining. Still others propose disequilibrium systems of direct control (wage- and price-fixing, rationing, allocations) if high levels of employment and growth are to be reconciled with price stability.

The four diagrams of Figure 1 illustrate different types of inflationary positions in a closed economy.[4] The horizontal axes all represent real national income or GNP; the vertical axes represent "the" price level with no direct controls. Aggregate demand curves D are drawn sloping downward. This slope illustrates Patinkin real-balance effects with a constant nominal money supply and static price expectations. In some circumstances inflation-engendered redistribution of income and wealth also leads to a downward slope of D (see Section VI). Aggregate supply curves S are drawn sloping upward—in some full-employment cases, as vertical lines. Shifts in aggregate supply functions result from changes in money-profit margins and money-wage rates. Interdependence between D and S functions may exist; it is considered explicitly in Figure 1d.

Pure-demand-inflation theorists tend to assume that at some income level Y_0 (Figure 1a), corresponding to full employment, the aggregate supply function becomes completely inelastic, as drawn. No income level lower than Y_0 is a full-employment one, and increases in demand beyond D_0, as to D_1 and D_2, raise the price level from P_0 to P_1 and P_2.

Pure-supply-inflation theorists, on the other hand, maintain that, in societies of oligopolies, unions, and other pressure groups, the aggregate supply curve moves upward, as from S_0 to S_1 and S_2 in Figure 1b,[5] whatever happens to aggregate demand. The full-employment real-income level Y_0 is then maintainable only at rising price levels (P_0, P_1, P_2). Unemployment is the cost of holding prices closer to P_0 or to any previously achieved level. If aggregate demand, for example, is held at D_0, real income falls to Y_1 and Y_2. Even so, the price level rises from P_0 to P_1' to P_2'. If the government is committed to full employment, the path of temporary equilibrium points will be something like *a-b-c-d-e.*

---

[4] More extended classifications of inflation theories are found in several sources, such as Holzman [77], Machlup [109], and Phelps [130].

[5] Supply shifts might also result from rising import prices in an open economy.

One variety of mixed-inflation theory denies for several reasons, one

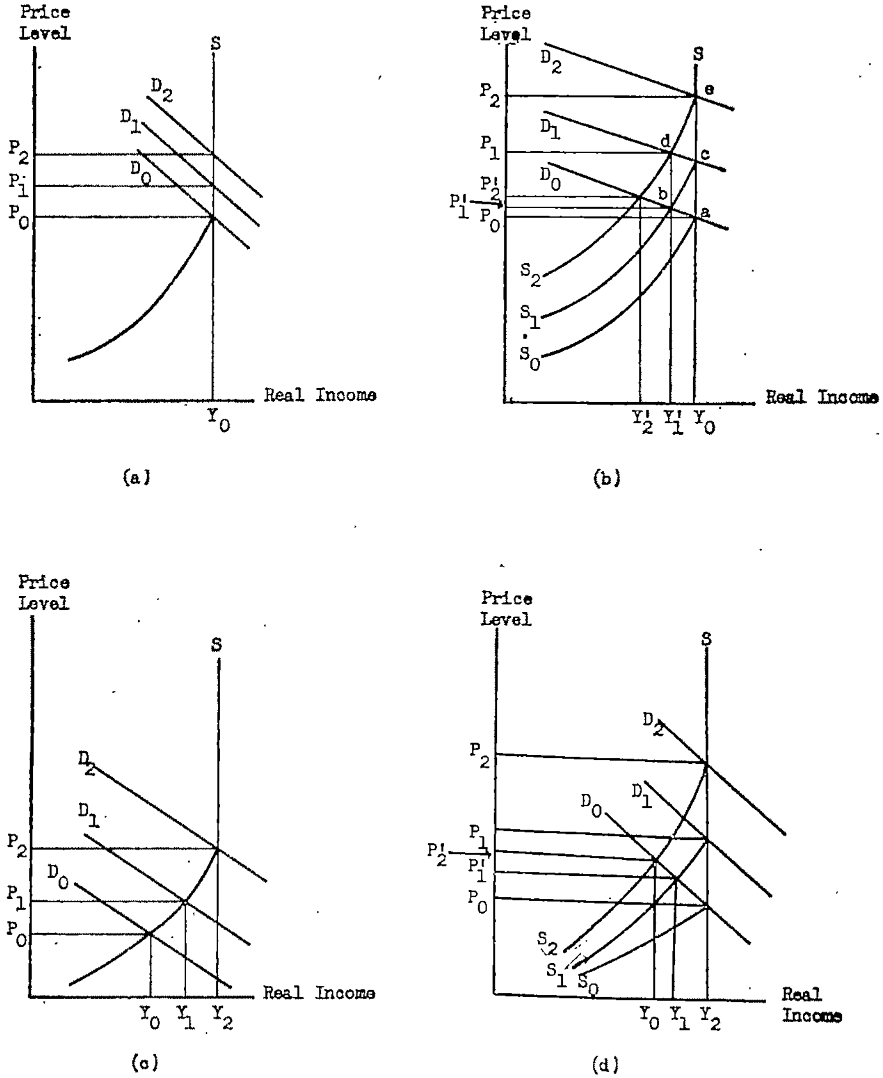

FIGURE 1

of them money illusion,[6] that aggregate supply is price-inelastic at full employment. In Figure 1c, (Y_0, P_0), (Y_1, P_1), and (Y_2, P_2) are all full-employment positions in that no involuntary unemployment exists. The first corresponds to A. P. Lerner's "low full employment" with substantial voluntary unemployment [99, Ch. 13], and the last to his "high full employment" with little or none. The region between low and high full employment was called by Keynes "semi-inflation" in contrast to the true or full inflation of Figure 1a. Mixed-inflation theorists usually think society prefers the couple (Y_2, P_2) to the other alternatives, even when all three are full-employment positions. In this type of mixed-inflation theory, inflation does not continue after (Y_2, P_2) is reached. In this respect, the solution is related more closely to demand than to cost inflation. As we shall see, this model is mainly relevant to the possible growth-inducing effects of inflation.

Another mixed theory is illustrated in Figure 1d, where interrelated shifts in S and D induce unemployment inflation. This differs from the pure cost inflation of Figure 1b in that the extent of the shift in supply increases as unemployment declines. Whether demand rises from D_0 to D_1 and D_2 or remains at D_0, supply moves from S_0 to S_1 and S_2; the shift is larger, the closer the economy approaches full employment at Y_2. In this version full employment is a more unstable position, and more difficult to achieve by rapid inflation than under pure cost-push, because the induced supply shifts are larger at low levels of unemployment. On the other hand, price stability may be possible at the cost of less unemployment in this model than in the one represented in Figure 1b. The model represented in Figure 1d may also be more realistic than that of Figure 1b; casual empiricism suggests that cost-push is often moderated by the level of demand.

Still other mixed theories will be discussed below;[7] these involve disaggregation and are less easily depicted in two dimensions.

B. *Definition of Inflation*

In the preceding diagrams and discussion, inflation has meant a rise in "the" price level, i.e., a depreciation of the monetary unit. This is the most obvious, but by no means the only, definition. Indeed, disagreement over the definition of the term is symptomatic of the confusion in inflation theory. We list some issues slurred over by definitions in terms of "the" price level. (No such list can be exhaustive.)

---

[6] We define money illusion as an unjustified expectation that price levels will remain constant or that short-run changes will be reversed in the longer run.

[7] One type is sectoral or demand-shift inflation, which depends on rigid wages and prices in declining sectors and flexible wages and prices in expanding sectors [154]; another simply divides the economy into two interrelated sectors, one of which operates with flexible prices, the other with cost-determined markup prices [43] [134] [135] [158].

1. Which of an infinity of possible price levels is meant? An implicit national income or GNP deflator has recently come into favor. Such deflators were largely unknown before 1939, and reliance was usually placed on wholesale price indexes. In many cases price indexes will show widely differing rates of change, and the differences may become a political issue.[8]

2. What allowance should be made for new products, quality changes, shifts in consumption habits, and other factors considered in the utility analysis of price index numbers? In the United States, an AFL-CIO task force relied on these factors during World War II in alleging that the official indexes understated the magnitude of wartime inflation. This was the Meany-Thomas Report [176]. In the late 1950's, a National Bureau of Economic Research task force, headed by Stigler, suggested on similar grounds that the indexes have overstated the magnitude of U.S. inflation subsequent to the Korean War [166].

3. When price controls are in effect, should the indexes be based on black market as well as official prices? Even in the absence of organized black markets, what allowances, if any, should be made for lessened availability of goods under control? The terms "repressed" and "suppressed" inflation have been used to refer to these cases,[9] as distinguished from "open" inflation when no anti-inflationary direct controls are in effect. Are "repressed" and "suppressed" inflation types of inflation or alternatives to inflation?

4. Assume that commodity taxes and subsidies are used widely, as in contemporary France. Should prices be taken gross or net of such taxes and subsidies? Suppose also that, as in France before 1959, subsidies are used to hold individual prices down, with commodities in the official indexes being given larger subsidies (or lower taxes) than others. How reliable are price indexes under this sort of *a posteriori* sampling bias?

5. Assume that, following destruction and disruption due to war, flood, or earthquake, there is a sharp fall (i.e., shift to the left) in the aggregate supply curves of Figure 1. Is the result to be called inflationary when no increase in aggregate demand has occurred? Does it make

---

[8] The Ikeda Government in Japan, for example, relied on wholesale price indexes to show that the rapid economic growth of 1960-62 was not inflationary, while the Opposition relied on consumer price indexes and national income deflators to show the reverse. Quite generally, quoting the U.S. Council of Economic Advisers [48, p. 167], "a period of stability in the wholesale price index tends to be a period of slow rise in the consumer price index and in the implicit price index for GNP." This is because of rises in prices of services and in public payrolls.

[9] These terms, due to Röpke, are often used synonymously. Where they are differentiated, we suppose that direct controls remain effective only in the short run when an inflation is *repressed*, but that these controls remain effective indefinitely when an inflation is *suppressed*. Compare Charlesworth [29].

any difference if the same destruction and disruption simultaneously create unemployment?

6. Consider Figure 1c, where a rise in the price level leads, at least in the short run, to an increase in real output and employment. Is this really inflationary, or should the term be limited to instances in which no significant increases in output or employment occur?

7. Suppose that, as probably happened in the 1920's, technical progress has brought about a widespread reduction in production costs with no corresponding reduction in prices. Were the resulting increases in money wages and gross profits inflationary?

8. Does it make a difference whether price increases have been anticipated correctly (so as to minimize their effects on the distribution of income), or whether they are unanticipated or anticipated incorrectly? As an extreme case of Lerner's [99a] "expectational" definition of inflation, suppose that prices fell by 5 per cent when a fall of 10 per cent was anticipated generally. Is this inflation?

9. Does it make a difference, as suggested by James [66, p. 3], whether a price level change is or it not regarded as permanent and irreversible?

Our answers to these questions may be affected by extraneous issues such as whether or not we consider inflation, or tight money, or direct controls to be Good Things. If one considers inflation, for example, a Bad Thing, it is difficult to avoid a question-begging definition which will render Good Results somehow noninflationary, even when accompanied by price increases.

For those unwilling to accept the evidence of price indexes per se, a number of alternative definitions are available. By no means all of these are new, and, once again, the list is not intended to be exhaustive.

1. Inflation is a condition of generalized excess demand, in which "too much money chases too few goods."

2. Inflation is a rise of the money stock or money income, either total or per capita.[10]

3. Inflation is a rise in price levels with additional characteristics or conditions: it is incompletely anticipated; it leads (via cost increases) to further rises; it does not increase employment and real output; it is faster than some "safe" rate; it arises "from the side of money"; it is measured by prices net of indirect taxes and subsidies; and/or it is irreversible.

4. Inflation is a fall in the external value of money as measured by foreign exchange rates, by the price of gold, or indicated by excess demand for gold or foreign exchange at official rates.

---

[10] These definitions, generally in disfavor in the period since World War II, are associated with the neutral-money theorists of the 1920's and 1930's, particularly in Austria.

An unusually comprehensive definition (including cost-push in the factor markets and excess demand in output and factor markets) is Turvey's [170, pp. 534 ff.]. He calls inflation "the process resulting from competition in attempting to maintain total real income, total real expenditure, and/or total output at a level which has become physically impossible, or attempting to increase any of them to a level which is physically impossible." In what follows, we avoid choice between alternative definitions in the hope that most of them will lead to similar qualitative results.

II. *Demand Inflation*

A. *Initial Orthodoxies*

During World War II, Smithies maintained [164] that: "The writer on inflation is fortunate in that his subject is generally well understood by economists."[11] Nevertheless, two positions were rival claimants to the title of orthodoxy. The quantity theory of money, with centuries of tradition behind it, imputed price level changes to changes in the quantity of money (a stock). An increase in this stock generated excess supply of money at existing prices and interest rates, meaning excess demand for nonmonetary assets. The other position, following Keynes' *How to Pay for the War* [90], stressed the level of national expenditure (a flow) as the main determinant of the price level, with increasing expenditures opening an inflationary gap after full employment was attained.

Most pre-Keynesians used the quantity theory to assert long-run proportionality between the quantity of money and the price level, although neither was defined unequivocally. This theory fell into disrepute during the 1930's because of sharp changes in the velocity of circulation, which it had treated as nearly constant; in other words, money as a proportion of income and assets or of the volume of real income and transactions actually varied extensively. However, "under full employment" and "over the long run," the money supply was still considered the major single determinant of the price level. A restatement of the quantity theory for the short run was found in the work of Warburton [180], who showed that changes in the quantity of money led changes in velocity (interpreted as deviations from statistical trend lines). Warburton regarded changes in the quantity of money as the causal force in fluctuations of the price level, partly directly $[\Delta M \rightarrow (\Delta Y \text{ and } \Delta P)]$, partly indirectly $[\Delta M \rightarrow \Delta V,\ \Delta V \rightarrow (\Delta Y \text{ and } \Delta P)]$.

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[11] A contrary view has been expressed by Ugo Papi: "La théorie de l'inflation est un des points les plus faibles de la pensée économique contemporaine" [15, p. i].

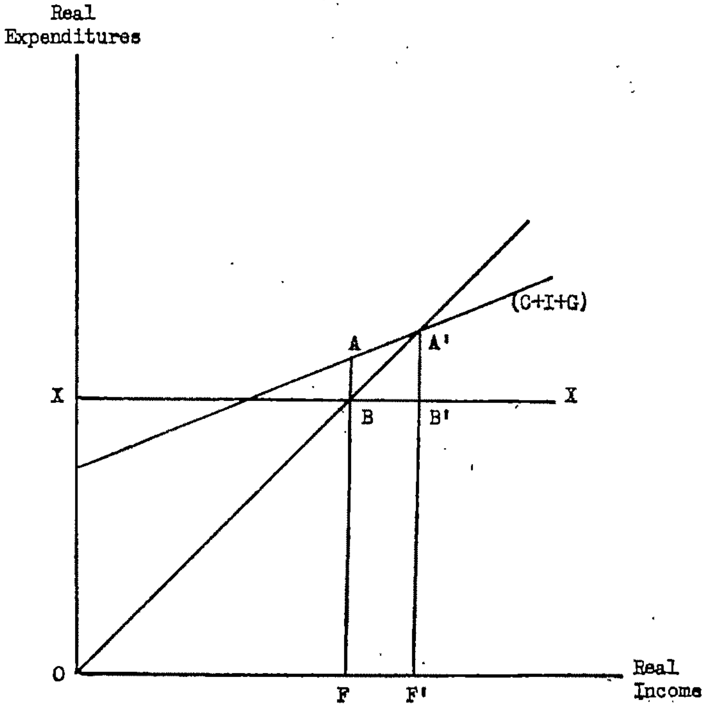

Figure 2

Keynesian inflation theory stressed income and expenditure flows rather than monetary stocks. The theory is presented in elementary form in a conventional "Keynesian cross" (Figure 2). Here, with standard notation, $C + I + G$ may be called an aggregate expenditure curve,[12] while OX represents aggregate capacity at full employment in the prices of the beginning of the period, and OF is full-employment income in the same units. If desired expenditures at OF exceed aggregate capacity OX, the differential, AB, may be called an inflationary or expansionary gap (a deflationary or contractionary gap if OX exceeds OF).[13] The gap $A'B'$, also shown in the diagram, can be proved equal to AB times the "standard" multiplier $1/(1 - c)$, where c is the marginal propensity to spend on domestic goods. The degree of inflation is assumed, in simple formulations, to vary directly with AB/OF and $A'B'/OF'$. A cut of AB in aggregate expenditures through a reduction of G, an increase in taxes, or a decrease in the money supply presumably eliminates the inflationary pressure. The intersection of the expenditure curve with the aggregate capacity-full employment point (*B* on Figure 2) is sometimes called "full equilibrium," since the price level is stable at full employment.

---

[12] $C + I + G$ is also frequently called an aggregate demand curve, despite the absence of a price axis.

[13] For various concepts of the inflationary gap, see Salant [150].

Difficulties with this presentation become apparent when we label the axes of Figure 2. Suppose, for example, that income and expenditures are in real terms, as drawn in Figure 2. There is then, it would appear, no such thing as an equilibrium price level; inflation continues indefinitely, so long as the gap persists. If, on the other hand, income and expenditures (but not the capacity line) are in money terms, the stability of the aggregate expenditure curve ($C + I + G$) as a function of income alone is highly questionable unless the inflation is both small and short. The instability (or improper specification) of the expenditure curve in monetary terms was not recognized generally until the fiasco of the pessimistic low-employment postwar forecast for 1946-47, which rested heavily upon such functions in current prices. (Subsequent work has shown the Keynesian expenditure to be imperfectly stable over the long period, even when deflated.).

B. *Postwar Developments*

Starting from the imperfection and incompleteness of the prewar theory, extensions of inflation theory occurred with rapidity. Little of the new theoretical extensions of the quantity theory of money has been concerned with inflation (other than hyperinflation). We shall therefore stress the expenditure-theory side, referring the reader to H. G. Johnson [81] for a parallel account of developments in monetary theory.

One important new development, however, has been the introduction by Friedman [59] of a quantifiable "expected rate of price change" as a major determinant of the velocity of circulation. The relative stability of the resulting velocity function, while velocity itself increased tremendously, was demonstrated by Cagan's study of seven hyperinflations [26], but no similar demonstration has been published for slower inflations. A typical postwar monetarist position on inflation is provided by Friedman [57, pp. 1-2], who maintains that the money stock rather than the income flow determines both the price level and the level of economic activity:

> The past three decades have seen first a sweeping revolution against previously accepted economic thought about the role of monetary factors in economic change and then a counterrevolution that is still incomplete but promises to be no less sweeping. Before the great depression of the early 1930's, accepted economic doctrine attached great importance to the stock of money as a determinant of the level of money income and of the price level. The great depression spawned a revolution in ideas. Though, on a retrospective examination, the depression is a tragic testimonial to the potency of monetary factors . . . the failure of the monetary au-

> thorities to stem the depression was taken as evidence that they could not have done so. The view became widespread that "money does not matter," that the stock of money is a purely passive concomitant of economic change and plays no independent part except as it may affect a limited range of market interest rates. . . .
>
> Two forces combined to produce a counterrevolution in ideas. One was . . . scholarly criticism and analysis of the ideas of John Maynard Keynes, the chief architect of the intellectual revolution. The other, and more obvious . . . was the brute force of events. Many countries in the postwar period . . . pursued "cheap-money" policies. Every such country experienced either open inflation or a network of . . . controls designed to suppress the inflationary pressure. In every case, the stock of money rose as a result of the cheap-money policies and so did prices, either openly or in whatever disguise was most effective in circumventing the controls. No country succeeded in stemming inflation without adopting measures directed at restraining the growth of the stock of money. And every country that did hold down the growth of the stock of money succeeded in checking the price rise.

Only a few empirical studies are available which bear on these issues. They include Patinkin's analysis of real-balance effects in damping the Israeli inflation [124] [125], Cagan's comparative study, mentioned above, of seven hyperinflations [26], which uses distributed lags of price changes as proxies for price-level expectations, and E. Lerner's account of inflation in the Southern Confederacy [101], which stresses the effectiveness of currency reform even in the militarily hopeless situation of the later stages of the Civil War.

1. On the income-expenditure side, there first developed in the 1940's a body of analysis that made substantial use of difference equations to recast Keynesian theory in dynamic terms. This analysis was designed primarily to ascertain the determinants of the speed and stability of the inflationary process. Will such a process, once under way, peter out when a new and higher price level has been reached, or will it continue indefinitely as hyperinflation until the financial system breaks down?[14]

Combinations of these dynamic issues gave rise to several contributions, usually introducing elements of cost along with demand inflation. Among the earliest were articles by Koopmans [95] and Smithies [164] during World War II; the immediate postwar inflation inspired the essays of Duesenberry [43], Holzman [76], Reder [136], Simkin [158], Turvey [169] [170], and others. The creeping inflation that followed the Korean War stimulated contributions by Paunio [126a], Pitchford [134] [135], and Lovell [107], among others. In general,

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[14] For a more exhaustive catalogue of the stabilizing and destabilizing elements in the inflationary process, see Section V.

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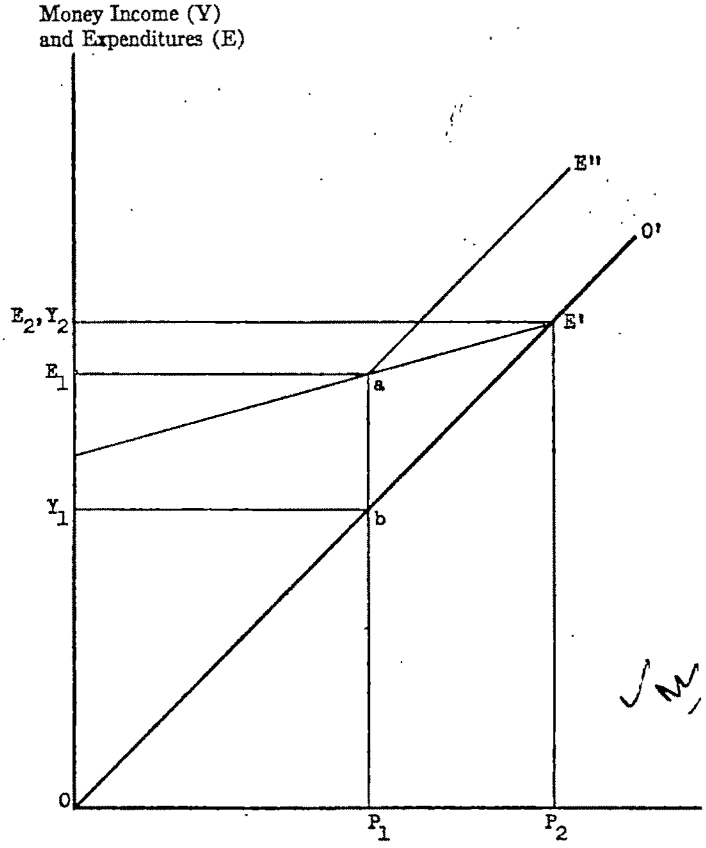

FIGURE 3

the market for productive services (factors) separately fr for goods. His theory is applicable to cases of suppre structural unemployment, unemployment-inflation, etc does not include monetary factors.[17] In Hansen's view, mand for goods or "goods gap" should be measured se the "factor gap" that relates primarily to labor. (Both mated in monetary units at controlled prices or at pri period.) For a full inflation, there must be both a good gap, each involving positive excess demand. If there is a gap combined with a negative factor gap, for example, t

---

[17] As B. Hansen, following Walras, points out elsewhere [70], a compl take account not only of the factor and goods markets, but also of the s for cash and for financial claims, respectively. It is also interesting to example of an inflation basically generated by excess demand in the la Soviet Union of the 1930's [78].

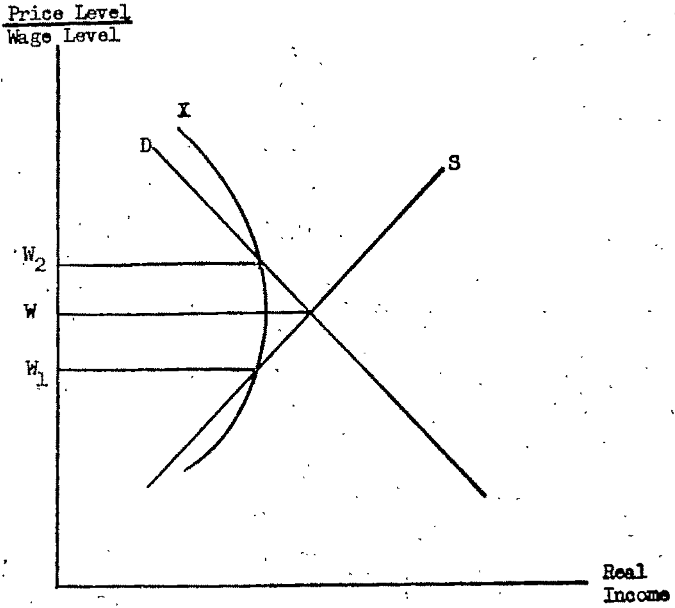

FIGURE 4

less an inflation than the consequence of disequilibrium—meaning the overpricing of productive services.[18]

For an introduction to Hansen's dynamics in the full-inflation case, we use as Figure 4 a diagram adapted from Ackley [2, p. 436]. The horizontal axis represents real national income supplied and demanded, and the vertical axis the ratio of the price level to the money-wage rate —i.e., the reciprocal of the real-wage rate. Curve *D* is an aggregate demand curve. Its downward slope implies, first, that total labor income varies with real-wage rates and, second, that workers' marginal propensity to spend is higher than that of profit receivers. Curve *S* is a hypothetical aggregate supply curve, hypothetical in the sense of representing what employers would be willing to produce with unlimited supplies of labor. It is drawn conventionally, sloping upward as the real-wage rate falls. Curve X, which first rises and then bends back as the real-wage rate falls, introduces the labor supply as a limitation of the economy's capacity to produce at full employment. After a certain point, it suggests, workers respond to higher real wages by consuming more leisure as well as more goods.

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[18] As an example of the different analytical results which stem from considering factor markets explicitly in inflation analysis, take the case of an increase in labor productivity. The primary effect in commodity markets of an increase in labor productivity is deflationary. However, since an increase in labor productivity increases the demand for labor and is therefore inflationary in labor markets, its total impact is indeterminate.

Hansen also argued very strongly in 1957 [69] for the importance of disaggregation in the labor market. Wage-push inflation analyses which utilize the level of unemployment as an unambiguous indicator of an excess supply of labor are apt to yield biased results for two reasons. First, to the extent that there are unfilled vacancies, excess supply in the aggregate might be reduced or even eliminated. Second, to the extent that labor is not homogeneous and the flexibility of wages to excess demand or supply differs between labor submarkets, it is important to measure inflationary potential by weighting the difference between unfilled vacancies and the number unemployed in each such submarket by its flexibility ("reaction") coefficient. Enthoven [49] and Paunio [126a] accord similar disaggregative treatment to prices in product markets.

In Figure 4 the Hansen goods and factor gaps are both measured horizontally, the goods gap by (D-X) and the factor gap by (S-X). They are both positive between W_1 (high real wages) and W_2 (low real wages). Just above W_1, the goods gap is large and the factor gap small; just below W_2, the opposite is the case. The assumed rates of both price and wage inflation are given by Samuelson's conditions:

$$\frac{dp}{dt} = f_p(D - X) \quad \text{and} \quad \frac{dw}{dt} = f_w(S - X).$$

Consider a position slightly above W_1. The factor gap is small, so money wages rise slowly. The goods gap is large, so prices rise rapidly. This lowers real wages to W. At W, the relative speed of movement is reversed, and real wages rise. Meanwhile, of course, both absolute prices and money wages continue to rise; however, real wages may fluctuate between $1/W_1$ and $1/W_2$. The inflation, open or suppressed, continues until D shifts to the left, or X shifts to the right. Neither of these movements will necessarily occur in time to avoid hyperinflation. (We would reach similar qualitative results from an initial position slightly below W_2, with a smaller goods gap and a larger factor gap.)

As for structural unemployment, mentioned briefly above, consider Figure 5, adapted from Fukuoka [60]. A more general treatment is found in Dorfman, Samuelson, and Solow [39, Ch. 2]. The problem is the availability of productive inputs only in unsuitable proportions. Thus, let L and C be stocks of labor and capital in real terms. Let the lines A and B represent limits of substitutability between them, sometimes called ridge lines. This means that, with labor services free, the economy would produce at some point on A, while with capital services free, production would be at some point on B. The marginal productivity of capital is negative above line A, and the marginal productivity of labor is negative to the right of line B. Let the point P_0, with co-ordinates L_0, C_0, represent the endowment of an economy attempting development or reconstruction with a shortage of capital or after widespread capital destruction. In such cases, increases in aggregate demand serve only to increase the prices of most final products, capital instruments, and labor,[19] without increasing total output and without decreasing unemployment below LL_0 on Figure 5.

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[19] We recognize that the "shadow wage," under these circumstances, is zero.

3. We have mentioned and illustrated (in Figure 1c) the possibility of an upward-sloping aggregate supply function with a positive price elasticity. Not all points on this supply function correspond to full capacity. Even if they did so correspond, however, the resulting capacity functions might have an elasticity, positive or negative, depending on the reactions of voluntary unemployment, private investment, industrial conflicts, and the availability of foreign exchange to changes in the price level. A number of writers, from mercantilist times to the present day, seem to have considered aggregate supply functions as nonvertical even at full employment. This implies that capacity functions like the *X* curve of Figure 2 are nonhorizontal even when the degree of involuntary unemployment is constant at a low level. Patinkin [126] was among the earliest postwar writers to use an upward-sloping capacity function under the explicit assumption of full employment [126]. Bronfenbrenner [21] adopted a nonhorizontal capacity function but drew it rising to a maximum and subsequently falling (*X* in Figure 6). Here the axes represent promised real income and realized real output. The effect of extravagant promises (by "em-

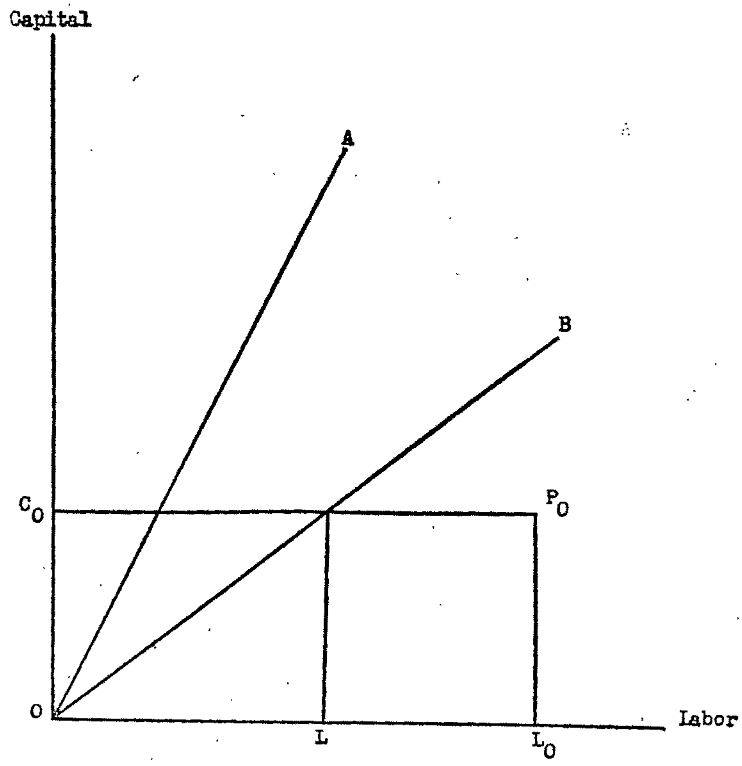

FIGURE 5

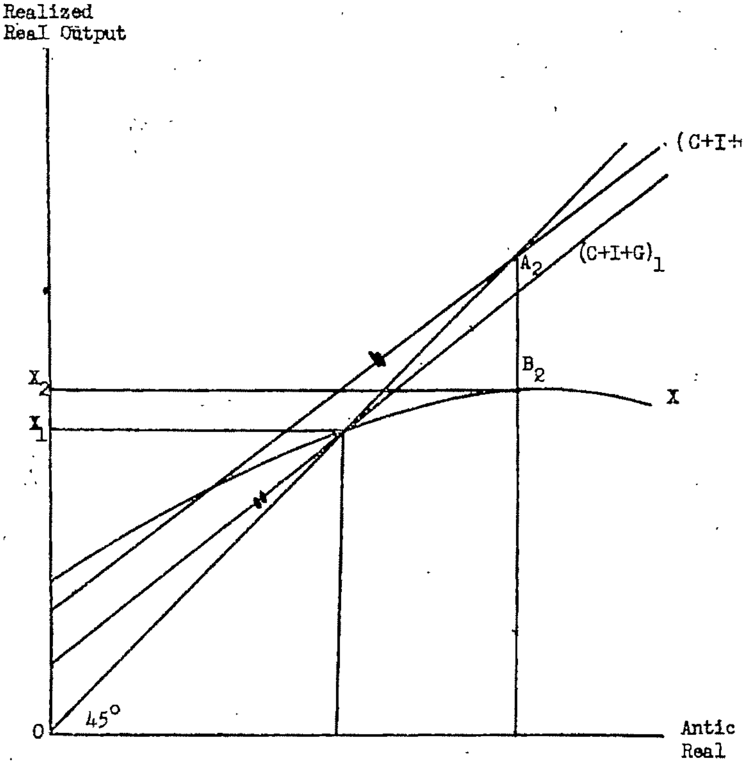

FIGURE 6

ployers" to "suppliers" of inputs), Bronfenbrenner assumed, doxically the breakdown of money illusions and the lowering of willingness to supply productive resources. (When real inco above a certain point, a preference for leisure may produce result.) If the society's expenditure curve is at $(C + I + G)_1$ no inflationary gap and presumably no inflation, while real at OX_1. If the expenditure curve is increased to $(C + I + G$ easy monetary or fiscal policy, an inflationary gap of A_2B_2/μ where μ is the standard multiplier $1/(1 - c)$. At the sa however, real output rises to OX_2, largely as a consequence illusion.[20]

Clearly, also, an overambitious or overly optimistic public spending plan can, in a world of incomplete information, rai flationary gap beyond A_2B_2/μ while simultaneously lowerin below OX_2 or even OX_1. Examples may be West Germany pr currency reform of 1948 or Japan under the Ishibashi policies of 1946-47.

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[20] A deflationary implication is also possible when B_2 (maximum point on X left of the 45-degree line.

Another strain of argument, with similar conclusions, stresses the effect of inflation upon the real rate of interest. Unanticipated price inflation lowers the real rate of interest possibly to or below zero, increasing investment and therefore output growth. This application of the Keynesian "euthanasia of the rentier" is found in Vickrey's work [178] and is an important element in Kaldor's growth models [82]. A third strain points to the possible maintenance of industrial peace, avoidance of strikes, and increases of output by wage and price increases where trade unions anticipate wage increments in excess of productivity gains and where employers refuse to cut profit margins. This view is associated particularly with Slichter [160] [162] [163].

In several Latin American countries, doctrines called *estructuralismo* and *Cepalismo*[21] affirm that aggregate output responds to price increases with greater elasticity than to equal real-income increases achieved by noninflationary means. For this reason, as well as because inflation tends to lower real interest rates and helps maintain industrial peace, many Latin American writers oppose monetary and fiscal controls over the price level. Their position has been systematized sympathetically by Seers [155-a]. These several issues have been discussed, with copious references to Spanish- and Portuguese-language sources, in essays by Hirschman, de Oliviera Campos, Felix, and Grunwald in *Latin American Issues* [75] and in two international conferences on inflation and growth (Elsinore [66] in 1959 and Rio de Janeiro in 1963).

Structuralism sometimes involves arguments like those illustrated by our Figures 1b, 1d, 3, 5, 6, and (below) 8. Yet another diagrammatic presentation appears to take the form of Figure 7, which itself involves two separate panels. On the right-hand panel an increase in the nominal supply of money raises both aggregate supply and demand for domestic output currently produced, by partial expropriation of rentiers (with their supposed propensities for capital export and land speculation) and by subsidy of the more active elements in society (who both produce and purchase current domestic output). The vertical rise in supply, from S_0 to S_1 to S_2, reflects higher money wages and other costs. The horizontal increase in demand, from D_0 to D_1 to D_2, reflects increases in the level of money incomes. A hybrid curve labelled T passes through the points of intersection Z_0, Z_1, and Z_2. Its positive slope, not to be confused with those of the supply functions S_i, illus-

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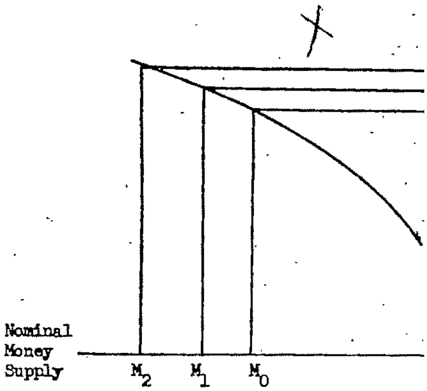
[21] "CEPAL" refers to the U.N. Comisión Económica para la América Latina (ECLA). Its most influential spokesman on the international scene has been Prebisch. For a Middle-Eastern presentation of the same position, see Nasr [66, pp. 491-93].



FIG

trates the structuralist view that vary with the price level and tha verse the growth in both output an

The left-hand panel suggests a conventional quantity theory of money supply from M_0 to M_1 to as well as raising prices from P_0 than proportionately to the rise i structure is indicated a second tim by making M_2/M_0 substantially other pairs of money supplies and

A frequent rebuttal of *estructur* by Bernstein and Patel [13], is t 6) and Y_0Y_2 (of Figure 7) are goods. Productive resources are di inflation, which may not be good the long term. This is essentially tarists," the Latin American oppo study by U Tun Wai [179], conce derives from international comp from intertemporal comparisons

d U.S. data and again from Latin American [112] [66,
: prostructuralist Seers, surprisingly enough, agrees with
ie Latin American case [155a, p. 191]. (These writers do
ossible parabolic relationships.)

of structuralism native to the United States, with roots
price-inflexibility theorists of the Great Depression, re-
rices of capital goods and to wage rates in strongly organ-
f the economy. Such prices, it is said, rise in consequence
demand or cost but do not fall in response to decreased
ost. Wages likewise rise in response to increases in living
isiness profits but do not fall when the latter decline. The
s one-way flexibility is that shifts in demand have infla-
s, raising prices and wages in sectors to which demand
iving them substantially unchanged in sectors from which
s.

rd bias in the price level is called "sectoral" or "demand-
n. It is another type of "mixed" inflation theory, whose
pment, both in academic literature and in congressional
is been by Schultze [154].[22] An important predecessor is
plying the downward price rigidity which made him
: 1930's under the new rubric of "administered inflation"
The Schultze sectoral-inflation theory is supported in the
of the Joint Economic Committee on Employment,
Price Levels [46], also known as the "Eckstein Report."
cost-push theories, demand-shift theories confine mone-
il policy to relatively passive roles. Once the price level is
y sellers' decisions, monetary and fiscal policy can main-
me of employment by validating these decisions. If this
the main consequence is reduced employment, as in the
agram of Figure 1b.

al approach did not completely convince all readers of
Report (e.g., Minsky [117] and Robinson [144]) of its
s with cost-push (Figure 1d), the eventual development
ply and unemployment in the declining sectors will end
and perhaps reverse it, given a constant rate of "sectoral
upshot of a series of sectoral demand shifts should

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ks that, with markup pricing, prices might even rise in the declining
ige costs rise as demand and output decline. The situation is aggravated
factors: the recent trend toward a higher proportion of overhead and
of variable costs in total costs; the "wage contour" effects (Section III)
in expanding industries; and the fact that the outputs of the rising-price
ies may be inputs of contracting industries.

eventually be upward price-level pressure when the pace of shifting demand is increasing and downward price-level pressure when this pace is decreasing. If price-level pressure is, in fact, continuously upward, some other force than demand shift is likely to be operating.[23]

Sectoral inflation has also been attacked by implication by demand-pull theorists whose attention has been directed particularly at statistics indicating downward rigidity in prices and wages. While admitting the downward rigidity of quoted prices and wages, it is claimed that, during recessions and depressions, actual prices and wages fall, either openly or under various disguises. Bailey [9] summarizes much of this evidence for the United States in the middle 1950's; Flueck's contribution to [54] provides detail for individual industries.

III. *Supply or Cost Inflation*

Cost inflation has been the layman's instinctive explanation of general prices increases since the dawn of the monetary system. We know of no inflationary movement that has not been blamed by some people on "profiteers," "speculators," "hoarders," or workers and peasants "living beyond their station." Cost-inflation reasoning underlay, for example, the various Statutes of Laborers which followed the medieval Black Death. In the Classical Period of political economy, both the Anti-Bullionist opponents of Ricardo and the Banking School opponents of the Bank Act of 1844 were in large part cost-inflationists (or cost-deflationists, when prices were falling). In the United States, during the Greenback and Bimetallist controversies of the later 19th century, apostles of "quality" theories of money, who explained price changes (under "sound money") by changes "on the side of goods," could be termed cost-deflationists.

An analytical advance was made by Keynes in his *Treatise on Money* in 1930. Here a distinction was made [88, I, pp. 166 ff.] between a "spontaneous" and an "induced" increase in earnings, the first of which represents supply or cost inflation. This insight was however not expanded in the subsequent *General Theory* [89], nor in the tract on *How To Pay for the War* [90]. Partially as a result of the prestige owed to the Keynes of the *General Theory*, the very existence of cost inflation has been questioned by many professed followers of Keynes.

Although cost-push theories of inflation are not in fact new, their revival in the 1950's has been called *The New Inflation*, e.g., by Thorp and Quandt [166a]. The title symbolizes reaction against the demand-pull orthodoxy of the prior decade but does not deny that cost-push may involve increases in money supply, money income, and money expenditures, particularly if decreases in output and employment are to be avoided.[24]

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[23] It is also worth noting that Schultze's model employs a dual price theory. Prices rise in the expanding sectors as a result of "flexibility" in the face of demand-pull and are maintained in the contracting sectors by a cost-push markup approach. These views are theoretically inconsistent; however, as Robinson [144] argues, an all-purpose price theory is not necessary and may not accord with realities.

More important, the term "new inflation" embodies the opinion that the strength of economic pressure groups (including, but not confined to, trade unions), together with increased public concern with unemployment, had increased the likelihood (in many countries) of "disequilibrium" price and wage increases being validated by expansive monetary and fiscal policies, resulting from organized pressure on monetary and fiscal authorities. These same factors had also, in this view, decreased substantially the likelihood of "equilibrating" price and wage reductions except in the most drastic of depressions. The combined effect of these changes (if they have come about since World War II) is clearly toward monotonic secular inflation, although the rate of such inflation need not be extreme. Indeed, many U.S. writers have pointed out that the rate of the allegedly "new" inflation of the post-Korea decade has been slower than the admittedly "old," but equally monotonic, inflation between 1896 and 1914.

Before examining the macro-character of cost inflation, it may be worth while to review, at the micro-level, the conditions under which a wage or profit "push" can occur. In the first place, such a "push" can come about as a result of market power only under noncompetitive conditions. The demand for labor as a whole, or for some type of labor, must be less than perfectly elastic; labor (management) must be able and willing to shift its supply functions upward despite unchanging demand. Analysis of wage and profit "push" must take into account, therefore, both supply and demand conditions in labor and product markets.

A. *Demand for Labor*

The Marshallian theory of joint demand is a useful tool in analyzing the demand side of the labor market. The more inelastic the demand for groups of laborers, the more wages can be pushed up (without an intolerable reduction in either profits or employment—see below); and, following Marshall, the demand for labor will be more inelastic (1) the more essential labor is in the final product, i.e., the lower the elasticity of substitution between labor and cooperating factors; (2) the more inelastic the demand for the final product; (3) the smaller

---

[24] In our own view, pure supply inflation (Figure 1b) is not likely to occur over the long run. Most inflations containing "push" elements appear to be mixed ones in which "push" and "pull" interact in ways described elsewhere in this paper.

the ratio of wages to total cost;[25] a
of cooperating factors.[26] These Ma
need not be elaborated here. Their
following discussion.

Several inferences may be draw
In terms of joint-demand analys
group of workers may have consi
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and nonunion labor and other facto

At the other extreme, it is clear t
unions will have power to raise w
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sessed of special advantages whic
vious that under competitive con
firm alone would immediately redu
ually drive it out of business.

This should not be taken to imp
competitive industries, for bargai
or multi-employer basis. If all uni
with many employers simultaneou
pushed up, since the firms in questi
perfectly elastic demand curve.[28]

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[25] For exceptions to this "law," see [23].

[26] It is worth noting that the ability of a
incorporated into this analysis through eithe
raise wages can be viewed from the demand s
workers can substitute for employed worker

[27] Milton Friedman [56] uses Marshall's l
than industrial unions to be successful in
unions embrace such a small percentage of t
push therefore unlikely. See also Ulman's

[28] This argument, it should be noted, ap
theoretical analysis of the unexploited mono

rtter [27] and Ulman [175] have demonstrated that this
ning is a two-edged sword—since employers do not fear
es, their incentive to resist wage increases and to accept
engthened, and the wage increase under multi-employer
ay therefore be less than under piecemeal bargaining.

vs may be reconciled, in part, by introducing the level, and
of changes in the level, of business activity or of un-
(i.e., the demand-pull factor) as additional variables.
vely buoyant business conditions, firms are less loath to
vage increases and do not wish to engage in strikes. The
mand curve is less elastic and may be expected to shift
its are higher and the costs of work stoppage greater than
el of unemployment is high.[80] Multi-employer bargaining
last restraint on wage increases. On the other hand, when
ack or declining, and there may be expectations of down-
rofits are lower, and firms cannot as easily grant generous
es and are more willing to face strikes. Multi-employer
ay encourage this latent resistance. Even though the de-
facing the group is less elastic than that facing each in-
, it is sufficiently elastic that the losses to the group from
se may be very large.

the opportunities for obtaining wage increases in the ab-
rent increases in demand are greater in enterprises that
ligopolistic market structures.[81] Statistical evidence has
d by a number of writers including Ross and Goldner
arino [62], and Levinson [102]. Most economists are
if there is wage-push, it tends to be greatest in such in-
theoretical reasons are obvious: the product-demand
oligopolists are less elastic than in more competitive
ing the unions something "to climb up on" as well as
ssible to pass part or most of a wage increase on to the
n raising prices, the oligopolist is protected against new

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less demand is perfectly inelastic, the group as a whole will lose sales to

part of business willingness to raise wages in good times is the greater
ce because unions have more funds with which to fight strikes; union
ncially better able to sustain a work stoppage; there is better control over
in the sense that fewer unemployed nonunion laborers might take over the
mbers. To the extent that unions do fear the employment effects of wage
fears are also reduced.

some writers [58] [137] [153], unions consciously or unconsciously
t. There is some evidence that they act to maintain or increase the degree
various product markets.

competition by large capital requirements (steel), technological know-how (flat glass), or marketing advantages (automobiles, cigarettes) [153].

The large profit margins in some oligopolistic industries[32] may encourage inflation by activating (providing an excuse for) unions to seek large wage increases. Statistical evidence of the significant relationship between wage increases and profit margins is provided by Levinson [102], Bowen [18], Bhatia [14], Eckstein and Wilson [47], among others.[33] The wage increases that result, however, do not necessarily lead to price increases (since they may be paid from profits). Neither are they necessarily classifiable as "push." First, large profits may be the result of previous increases in demand; if so, the resulting wage increases are more appropriately interpreted as due to lagged demand-pull. Second, because of management preference (according to Kuhn [97] and Reynolds [141]), increments to profits from productivity increases are more likely to be used to raise wages than to be retained by corporations or to result in lower prices.[34] Such wage increases would occur, presumably, even without unions. To paraphrase Reynolds [140], unions ride the coattails of oligopoly quite effectively in some branches of heavy industry, but their status is still that of junior partner rather than prime mover. Segal [156], utilizing new data on the extent of collective bargaining in manufacturing, argues that unions must be viewed as senior partners. His position rests on the facts that (1) those industries with high concentration ratios and large wage increases are also industries that are usually highly unionized; and (2) in a number of industries (e.g., metal products, machinery) with low concentration ratios but with strong unions, wage increases were comparable to those in the concentrated industries.[35]

B. *Supply of Labor*

The goals of unions and likewise their control over supply are important to the question of whether or not cost-push can exist. A major goal of all unions is raising wages.[36] The interesting questions are: what determines the extent of union (money) wage demands and what (supply-side) powers do they have to enforce them? At least three different sets of variables are widely believed to serve as guidelines for unions in determining their asking price: changes in the cost of living index (CPI); the economic situation of the enterprise (as suggested, for example, by profits and productivity); and neighboring strategic wage rates.[37] Are these various guidelines likely to lead a union to ask for a wage increase that involves a "push"? The answer is (in the first approximation) that such a wage increase is less likely with either of the first two guidelines than with the third. An increase in the CPI is likely to reflect an increase in the money demand for final output and therefore for labor.[38] Increases in wages geared to changes in productivity and profits may not involve a push or even a price increase when the possibility of absorbing above-average wage increases exists (but see below). On the other hand, attempts to eliminate wage differentials will more often involve a "push," since they would tend to be relatively uncorrelated with excess demand, excess profits, or large increases in productivity. The existence of wage differentials and attempts to eliminate them appear, then, to be a basic factor in the propagation of wage-push.[39]

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[32] These result not only because the "rent" or "monopoly profit" normally accrues to enterprises in these markets but for two additional reasons: productivity gains tend to be largest in the heavy industries; and, because of their large overhead costs, short-run expansion usually leads to still higher profit rates.

[33] This evidence refers only to the United States. British data do not support strong relationships between changes in wage rates and profits (see Section IV).

[34] This is due to the following causes: large short-run profits encourage entry which reduces long-run profits; the importance of rising wages for worker efficiency and for enabling the enterprise to attract and hold the best workers; and the unpopularity among managements of lowering prices.

[35] Bowen's results [18] are similar to Segal's. In their earlier study, Ross and Goldner [147] felt that the degree of unionization was of independent significance in raising wages in oligopolistic markets.

One mechanism by which cost-push may develop from wage differentials is as follows. Management hires a high-productivity worker within a given job category at a high wage [133]; a high-productivity firm within an industry grants a large wage increase [64]; high-profit oligopolistic firms grant wage increases; the demand for products of some firms or industries rise and for others fall, leading to induced price and wage increases only in the former [154]; etc. Elimination of these differentials becomes a goal of unions. To the extent that unions succeed in removing them and, in doing so, achieve wage increases larger than are competitively justifiable in terms of (marginal) productivity or demand for labor, prices will have been pushed up, and employment will decline. In other words, the attempt to overcome "inequitable" differentials induces "unrealistic" demands by unions. Wages are, in effect, administered without regard for the state of demand.[40]

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[36] We abstract here from fringe benefits and conditions of work, though these are, of course, important.

[37] Called also "wage contours" by Dunlop, "orbits of coercive comparison" by Ross, and "wage-constellations" by Harbison.

[38] This would not be the case if the rising CPI were due to a prior cost-push.

[39] An obvious necessary condition for a wage-push is that the union be more concerned with higher wages than with the unemployment which may result from the higher (desired) level of wages. Most writers feel that this is the case. Ross [146] argues that unions take no account at all of the employment effects of their wage decisions. Schultz and Myers [155] feel that employment effects are taken into account only when unemployment is high or when these effects are likely to be severe (e.g., when there is strong competition in the product market or when the industry is poorly organized). Haberler [66] argues that the employment effects are not easily visible to unions (and therefore largely ignored) because the short-run demand for labor is relatively inelastic. It is only over the longer period that wage-push results in unemployment. Union resistance to lowering wages in the face of deficient demand is well known; unions usually prefer to ration jobs than countenance a drop in wages.

Union attitudes toward the price effects of their wage demands should also be considered. If labor bargained as a national unit, the price effects of wage increases would substantially reduce the increase in real wages that resulted from the increase in money wages. Since in the United States and in many other nations unions are, in Kerr's term [84], "sectional bargainers," the price effects of each individual union's wage demands have at most an insignificant effect on its real wages. This allows each union to ignore its own price effects. Furthermore, most unions feel that wage increases "should" be paid out of profits.

The operation of this mechanism depends largely, as Kuhn [97] has noted, on short-term labor immobility regardless of wage differentials.[41] Given mobility, a rise in productivity (demand) and then wages in one firm (industry) would attract labor from other firms (or industries). The resulting competitive forces would tend to equalize wages at a rate perhaps slightly above the old one but not so high as with immobility. Differential rates plus immobility spell dissatisfaction; and dissatisfaction leads to the "greater than competitive" wage-push.[42] It is important to note that unions contribute substantially to immobility via closed or union shop, seniority, pensions, etc. and thereby contribute to the wage-push via this channel as well.

The existence of union rivalry for wage increases has been mentioned as a major cause of cost-push by foreign and U.S. observers [16]. Turner [168] ascribes the failure of the British wage-stop of 1950 to differential wages and competition among unions. He further points out that in 1952-53, when profits were very low and unemployment relatively high, wage differentials induced some unions (e.g., in cotton textiles with 30 per cent unemployed) to ask for large wage increases. Gösta Rehn [139] has argued, for Sweden, that wage-push can be

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[40] At this point it is worth noting that many economists feel that relative wages are distorted at low employment, as only the most powerful unions are then able to secure substantial wage increases. As business conditions improve, wages of members of weaker unions and of unorganized workers tend to catch up, thereby accelerating inflation. This is not quite the same process as that which we have been considering. It denies, for example, the ability of unions to emulate key bargains and to close wage differentials at below full employment. It implies that under conditions of rising or excess demand, wages tend to equalize and that equalization is due perhaps more to the competition by management for labor than to workers' bargaining prowess.

[41] It also depends on the downward rigidity of wage rates.

[42] Ulman [175] feels the pattern is also followed to some extent in nonunion firms. This is partly a result of the greater demand for the products of unorganized firms as prices rise in the unionized sector and as the union wage increases augment demand generally. Further, management is encouraged to raise nonunion wages to maintain morale and efficiency and to avoid unionization, both major goals of management. This is offset, of course, by the possibility of an increased supply of labor in the nonunion sector.

eliminated only by the prior elimination of "excessive" wage differentials and "excessive" profits. Lundberg [108] argues that much of the Swedish inflation of the 'fifties resulted from a "wage-wage spiral" to eliminate inequities in the wage structure. In a sense the mechanism described above has been institutionalized and accelerated in the United States by so-called "key" bargaining. Since World War II, large segments of an industry or of the economy await the results of such key bargaining sessions as between the appropriate unions and U.S. Steel and General Motors. These results serve as a "pattern" for both labor and management and are usually followed closely by other firms and industries.

We have stressed the importance of the overcoming of wage differentials as a major factor which induces wage demands involving a "push." Whatever the guideline of union action, however, a "push" may be involved if the union applies sufficient leverage. Thus, the unions may not ask just for an increase in wages equal to the increase in the CPI but may apply a "markup" [2]. A similar strategy may be pursued where the guidelines are profits and productivity. At any given level of demand, the extent of pressure for wage increases depends upon a number of factors. The nature of the union movement is important. The more militant the unions and the less their sympathies with the stabilization policies of the government, the more likely it is that wages will be pushed up. The unions in France and in some Latin American countries after World War II fall into the militant category. On the other hand, British unions during the tenure of the Labor Government might have been called "partners in social control" [84], and some writers [93] [114] feel that they restrained inflation for a period after World War II. Others argue that the high postwar levels of employment in the United States [19], plus government "guarantees" of "high" employment [6] [118] [119], have engendered expectations of larger wage increases than would normally have been bargained for in the prewar period, given the values of the indicators guiding union wage demands. This is supported in fact by the Phillips-curve analysis (Section IV): Samuelson and Solow [151] and France [55] find that the curve is farther to the right in the postwar period than in the prewar. This might also be due, of course, to the great percentage of the labor force currently in unions [113].[48]

Finally, it is important to note that the ability of unions to push up wages depends to a considerable extent on their degree of control over the supply of labor in the firm or industry. Most of what has been stated above depends implicitly on the existence of such control. While it is not easy to measure the degree of control exercised by labor, it is clearly related to the extent of union organization and is further indicated by many of the practices and institutions connected with unions such as closed or union shop, check-off of union dues, control of entry into the union, make-work and like practices, strike threats and strikes, all-or-none bargaining or approaches thereto, and currently (1962-63) the movement for major reductions in working hours.[44]

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[48] We also noted above (footnote 30) that union wage demands are usually larger in good times than in bad. The various reasons listed above for this phenomenon are all attributable, of course, either directly or indirectly to the influence of demand-pull rather than the cost-push.

C. *Markup Pricing*

Markup pricing tends to facilitate the inflationary potential of a cost-push under conditions of stable or gently rising demand, and rigid prices at low levels of employment or in the face of declining demand. On the other hand, it probably slows down an inflation that is due to rapidly rising demand forces.

Assume stable demand. To the extent that wages are "pushed up" in oligopolistic enterprises as a result of the factors mentioned above, the cost increase will usually be translated into a price increase, regardless of the price-setting technique practiced. An enterprise that attempted to maximize short-run profits (and whose prices were set flexibly in the face of changes in demand) would, after a wage-push or any other cost increase, cut back on employment and output.[45] Under an assumption of stable demand, price would then rise until demand for output was equated with reduced supply [2]. Similarly, if the enterprise followed some form of "full cost markup" pricing, prices also would be raised to reflect the higher costs. Under markup pricing, the cost-push tends to be facilitated for the following reasons. First, the price rise follows directly, and therefore more rapidly, upon the wage increase. Second, when prices are insensitive to demand, profit rates tend to be maintained. The resulting price is apt to be set at a higher level than under short-run profit-maximization.[46] Third, the previous argument implies that management will put up less resistance to wage increases when markup pricing is practiced.

These same arguments apply to the case of declining demand (or of relatively high levels of unemployment). Under these circumstances, markup pricing translates rigid wages (downward) into a price floor. On the other hand, under profit maximization and flexible prices, a drop in demand tends to reduce prices and profit margins. It would also foster resistance by management to the maintenance of the previous level of wages.

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[44] For further discussion of these issues, see Becker [12], Lewis [103], and Nutter [123].

[45] Assuming that the MRP of labor declines with increasing output.

[46] This argument is especially forceful if declining output involves rising average unit costs [154].

Under conditions of rapidly rising demand, where there is, in effect, demand-pull rather than cost-push, price increases are slowed down by markup pricing. Since prices are set with regard to costs rather than demand, and since the increase in demand is (by definition) outstripping the rise in costs, prices will tend to be set at lower levels than are warranted by the state of demand. This lag is further facilitated, in the absence of escalation, by the long period between bargaining sessions during which excess demand builds up without wage increases.

As noted earlier, markups tend to be adjusted to changes in demand conditions, though with a lag. The effects noted above are reduced to the extent that this lag is shortened.

D. *Profit-Push*

Administered prices raise the possibility of profit-push inflation—by raising markups, businesses may, without any increase in costs or in demand, raise prices. It is contended that a profit-push is likely to be smaller than a wage-push partly because profits constitute a smaller part of price and because such a push is more likely to be "once and for all," whereas wage-pushes are more likely to be continuous [64] [66, pp. 27-31]. The truth of the latter contention would seem to be based on the following observations.

1. Profits of an enterprise depend not only on high prices but on large sales and low unit costs; and the latter depend in part on the price charged. This would give firms pause when considering price increases in the absence of increases in wages or in demand. On the other hand, the real wages of workers depend upon money wages, prices, and employment. Under piecemeal bargaining, the prices of goods bought by the workers in any firm do not depend on their own money wages; and, as we have noted above, loss of employment is not usually seriously considered by workers or unions in pressing wage claims. There are therefore no pressing indirect effects of wage increases which might cause individual unions to forego wage demands.
2. There would seem to be a lower probability of a profit-profit competitive spiral than of a wage-wage spiral. Furthermore, to the extent that there may be competition among enterprises to increase profits, it would seem to be conducted along the paths of increasing productivity and/or sales (via advertising, etc.) rather than by raising prices.
3. The positive expenditure effects of wage increases are likely to be greater than those from higher profits, thereby sustaining the power of initial inflationary forces.

To the extent that the above relationships do not hold completely, and to the extent that a profit-push induces new wage demands, a profit-push may also be continuous.

E. *Macro-Aspects of Cost Inflation*

Pure cost- as well as mixed-inflation models (Figure 1d) appeared in some profusion beginning in 1948. They were obviously stimulated by the several rounds of wage and price increases in the United States and elsewhere which resulted from free collective bargaining in the postwar period. The wage rounds were considered aggressively inflationary in most business circles, purely defensive in most labor circles. Whichever the case, some economists developed models that attempted to assess the characteristics of this wage-profit-price-spiral process which seemed to be such a clearly observable phenomenon in the economic life of many western industrial nations.

F. *The Basic Model*

The process of cost inflation was first generalized as income inflation (Turvey's term [170]) by Reder [136], and subsequently by Aujac [5], Duesenberry [43], Holzman [76], and others. Ackley's [1] [2] administered price or markup inflation is an adaptation of these earlier models. The basic idea of income inflation (as opposed to demand inflation) is that, at full employment, different income groups in the population attempt to raise or maintain[47] their real incomes by raising their monetary incomes. If output cannot be expanded to satisfy expectations, prices rise, and all groups experience some frustration in seeking their objectives. In other words, real demand (in terms of income objectives) exceeds the real supply available to satisfy these demands. The process continues as long as frustrated groups, in Aujac's terms [5], try to change monetary arrangements to suit their economic objectives. The process stops when everyone adapts to the new monetary arrangements. This may occur when those who have no power to change their incomes (say, rentiers) are sufficiently "expropriated" through rising prices to satisfy the demands of the other groups; through "money illusion";[48] or, in general, when the course of inflation has caused a sufficient reduction in employment or in real expenditures that the power to make excess income claims ceases. As noted above, groups are not necessarily historically or functionally defined but comprise any collection of individuals with interests in common—not only wage

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[47] As Aujac [5] points out, to start the process it is only necessary for one group to attempt to raise its real income, since this reduces the real incomes of other groups; if these other groups attempt to maintain their previous real incomes, inflation results. The process can also be started by an increase in demand or decline in supply in the free market sector [43], a rise in import prices, rise in cost of living, etc.

[48] I.e., if persons think, wrongly, that real incomes have risen as much as money incomes.

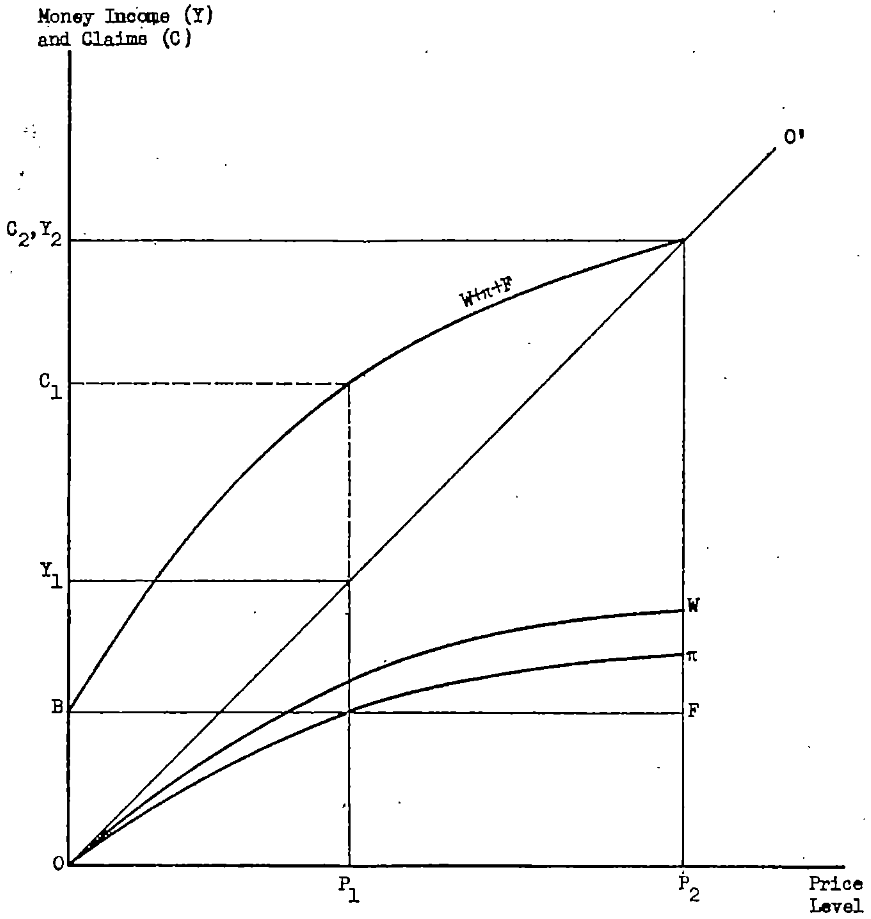

FIGURE 8

earners, profit receivers, and rentiers but the government, farmers, exporters, importers, members of individual unions, the aged, etc.[49]

This process is illustrated in Figure 8, which is adapted from Reder [136].[50] The price level is measured horizontally and money income vertically. All points on the ray *OO′* through the origin represent the same real income. Let *OB* represent the fixed-money claims of the rentiers (receiving rent and long-term interest). Let the *W* and *π* curves represent the claims of wage and gross-profit receivers respectively. Let $W + \pi + F$ = total claims C, which exceed the real income available at Y_1P_1, leading to a price rise. These conflicting claims are eventually resolved by the rise in prices at Y_2P_2 by mechanisms described above (also see Section V), at which point prices cease to rise further. Had the $W + \pi + F$ curve failed to converge on OO' from above (or cut it from below), a stable solution would be impossible, and prices would rise indefinitely.[51]

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[49] It should be noted that competition among unions to "rectify" wage differentials (which, as we have seen, is such an important aspect of cost inflation) falls naturally within the scope of the process just described.

[50] The reader will note the formal similarity between Figure 8 and the demand-pull Figure 3. In fact, of course, the stabilizing and destabilizing factors in the two cases may be quite different.

In his markup-inflation model, Ackley assumes that all business firms base their prices on some markup over their material and labor costs and that labor seeks an increase in money wages that involves a markup over the increase in cost of living. The result is a ratchet of the sort just described. Ackley further points out that much of the process develops in interfirm transactions; each enterprise is induced to mark up price as the result of markups included in the prices of intermediate products it purchases from other firms. Ackley's model adds to the income-inflation picture in several respects. First, it relates the price-setting process to actual business practice. Second, it stresses the insensitivity to demand, in the short run, of administered prices and wages. Thus, if all wages and prices were set by markup, an inflation could not be initiated in the short run by excess demand, nor be stopped by deficient demand. Over the longer run, the picture is different. Eventually, excess demand induces an increase in the rates of markup, thereby beginning or accelerating an inflation without a change in costs. However, since there always is a competitive sector in agricultural commodities and other raw materials, excess demand induces a markup inflation immediately via rises in the prices of these inputs [43]. Ackley provides, therefore, an interesting unified "push-pull" model (Figure 1-d): pricing and wage-setting according to cost markups provide the "push"; and the variation in size of markups according to business conditions (as well as his inclusion of a flexible price sector) represents the effect of "pull."

G. *Sociopolitical Implications*

Income inflation lends itself to sociopolitical interpretation [5] [8] [22] [76] [111] [136]. Almost every group in the economy periodically tries to improve its real economic position. At high levels of employment, no group can gain without another group losing.[52] If, under these conditions, the distribution of income were determined in real terms, open social strife and tension would be much more common, and social cleavages would be significantly more marked than they are.[53] Inflation acts as the great "social mollifier" [22] by allowing various social groups to raise their money incomes (and perhaps real incomes) without cutting any other group's money income.[54] The rentier and other passive groups, of course, lose by the process to the active groups; this fact has led one writer [22] to refer to secular inflation as the social rebuttal to compound interest.[55]

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[51] One typical model (allowing for lags) underlying the curves in Figure 8 would be to postulate $W_t = f_1(P_{t-1})$, $\pi_t = f_2(P_{t-1})$ or $f_3(W_t - W_{t-1})$, $F_t = F_{t-1}$, and $Y_t = W_t + F_t + \pi_t$. Here the rate and extent of inflation depend on the values of the functions and on the size of F, the "expropriable" group. For a model of this type, see [76]. It should be noted that these pure cost-push models (Figure 1b) assume (1) that the monetary system accommodates completely to changes in real variables, and (2) that the size of the "push" is completely independent of the level of demand. Also, to the extent that the forces implied in such a model operate in an economy, they supplant Keynesian expenditure-multiplier processes as determinants of money national income. Compare Holzman [76].

Reder argues similarly. He claims that the price level is determined by the trilateral monopoly of unions, employers, and the monetary authority who presumably represents the rentiers. The monetary authority is forced to choose between full employment and price stability as its primary goal (as in the "dilemma model" below). To the extent that full employment is favored, the monetary authority leaves price-level determination to collective bargaining, and abdicates its responsibility to the rentiers. As a "social mollifier," inflation serves a real socioeconomic function, which can only be replaced by a rate of economic growth high enough to meet all expectations of higher incomes. The difficulty in stopping income inflation, then, results from the fact that not only labor but all producer pressure groups in the economy prefer it to the alternatives. This is indicated by their actions [20a],[56] if not by their words.

H. *The Dilemma Model*[57]

With remarkable prescience, both Keynes [89, pp. 298-302] and Mrs. Robinson [143] foresaw in the prewar period the dilemma facing most western nations today: the impossibility of achieving simultaneously, and without price or wage controls, the twin goals of full employment and price stability. The records of many nations[58] with strong producer pressure groups show that as unemployment is reduced below a certain critical level, prices begin to rise; and that, when prices are stable, unemployment is above the level that is considered "socially tolerable" by nations committed in principle to a full-employment goal.

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[52] This statement is inaccurate to the extent that productivity increases.

[53] Marchal [111] considers U.S. agricultural policy as an example of income escalation to reduce sociopolitical tension.

[54] Lindblom [104], on the other hand, argues that union power is so great that unions are incompatible with capitalism, causing either so much unemployment or inflation that the competitive price system must be abandoned.

[55] Reder [136] refers to it as the "safety valve."

[56] Fellner [51] has argued that the income struggle requires a money illusion because with perfect foresight it would be clear that there is no gain with subsequent price rises. This would only be true (1) if all groups had the same bargaining power; (2) if all groups bargained simultaneously; and (3) there were no lags between income and price increases.

[57] The term was first used, and the problem first analyzed in detail, in Reder [136]. The most exhaustive study of "dilemma" issues is Bowen's [19]. An early and famous statement of the "dilemma" appeared in 1952 in the London *Economist* [177].

Prices may rise before full employment of labor is reached for several reasons involving no exercise of market power by any economic pressure group. A. H. Hansen pointed out that, as capacity is approached, marginal and average costs of production may rise because of diseconomies of scale [67]. A number of analyses involving low elasticities of substitution between productive factors suggest that nonlabor factors may be overemployed, while unemployment of labor still exists. (Smithies' essay [165] is a case in point; another, treated in Part I, is Fukuoka's [60] discussion of structural unemployment.) Any attempt to reduce unemployment of labor by increasing demand would raise the prices of nonlabor factors. Finally, attempts to reduce frictional and structural unemployment of labor below some variable limit may lead to rising wages and prices before the number of unfilled vacancies exceeds the number of unemployed.[59] Structural and frictional unemployment, taken together, are commonly estimated at no more than 3 per cent of the U.S. labor force, a level that is fortunately considered "socially tolerable" by most writers.[60]

To whatever extent unemployment (regardless of its technical variety) can be reduced most rapidly or permanently by increasing aggregate demand, but at the risk of rising costs under pure competition or increased monopoly power in the hands of imperfectly competitive pressure groups, the authorities are faced with a dilemma between the desirable alternatives of stable prices and high employment.[61] For those who feel, like A. G. Hart (cited in [152]),[62] that upwards of 10 per cent unemployment was required to stabilize prices and, like Phillips [133], that 7-8 per cent is still required, the implied power of unions and big business is indeed great and the dilemma very sharp. At the other extreme Morton [119] and Friedman [56] appear to feel that there is almost no dilemma—that a small amount of unemployment (and no government guarantee of full employment) would rapidly end the wage-push, and that inflation occurs only because the fiscal authorities insist on validating all wage increases, thereby encouraging abnormal wage demands.

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[58] As demonstrated graphically by the so-called Phillips curves (below).

[59] Frictional unemployment may be ascribed to the imperfect mobility of labor. Even when there are as many unfilled vacancies as laborers seeking work, such factors as ignorance and costs of transfer keep some labor temporarily unemployed (and some jobs unfilled). To the extent that unemployed workers are unsuited by skill to fill existing vacancies, the duration of unemployment may be longer. This is usually called structural unemployment in advanced economies.

Cases of unemployment of labor while capital is fully employed are sometimes called "structural unemployment" in underdeveloped or reconstructing economies, along with the "money illusion" cases considered above under the same title. An important difference between the two is that when elasticities of substitution are low, inflation cannot increase real income; whereas, when money illusion is important, inflation can increase real income by reducing voluntary unemployment, industrial disputes, and so on.

[60] This would be equivalent to Phillips curve *I* in Figure 9 below.

[61] It is worth noting, however, that Hansen's rising costs at above-optimum capacity are once-and-for-all increases and eventually stabilize at the highest level of employment reached. Further, frictional and structural unemployment cannot be reduced significantly

The existence of the dilemma and its inflationary close-to-full-employment solution by most nations led Reder [136] and Hicks [74] to point to fundamental changes that have taken place in the macro-adjustment mechanism. Reder points out (from a 1948 vantage point) that Keynesian theory assumes the existence of a "unified monetary authority" which can affect global magnitudes such as the price and employment levels. It further assumes that all other groups must accept the decisions of this authority. In fact, the monetary authority is part of a trilateral monopoly along with labor and management, all of whom can exert independent power in the macro-market place. Like other monopolists, the monetary authority can control either the price level or the level of employment, but not both at once. Hicks, following the same line of thought, argues that nations are no longer on a gold standard but rather on a "labour standard." In the old days, if wages were too high, unemployment ensued and wages yielded. Today there is no such thing as a disequilibrium wage because the monetary system accommodates to disequilibrium in the labor market. Hicks attributes to the "labour standard" of the value of currency many of the world's present foreign trade problem.[63]

I. *Some Critics of Cost-Push*

While no one has ever doubted, to our knowledge, the possibility or existence of inflation due to excess demand, doubts have been expressed by many regarding cost-push inflation. Some of these doubts were indicated earlier in this section; others are mentioned in Section IV below.

We have already noted that Morton [119] and others long ago argued that there is no real dilemma—that unions would not press for inflationary wage increases and business interests would resist wage

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by increasing aggregate demand but can (over time) by measures to increase labor mobility, to retrain workers, etc. This reduces the sharpness of the dilemma in these cases in comparison with the "market power" cases.

[62] Hart delivered this opinion in 1949 and may have revised it since.

[63] D. H. Robertson posed the problem in similar terms in *The Three Banks Review* (March 1949), cited in [152].

pressures more effectively were it not for the guarantees of full employment stemming from what Morton considers "uncritical Keynesianism." With Keynesians riding high, urging monetary and fiscal ease whenever unemployment develops or sales decline, even the less organized sectors of the economy exhibit inflationary behavior. Unorganized wage rates rise as rapidly as organized ones, and employers sometimes pay higher wages than are negotiated by "responsible" unions in collective bargaining. This is the so-called wage drift, noted particularly in Scandinavia, but in other European nations as well [71] [131]. (It is found also in the Soviet Union where, however, Keynesian economics is not responsible [78].)

A number of U.S. economists have sought, thus far unsuccessfully, to enact this "anti-Keynesian rebuttal" into legislation by amending the Employment Act of 1946 to include price-level stability as a specific objective. (See testimony by Aschheim, Bach, Ensley, Jacoby, and Nourse in [80].) One reason for the failure of this effort, although probably not the most important reason, was the failure to agree on appropriate action in cases of underemployment inflation, the simultaneous sounding of Hart's "whistle" and "gong" [72]. Some advocates of the amendment favored rigid rules of monetary or fiscal policy; some favored a pre-1914 variety of automatic gold standard; some relied on the best judgment of the monetary authorities in individual cases.

There is some question whether price-level stability as a specific policy objective would be sufficient to eliminate cost-push without a serious rise in unemployment. If, as Rehn, Eckstein and Wilson, Dunlop, and others have argued, union wage demands are geared to wage differentials and profits, price stability with high levels of employment would seem fully attainable only if the government also formulated and implemented policy objectives regarding wage differentials and ratios of profits to wages. Measures that substantially increased labor mobility would also seem essential.

Further criticism has come from the monetary side. Selden [157] has taken the 1955-57 period in the United States, which is considered by many push theorists (along with 1958) as exhibit A,[64] and subjected it to searching analysis. (Part of his argument, and a counterargument by Phelps, are presented in Section IV.) Selden argues that inflation could only have been caused (proximately) by one or more of the following: "an increase in stock of money, an increase in monetary velocity, or a decrease in real income." Narrowing it down still further, he shows that the major villain in this period was velocity; the minor villain, stock of money. He then attempts to demonstrate that cost increases were not responsible for the increases in either velocity or stock of money. He argues instead that the investment boom of 1957 and erroneous policies followed by the Federal Reserve System led, respectively, to the increases in velocity and in the stock of money, which in turn caused the price rise.

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[64] There is evidence that "push" elements were more dominant in 1957-58. As both Haberler [66] and Holzman [77] have pointed out, prices, wages, and unemployment all rose together beginning in March, 1957—suggesting inflation along the lines of Figure 1b or 1d.

Selden's analysis casts doubts on those who take the extreme view that *only* cost factors were responsible for inflation in this period. On the other hand, as J. M. Clark has stated [32, p. 5], one can point to some elements of demand in the inflation picture in this period, without disproving the possibility of cost-push as well, when (in his view) both factors were probably operating simultaneously.[65] Ball [11] also argues that Selden's attack attributes to the cost-inflation position a monolithic purity that is quite unrealistic. He further attempts (1) to spell out channels through which autonomous increases in costs may affect velocity; and (2) to argue for the importance, in dynamic situations, of the wage-setting as well as the monetary equations for the determination of changes in the absolute levels of prices. Machlup [109] and Fleming [53] also show, without specifically considering Selden's work, the various ways in which the supply and velocity of money may be increased by a cost-push. Fleming demonstrates that, in theory (though not in the specific case Selden was considering), the usual techniques of denying cost-push via the quantity theory can also be used to deny demand-pull of the Keynesian type. This would leave some form of the quantity theory as the sole explanation of inflation.[66]

IV. *Quantitative Testing*

The various theories presented thus far have been given a qualitative exposition with occasional and incidental references to quantitative evidence. There have been developments, however, in the postwar period on the quantitative side as well. This section considers several of them.

1. During the postwar inflations of the later 1940's, demand theorists frequently were asked: In which sectors of the economy (households, business, international, government) was inflationary pressure concentrated? In particular, what were the relative roles of public deficits and private credit expansions? Many studies along these lines were made in Western Europe, particularly the Netherlands and Scandinavia [15, pp.

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[65] Clark had reference to Selden's presentation before the 1959 Joint Economic Committee Hearings, where he presented views similar to those in his article.

[66] That is to say, most modern quantity theorists would argue that an increase in M and/or V is a necessary, though not sufficient, condition for inflation.

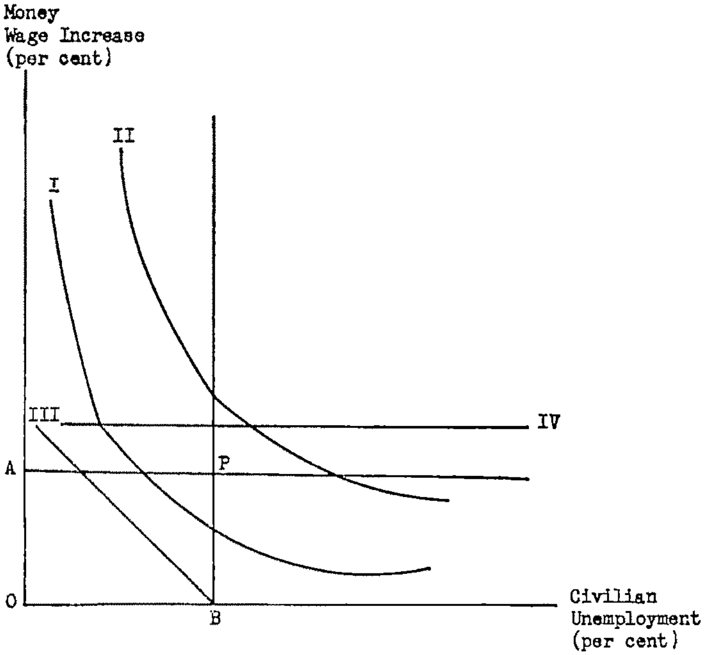

FIGURE 9

50-60]. Another ambitious analysis was Tsuru's statistical examination of postwar Japanese experience [67]. Tsuru concluded that inflationary pressure came from all but the international sector; in contrast with opinion among economists in the American Occupation, Tsuru's results tended to minimize the role of the public deficit and to stress the role of credit expansion to the private sector.

This type of analysis, based on the nation's economic budget, declined after 1950, although a recent sample by Holtrop [66, Ch. 13] was presented at Elsinore in 1959. Perhaps the most important reason for its decline was the rise of interest in cost-push inflation. Another was the difficulty, not solved completely, of distinguishing between expenditures that operate to increase real national output by increasing employment or accelerating technical progress and those that do not. In many countries a third difficulty was the ambiguous status of public corporations: are they part of the private or public sector?

2. Probably the most widely discussed and applied methodological innovation of inflation theory during the 1950's has been the Phillips curve [32], used originally by A. J. Brown [24] and B. Hansen [69], relating the rate of wage increase to the percentage of unemployment of the civilian labor force.

Phillips' own work was highly aggregative. He plotted percentage money-wage increases (Figure 9, vertical axis) against civilian unemployment as a percentage of the civilian labor force (same, horizontal axis) for Great Britain over the period 1861-1957 and obtained a curve of the form *I*, with some lag in the wage-change variable.

Let *OA* (Figure 9) be the maximum percentage of money-wage increase that a capitalist economy with rising productivity can absorb, on the average, with neither inflationary consequences nor shifts in the income distribution in favor of labor. Let *OB* be a maximum acceptable level of unemployment (involving minimal long-term involuntary unemployment). Let *OA* and *OB* cross at *P*. If the statistical curve passes between point *P* and the origin *O*, as in type *I* curve, monetary and fiscal policy can check supply-side inflation (from the side of labor) without imposing unreasonable unemployment on the economy. If, on the other hand, the curve passes outside *P*, as in type *II* curve, the opposite conclusion may be drawn. We are not objectively certain as to the location of *OA* and *OB* and, therefore, of *P*. Frequent U.S. approximations, however, fall between 2.5 and 3.0 per cent for *OA*, 3 and 4 per cent for *OB*.

If we suppose a pure demand inflation, with money-wage rates passive until high employment is attained, the Phillips curve should follow the horizontal axis to the right of *OB* or possibly fall below it. Once high employment is reached at *OB*, the curve should rise like type *III* curve of Figure 9. If, on the other extreme, we suppose a pure supply-side inflation, with the percentage of wage increase achieved by organized labor independent of the state of employment, the Phillips curve may be approximately horizontal and lie above the *OA* line, as in type *IV* curve of the same diagram. The data seldom show any of these ideal patterns unequivocally.

Phillips was also able to explain much of his residual by introducing a second independent variable, the rate of change in the level of unemployment. This is a proxy variable for shifts in demand over the cycle. Phillips further concluded that the cost-of-living index had little effect on wage changes, except when the index rose rapidly as a result of rising import prices.

Phillips' original study of 1958 opened the way to developments along several lines, some of which are summarized in dissertations by Perry [128] and Bodkin [17]. These new studies have covered a number of other countries, introduced additional variables, and involved some disaggregation.

Further studies of the United Kingdom were made by a number of economists. Studies similar to Phillips' were conducted by Lipsey [105] and Routh [149]. Lipsey concluded that changes in the cost-of-living index helped significantly in explaining wage changes over the interwar and postwar periods. This may not be inconsistent with Phillips' conclusion, however, since the average for the period may have been heavily influenced by the years of rapidly rising prices. Routh raised questions regarding the validity of Phillips' data and his methods of aggregation, but his conclusions were roughly the same. Lipsey and Steuer [106] tested Kaldor's hypothesis [82] that wage changes in the United Kingdom were to be explained primarily by the level of profits. On the basis of time series for 10 British industries, they concluded that profits were not significant in explaining postwar changes (though they were barely significant in the interwar period), whereas (following Phillips) the level of unemployment was. Klein and Ball's study [93] of the postwar British economy also concluded that profits and productivity were not very helpful in explaining wage changes. The significant independent variables, in their estimation, were the level of unemployment, the consumer price index, and hours worked per week (a proxy for changes in the demand for labor). The significant upward shift in the wage-adjustment function after 1952, they attributed to the greater militancy of the trade unions under a Conservative government.

In a series of papers, Dicks-Mireaux and Dow [37] [38] [40] [41] carried the analysis of British postwar inflation still further. Their major innovation was to substitute the difference between "unfilled vacancies" and unemployment *V-U* for unemployment per se, thereby providing a better approximation, both theoretical and empirical, of the excess demand or supply in the labor market.[67] In addition, changes in prices and in productivity were included among the independent variables. Wages, they found, were most sensitive to changes in *V-U*, increasing by some 3 to 4 per cent as a result of a 1 per cent increase in *V-U* (at the relatively high levels of employment that characterized the postwar period). On the other hand, changes in wages were not affected as significantly by changes in prices as by *V-U*, except when the price changes were rapid; and, while increases in productivity tended to reduce prices, they had virtually no effect on wages. Their equations suggested that if, over the period studied, *V-U* had been constant at the average for the period and prices had been stable, wages would have increased by some 2½ per cent on the average (with a low of 1 per cent in the wage-restraint period, 1948-50, and a high of about 3½ per cent in the mid-'fifties). They attributed this primarily to union "pushfulness" but admitted the possibility that lagged effects from other variables might have been responsible.

Studies have been made for several other countries, particularly for the United States. The U.S. materials have led a number of writers, including Phillips himself [133], to more pessimistic results than have the British materials, regarding the possibility of checking wage inflation by monetary and fiscal measures within tolerable limits of unemployment. In terms of Figure 9, Phillips curves for the United States approach more closely our pessimistic type *II* curve than our optimistic type *I* curve. The most familiar such curve was derived by Samuelson and Solow [151]. A study by Bhatia [14a] is even more pessimistic. Dividing U.S. data by time periods, Bhatia found a curve approximating our type *IV* for his latest period, the decade 1948-58, thus suggesting that inflation in this period resulted from the operation of supply-side forces (but see below). France [55] estimated that with all other variables constant, wages in the postwar United States would rise 4.2 per cent annually with 4 per cent unemployment, and 2.4 per cent (4.1 per cent if fringe benefits are included) at 10 per cent unemployment.[68] Surprisingly, he also estimated that the corresponding wage increase for the 1890-1920 period would have been 3.1 and 1.9 per cent, respectively. The major difference between the periods, he felt, was the much greater downward wage rigidity in postwar recessions.[69] In general, the level of unemployment and its changes provided a fairly good explanation of wage changes in both periods except when prices were rising rapidly; slowly rising prices and productivity changes had little explanatory power.

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[67] They argued, incidentally, that excess demand in final output markets will usually exceed excess demand in labor markets since part of the derived demand will be directed at nonlabor factors of production. Where capital and labor are complementary and the gestation period of capital is lengthy, it takes a long time for excess demand in goods markets to make itself felt in labor markets.

Contrary to the results for England, some U.S. studies have concluded that the level of profits goes far toward explaining changes in wage rates in postwar United State. Using cross-section data by industries, Levinson [102] found that the profits variable was more significant than the percentage of unemployment. This result was supported by Bhatia [14]. Using time series, Bhatia found that the level and rate of change of profits provided a better explanation of wage changes than did the level of unemployment. This led him to the conclusion that the U.S. postwar inflation could be equally well interpreted as either profit-push or demand-pull (with prices and profits rising ahead of wages). Eckstein and Wilson [47] regressed wage changes against both profits (taken as reflecting economic conditions in the product markets) and the level of unemployment (reflecting conditions in the labor markets). Instead of using calendar-year data, they substituted "wage round" time-period data, since it had been shown [102] [110] that contracts, in fact, tend to "cluster" over periods of from one to four years. This combination of factors resulted in an improved statistical fit at the cost of a reduction in degrees of freedom. They also found strong evidence of "wage contour" effects (see Section III) and supported the conclusions of earlier researchers that changes in the cost-of-living index were important only when prices were rising rapidly.

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[68] An unpublished study by Phillips suggests roughly comparable figures.

[69] After this paper was completed, a follow-up by McCaffree [113] appeared, which suggested, for postwar United States, a difference between the wage-adjustment equations in union and nonunion markets, respectively, with higher rates of change at high levels of unemployment in the former.

A number of other studies along similar lines have been undertaken. P. Ross [148] obtained a poor fit with a Phillips curve on a "labor-market area" basis. Bowen [18] and Reynolds [141] suggested the degree of concentration in the employer's industry and percentage of workers under collective agreements as additional variables. Levinson [102] concluded that the former, but not the latter, was significant in explaining wage changes. As indicated above, using newer data, Segal [156] and Bowen [18] found both significant. Finally, Klein and Bodkin [94] studied the wage-unemployment relationship for Australia, Belgium, Canada, France, Italy, Japan, and West Germany, with results comparable to those for the United States and United Kingdom for all countries except France and Italy.

Monetary theorists have objected to the entire Phillips technique as a statistical artifact, the significant mechanism being changes in the money supply. An increase in the money supply (relative to income) simultaneously raises wage rates and lowers the degree of unemployment. Reductions in the money supply (relative to income) have the opposite effects. Some income theorists argue similarly, with the independent variables being changes in aggregate income and expenditure. Another theoretical objection is that the relevant dependent variable is the percentage change in real rather than merely money wages.

Using elements of demand-inflation theory, it would be interesting to examine the effects of different monetary and fiscal policies on Phillips curves. If demand-inflation writers are correct, the curves should (either immediately, or after short lags) resemble types *I* or *III* in Figure 9 during periods of anti-inflationary monetary and fiscal policy, even though they may resemble types *II* or *IV* in periods of "full-employment at whatever cost" in which wage and price increases are validated regardless of cause.[70] If demand theorists are correct, furthermore, these separate subperiod functions should fit significantly better than the long-period Phillips curves which combine them.

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[70] The implied criticism may be too strong, since the policy issues are controversial. Demand-inflation orthodoxy, meaning tight over-all monetary and fiscal policy in the face of wage and price increases in inelastic-demand industries A, B, C, . . . can reduce output and employment in innocent industries D, E, F, . . . while leaving A, B, C, . . . almost unaffected. The orthodox policy may be to some extent self-contradictory if it reduces demand and raises average cost in decreasing-cost or capital-intensive industries G, H, I, . . . by raising taxes and interest rates. Business and labor pressures for monetary and fiscal ease in validation of wage and price increases can therefore be supported by prima facie arguments.

3. Other quantitative studies have attempted to distinguish between demand and supply inflations with the aid of correlation techniques in disaggregated models. Selden [157], and subsequently Attiyeh [4], asked the question: which specific prices rose most during a given inflationary episode (or series of such episodes)? Using U.S. data, they showed that output simultaneously increased most significantly in sectors with greatest price rise. To answer the questions of why other prices did not fall, or why the general price level rose because particular prices did, these writers rely on monetary expansion rather than either income expansion or sectoral inflation. Their argument runs in correlation terms: the higher the positive correlation between price and output increases (both in percentage terms), the better the case for demand rather than supply inflation. Less than perfect correlation is to be anticipated, since demand, supply, and income elasticities vary as between commodities.[71] It is not clear how high the positive correlation must be to confirm the demand-inflation hypothesis or how these writers distinguish their results from those of Schultze-type sectoral inflation.[72]

Phelps [130], also using U.S. data, carried the argument further. In a world of shifting supply and demand curves, he doubted the significance of the sign of price-quantity correlations of the Selden variety. If demand but not supply functions were shifting, Selden's results would follow, but if supply functions were shifting for reasons unrelated to cost inflation, one should expect correlations of the opposite sign. Phelps therefore compares the absolute sizes of price-quantity correlations in the 1950's with those in the previous decade. He supposes that the immediate postwar inflation of 1946-48 was fundamentally a demand inflation.[73] When, for the later decade, price-quantity correlations were significantly smaller in absolute value than those of the base period 1946-48, Phelps argues that the role of supply factors was more significant in the later period.

4. In a controversial study, Eckstein and Fromm [45] used input-output techniques to estimate the proportion of postwar (1947-58) U.S. industrial price inflation which can be traced to a single major industrial raw material, namely steel. They reach the surprisingly high results of 40 per cent for wholesale prices and 23 per cent for finished

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[71] To the extent that a cost-push inflation is self-validating via increases in demand that result from the redistribution of income from profits to wages, the observed correlations give a misleading idea as to the ultimate cause of inflation [77].

[72] It should be remembered that Selden's paper antedated Schultze's thesis by over a year.

[73] Chandler, in his study of U.S. inflation, 1940-48 [28, Ch. 16], feels that the 1946-48 inflation was a mixed inflation with cost as well as demand elements. However, the bulk of his evidence shows merely that wages rose more rapidly than productivity. (See paragraph 6 below.)

goods prices. This means that, had steel prices risen by no more than the average of U.S. wholesale and finished-goods prices, 40 per cent and 23 per cent, respectively, of the "creeping inflation" of this period would have been avoided.

Like most other input-output studies, this one ignores possible substitutions in production between steel and other industrial materials. It also ignores possible substitution in consumption between commodities using more steel and those using less. Had steel prices risen less, demand would have shifted to some degree in favor of steel as against rival materials. Prices of these rival materials would therefore presumably have risen less than they did. There would have been less inflation over all, but the apparent responsibility of steel might have remained the same, or even increased, by the application of the Eckstein-Fromm technique.

5. In his paper for the Rio de Janeiro conference on "Inflation and Growth in Latin America," Harberger [71a] uses aggregative regression analysis of the rate of price change in Chile for quantitative comparison of the validity of monetary and structural explanations for inflation. Harberger considers all nonmonetary factors, including what we have called cost-push, as aspects of structural inflation. The variables he uses in his analysis are:

P_t: per cent change in consumer price index from December of year $(t-1)$ to December of year (t).

Y_t: per cent change in real income from the fourth quarter of year $(t-1)$ to the fourth quarter of year (t).

M_t: per cent change in money supply from December 31 of year $(t-1)$ to December 31 of year (t).

M_{t-1}: per cent change in money supply from December 31 of year $(t-2)$ to December 31 of year $(t-1)$.

A_t: per cent change in price level during year $(t-1)$ *minus* per cent change in price level during year $(t-2)$. (Harberger refers to this as an *acceleration* factor.)

W_t: per cent change in minimum wage for public employees (*sueldo vital*) from the first quarter of year $(t-1)$ to the first quarter of year (t).

Harberger presents three equations. The first is purely monetary, allowing for a two-year distributed lag. The others add the structural variables A_t and W_t, without marked improvement in the fit of the function. We reproduce his results for annual data in the 1950's; similar computations for quarterly data are less impressive:

$$P_t = -1.05 - 1.05\ Y_t + .80\ M_t + .34\ M_{t-1} \qquad (R^2 = .84)$$
$$P_t = -0.32 - 0.91\ Y_t + .74\ M_t + .34\ M_{t-1} + .20\ A_t \qquad (R^2 = .87)$$
$$P_t = -1.15 - 0.89\ Y_t + .70\ M_t + .29\ M_{t-1} + .16\ A_t + .13\ W_t \qquad (R^2 = .87)$$

Not only do the results appear reasonable (with the "right" signs), but:

> The constant terms (indicating what would happen to the price level if real income, money stock, etc., remained unchanged) are not significantly different from zero. The coefficients of the change in income are not significantly different from −1, indicating that a one per cent rise in real income, other things equal, causes close to a one per cent fall in the price level. The coefficients of both the current and the lagged change in the money stock are statistically significant, and their sum is not significantly different from +1, indicating that a one per cent increase in the quantity of money causes a rise of close to one per cent in the price level, other things equal. When . . . the acceleration of prices (A_t) is added as an explanatory variable, its coefficient turns out to be marginally significant at the 5 per cent level. When, however, the change in wages is also added, the coefficient of A_t loses significance, while W_t itself adds nothing to the explanation of the rate of inflation.

While Harberger's results support the demand explanation of inflation (in its monetary or quantity-theory version), they do not attempt refutation of the structuralist claim that expansion is a necessary result of the need for growth under Latin American conditions. That is to say, they treat Y_t as independent of the two monetary variables M_t and M_{t-1}, which begs one of the questions at issue in the monetarist-structuralist controversy.

6. During the creeping inflation of the 1950's, as emphasis shifted from demand to supply theories of inflation, it became a common practice to distinguish between types of inflation by comparing increases in money-wage rates and increases in average real labor productivities. Policy exhortations in a number of countries, including the United States,[74] have come to include this test: If the percentage

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[74] Two statements, less careless as written than as interpreted popularly, can be found in the 1962 *Economic Report of the President* [48, pp. 17, 186]:

> When better plant and equipment enable the labor force to produce more in the same number of hours, there is more to share among all the contributors to the productive process—and this can happen with no increase in prices. Gains achieved in this manner endure, while gains achieved in one turn of the price-wage spiral vanish on the next. . . . If labor leaders in our major industries will accept the productivity benchmark as a guide to wage objectives, and if management in these industries will practice equivalent restraint in their price decisions, the year ahead will be a brilliant chapter in the record of the responsible exercise of freedom.
>
> If all prices remain stable, all hourly labor costs may increase as fast as economy-wide productivity without . . . changing the relative share of labor and nonlabor incomes in total output. . . . If hourly labor costs increase at a slower rate than productivity, the share of nonlabor incomes will grow or prices will fall, or both. Conversely, if hourly labor costs increase more rapidly than productivity, the share of labor incomes in the total product will increase or prices will rise, or both. It is this relationship . . . which makes the rate of productivity change an important benchmark for noninflationary wage and price behavior.

rate of money-wage increase exceeds the percentage rate of increase in man-year (or man-hour) labor productivity, inflation arises from the side of supply. Conversely, if the rate-of-wage increase is no larger than the rate-of-productivity increase, any residual inflation must arise from demand.

The attraction of this test is statistical simplicity, but many writers have shown it to be fallacious.

a. It leads to ridiculous results. This test makes any episode a cost inflation when money wages rise faster than the normal rise of man-hour productivity, whatever happens to living costs or to real wages. The test also implies that, if productivity were temporarily constant, demand inflation would not raise money wages at all. We know that any substantial inflation, regardless of cause, actually raises money wages.

b. The test pertains only to one type of cost inflation, from the side of wage rates. The role of profit margins is ignored.

c. The results vary with the base period. While it is customary to use the end of World War II or the Korean War, labor spokesmen suggest the early 1920's, when wage rates fell, relative to average labor productivity in the United States.

d. The test assumes the impossibility of redistribution of the national income as an alternative to inflation.[75] A redistributive alternative, from profits to wages, has been proposed by several writers, perhaps most consistently by Keyserling [35] [91]. The sharpest opposition in professional circles is S. Weintraub's [181] [182]; Weintraub maintains that the ratio between industrial gross sales and industrial payrolls is a short-run statistical constant of approximately 2.0, which he calls the "magic *k*."

e. Productivity theories of wages and of demand for inputs stress marginal rather than average productivity. The test supposes that marginal and average productivities of labor move proportionately. This may not be true for major shifts in productive techniques, such as automation.

V. *Speed, Duration, and Extent of Inflation*

Our definitions of inflation imply that an inflationary process occurs when active claims for shares in the real national product, expressed either in terms of expenditure- or income-demands, exceed the amount that is produced. Inflation continues until these conflicting claims are somehow reduced or otherwise reconciled. The speed of the inflation that follows an initial inflationary shock (excess demand or excess claims on income) depends upon the reactions of various income and spending groups in the economy to that shock. If there are few reactions, and if these are delayed or not very vigorous, inflation will proceed slowly and will not go much beyond the initial shock. If, on the other hand, reactions and successive "reactions to reactions" are numerous, rapid, and vigorous, the resulting inflation can be explosive. Along these lines the properties of inflation will depend on the various factors set forth below after an inflationary shock of given size.[76] It is important to note that significance of many of these factors will vary from nation to nation and from time to time depending on the following conditions: the militancy of labor unions; commitment of authorities to full employment; climate of popular opinion regarding increases in standard of living, future price trends, etc.; marginal propensities to spend;[77] and so forth.

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[75] Phelps [130] suggests that a rise in the labor share of national income during inflation may be evidence of cost inflation, while a rise in the profit share is evidence of demand inflation. This ignores point (b) of the text immediately above and assumes that the labor share follows the wage rate. As Phelps himself points out, "Variations in the composition or structure of demand will generally affect distributive shares. An increase in the demand for commodities which are comparatively labor intensive can raise wages and lower profits. Also, changes favoring products in the noncorporate sector will naturally reduce corporate profits."

A. *Effects on Expenditure-Demands*

Rising prices and wages have a dual nature in the aggregate: rising prices are costs to buyers and incomes to sellers; rising wages are costs to producers and incomes to laborers. For this reason, rising prices do not necessarily imply increases in quantity supplied or reductions in quantity demanded as in the case of individual commodities, and it is therefore possible, in theory, for an inflation to continue forever [154]. This will not be the case, however, because the process of rising prices and incomes does affect (usually reduces) the level of real expenditures through a number of different channels [2] [107] [154] [170].

1. Real demand declines to the extent that spenders (households, firms, governments) are subject to money illusion and plan their purchases in money rather than real terms.[78] At full-employment inflation, maintenance of real demand would require a rise in the marginal propensity to spend to equality with the average propensity to spend (both

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[76] The extent of the inflation, of course, also depends significantly on the size of the inflationary shock.

[77] Harberger [71a] suggests that the major reason underdeveloped nations are more inflation-prone than developed nations is their smaller ratio of money stock to national income. Under these circumstances, a given budget deficit tends to result in more of an increase in prices than where the $M:Y$ ratio is higher. One factor that might explain the differences in ratios could be a higher marginal propensity to spend in the underdeveloped nations.

[78] Assume here zero price elasticity of expectations.

in money terms) [63]. This is unlikely in the absence of special assumptions regarding expectations.[79]

2. The impact on real demand will depend significantly on the method used by the government (and to a lesser extent by households and firms) to increase its money expenditures to compensate for higher prices. In the government case, increased expenditures financed by an increase in taxes will be least expansionary; those financed directly or indirectly by an increase in money supply, including bank credit, most expansionary.

3. Inflation tends to redistribute income (see Section VI) away from rentiers, pensioners, and the aged, and toward other groups. In a pure demand inflation, there may be a shift from wage earners to profit receivers. In a wage-push inflation, the shift will be to employed workers from profit receivers and those who have become unemployed. Since the marginal propensities to spend of these groups differ from one another, the aggregate propensity is changed in an inflation. The net effect in the first two cases cited is likely to be a small decline in real demand. In the wage-push case, an increase in real demand is a possibility.

4. To the extent that an economy's taxes on income and profits are progressive, inflation automatically increases the proportion of taxes to income, which reduces private spending. Aggregate real demand is also reduced to the extent that added tax receipts do not directly induce a compensating increase in government spending.

5. If prices rise faster than the quantity of money, real balance effects will tend to reduce household real expenditures at the higher price levels.[80] Investment by enterprises is also likely to decline because of the decline in the real value of firms' liquid balances, the consequent need to finance an increasing portion of investment needs from borrowed funds,[81] and the reduced availability of credit. On the other hand, to the extent that rising prices raise expectations of profits (reduce real interest rates), the inducement to invest increases.

6. The relative rise in wage rates and decline in profit rates in a wage-push inflation may reduce investment by business in the short run.

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[79] Paunio [126a] argues that price expectations are elastic and increase real demand, but Mueller [120] demonstrates that inelastic expectations have largely prevailed in the U.S. postwar economy. See next footnote.

[80] As indicated below, however, if rising prices engender expectations of further price increases, real balance effects are offset and real household expenditures may increase. These expectational effects will be transitory unless prices are increasing at an increasing rate. As Cagan shows [26], this is only likely to occur over the long run if the nominal money supply is increasing at an increasing rate.

[81] Duesenberry [44] argues that management is less likely to invest if the requisite financing must be obtained externally rather than internally.

Over the longer run, investment in labor-saving equipment may increase.

7. Inflation that is more rapid in one country than in others tends to reduce the volume of exports and to increase the volume of imports, thereby reducing demand for domestically produced goods and increasing the supply of goods available on domestic markets.

To sum up, rising prices and incomes lead to a decline in real demand in many of the circumstances cited above, increases in others. If the former dominate, this would tend to slow down the pace and extent of inflation but would halt the inflationary process only if decline in real demand were equal in magnitude to the original inflationary shock. Unfortunately, little is yet known about the quantitative importance of the various factors mentioned.

B. *Effects on Income-Demands*

The level of unemployment, and changes in it, will affect the ability of different economic groups to "push" for higher incomes. Thus, in a demand inflation, unemployment declines and profits rise. Inflation accelerates as unions achieve larger wage increases, which, in turn, lead to higher prices. On the other hand, in a cost-push inflation, unemployment will probably increase, thereby reducing subsequent wage or profit pushes until eventually the inflation is halted.[82] This process is thwarted, of course, to the extent that the government, following a full-employment policy, validates wage and price increases which would otherwise lead to rising unemployment.

C. *Lags*

The lags in normal economic sequences strongly influence the character of inflationary processes [45] [127] [164] [170]. Without lags, an inflation would run its course almost instantaneously; the existence of lags is largely responsible for the manageable rate of inflation in most Western nations today. Perhaps the most important lag is that of wages. For union workers this lag is a result of the fact that collective bargaining is rarely initiated more frequently than once a year, and then negotiations often drag on for months. The process is also slowed because a few months usually elapse before price changes are reflected in cost-of-living indices. The wage (income) lag is usually even greater for unorganized workers,[83] government employees, teachers, pensioners, and the aged. In fact, for members of some of these groups the lag is so great that in any given period they are more accurately considered to have fixed incomes and not to participate actively in the inflationary process. The greater the size of these relatively "passive" groups, the slower and smaller the price rise.

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[82] As indicated above, the redistributive effects of a wage-push induce an increase in real demand. It has been demonstrated that the increase in demand is not likely to offset fully the unemployment effects of the wage-push [77].

[83] During the more rapid price increases, unorganized workers may be able to adjust their wages more frequently than organized workers.

In those industries in which prices are administered on the basis of cost increases, the cost-change to price-change lag is probably much less than that of wages to changes in cost of living. However, such administered prices may be relatively insensitive to changes in demand.[84] Thus, in a demand inflation, as noted earlier, there may be a substantial stabilizing lag between the development of excess demand and subsequent increase in prices [1]. Other lags appear to be of lesser importance. In the government sector, there will be both receipts and expenditure lags: increases in tax receipts induced by inflation are not immediately received by the Treasury; and government expenditures often substantially lag appropriations. There is very little lag between the earning and receiving of income, except perhaps in the case of dividends. Similarly, the lag between receiving and spending income appears to be of little consequence.

D. *Wage Escalation* [79]

Wage increases due to escalation are no different in principle from those directly negotiated. If the escalated increases are no greater than warranted by excess demand for labor, they can be considered on a par with demand-induced wage increases. If, on the other hand, the arrangements are such that wages are forced up against deficient demand, the results are analogous to a cost-push. Furthermore, if the escalated increases just substitute for wage increases that would otherwise have been attained, then the institutional arrangement itself cannot be considered to have contributed to (detracted from) the inflationary process.

The impact of escalation in any period depends on the rate and frequency of adjustment and on the percentage of the labor force covered. If the total labor force were covered, and wages were escalated immediately and proportionately to changes in the CPI, a crucial stabilizing "lag" would be eliminated from the wage-price system with consequences indicated above.

The instability is increased if automatic wage increases are granted, not only for changes in the CPI, but for increases in productivity as well.[85] Typically, these various conditions are not met and the "lag" in wage adjustment is preserved at least in part. Although in some countries (e.g., Australia and Israel) virtually all wages are escalated, this is not the case for most nations. In the United States, less than 5 million workers have at any one time been subject to wage-escalation agreements; in Great Britain the number covered has not exceeded 10 per cent of the labor force. In theory, adjustments (in most nations) are made either quarterly or semiannually. In practice, adjustments are less frequent, because they usually do not take place until the CPI registers a certain minimum increase, which may be as high as 10 per cent. Finally, while some contracts call for escalation compensation for 100 per cent of the increase in the CPI, escalation typically provides only partial protection against price increases.

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[84] The significance of administered pricing in our economy is largely denied by Bailey [9], Flueck [54], and others.

[85] This "improvement factor" or "productivity factor" was first introduced in this country in the famous May, 1948 GM-UAW contract. To escalate wages for increases in

Statistical studies suggest that formal wage escalation has had no discernible influence on the speed of inflation in the United States. Not only are the numbers of covered workers very small, but wage increases in covered industries have not been greater than in noncovered industries. The same conclusion has been reached in studies of British and Danish escalation. These results suggest, as indicated above, that escalation in these countries serves primarily as a substitute for, rather than an addition to, other wage increases, and its extent is largely determined (as with wages in general) by market forces.

E. *Consumers' Expectations* [24] [26] [33] [92] [120]

It is well known that expectations play an important role in speeding up inflation, once price increases are already rapid. Hyperinflations are characterized by a "flight from cash," which is a more rapid increase in the price level than in the quantity of money. It is brought about by a reduced demand for real cash balances[86] and is evidenced by an increase in the velocity of circulation. Historical experience suggests that a marked "flight from cash" does not usually begin unless prices are at least doubling every six months [24, p. 195].[87] Apparently the public is willing to pay a fairly high price for the convenience of holding its customary stock of cash balances [33]. We will first consider the importance of buyer expectations in the creeping inflation that has characterized the U.S. economy in the postwar period and then turn briefly to the role of expectations under hyperinflationary conditions.

Expectations of price increases may cause consumers to alter the

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the CPI alone would, of course, freeze real wages (in the absence of other mechanisms for increasing wages). The GM-UAW contract was for more than one year, and it was natural under these circumstances for an "improvement factor" to be introduced.

[86] This is because the real cost of holding cash becomes very great when prices are rising rapidly.

[87] The historical experience referred to is the fact that many nations have had years of fairly rapid inflation without developing a signficant and continuing rise in velocity.

time pattern of spending, even though they do not lead to a marked "flight from cash." The only studies available to us [33] [120] are based on Michigan Survey Research Center data for the postwar period in the United States, a period in which the rate of inflation has been relatively slow. The results, therefore, cannot be generalized, but are not without interest and so are summarized briefly.

First, people differ substantially in their perceptions of past price trends. In periods of gently rising prices, not everyone recognizes that prices have risen. However, the longer that prices continue to rise, the larger the percentage of households aware of the trend.[88] Second, recognition that prices have risen does not necessarily lead to anticipation of future price increases. Third, and perhaps most surprising, the awareness by households that prices have risen and their expectations of further price increases do not necessarily lead them to speed up purchases; in fact, in the United States in both 1951 and 1957, high and rising prices led to a drop in expenditures on consumer durables. This occurred, according to interview data, because people were dissatisfied with high prices and so went on a buying strike; this attitude was more widespread than its opposite—"buy before prices become still higher." The incentive to speed up purchases is not likely to be strong in a creeping inflation, when prices are rising by, say, 2 per cent a year. As Conard [33] points out, an expected 2 per cent price increase should have no more impact on consumer-spending plans than a decline of 2 per cent in the interest rate.[89]

Finally, it is important to note that, taken alone, an increase in expenditures based on expectations of a price rise can cause a "once and for all" price rise, but not lead to continuing inflation. So long as people desire to hold some cash balances in periods of rising prices, and this seems to be the case, their attempts to shift from cash to goods are limited and not continuing [10, p. 53]. Expectations cease to affect the rate of price change once the community has reduced the real value of its nominal stock of cash to that amount it wishes to hold, given its anticipations of future price changes. Further increases in prices would require either a rising elasticity of expectations or an increase in the money supply. It is possible, of course, that elastic price expectations could so increase expenditures and prices as to lead to an upward revision in expectations, still larger expenditures and higher prices, and so on into a self-generating hyperinflation. Experience suggests that this is highly unlikely. In his study of seven hyperinflations, Cagan [26] demonstrates that each of these hyperinflations was primarily the result of increases in the quantity of money issued by the government in an attempt to maintain the real value of its revenues under rapidly rising prices. Velocities of circulation, of course, increase tremendously but not by the same order of magnitude as the quantities of money. Cagan attributes the failure of velocity to increase more rapidly to ". . . individuals' lingering confidence . . ." (p. 88) in the future value of money. This causes their expectations of price increases to lag behind actual price increases. He does show, however, that toward the end of each hyperinflation the lag apparently shortens as a result of an acceleration in the rate of price increase, which leads in turn to an acceleration of the increase in velocity of circulation. But even in these periods, the data show (p. 26) that the acceleration in the increase of note-issuance is primarily responsible for the breakdown of the currency system.[90]

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[88] On the other hand, as wage earners and as investors, individuals' perceptions of price changes are much sharper and are reflected much more quickly and directly in labor and investment markets, respectively.

[89] A later paper by Kessel and Alchian [87a], which came to our attention too late to be dealt with here, provides an extensive theoretical analysis of the effects of inflation, differentiating between unanticipated, anticipated, and transitional price increases.

VI. *Redistributive Effects of Inflation*

Inflation alters the distributions of income and wealth because individual prices of current goods and services, productive factors, monetary and real assets, and liabilities are not equally flexible and do not respond equally to general inflationary pressures. (Individual prices do not respond equally, partly because, when inflation reduces real-money balances, supply and demand functions for goods and services change in different degrees, depending on their complementarities [positive or negative] with cash-balance holdings.) The prices of some monetary assets and liabilities (e.g., cash, insurance policies, savings bonds) are completely inflexible. Most monetary rent and interest incomes in the United States and Western Europe are fixed contractually for long periods and adjust slowly to rising prices. (Rent and interest incomes *in kind* are favorite inflation hedges in underdeveloped countries.) Prices of physical assets held for use, rather than sale, can be assumed to adjust proportionately with inflation. Most other prices tend to rise with inflation but at very different rates: the price of durables tends to rise faster than the price of food; wages of government workers and professionals typically lag behind those of industrial workers.[91] Because of their inflation-sensitivity, real estate, precious metals, jewelry, art objects, and stable foreign currencies are often used as inflation hedges.

[90] Whether or not these inflations would have been self-generating at this point could only be determined, it would seem, if the increase in supply of currency were cut off but there was no currency reform.

[91] These figures reflect the experience of postwar United States and may not be characteristic of other time periods or of other nations.

Complete assessment of the redistributive impact of inflation requires consideration of the relative impact on individuals, groups, organizations, and sectors of the differential inflation-sensitivity of all these factors. It is often difficult, however, to separate the effects of general inflation on relative prices from those due to changes in other supply-demand relationships, including economic growth. Thus, if wage rates and prices rose by the same percentage, this would not mean that wages were keeping pace with other factor shares if, at the same time, productivity increases had caused a large rise in real GNP. Similarly, a rapid increase in wages of electronic specialists may represent either inflation-sensitivity or an increasing real demand for this type of labor.[82]

The differential impact of inflation on individuals and groups is a function of two factors: first, their ability to foresee inflation; and, second, their ability to adjust their economic behavior (e.g., put savings in common stocks rather than savings bonds, include escalation clauses in union contracts, etc.) to inflation [8, Ch. 3]. If everyone had equal ability to predict and adjust to inflation, inflation would have no redistributive effects. In fact, however, there are very great differences among individuals and groups on both counts, and these are responsible for redistributions of income and wealth during periods of rising prices. It should also be remembered, however, that if the argument of Section V is correct, the larger the number of people who correctly anticipate price rises and the greater their ability to adjust to them, the more rapid the inflation is likely to be.

A. *Income*

Two related hypotheses on the redistributive effects of inflation have been accepted for many decades but have only recently been analyzed [3] [7] [50] [85] [86] [87]. The first hypothesis states that in an inflation money wages tend to lag behind prices; as a consequence, real wages fall and real profits rise. The second hypothesis states that business firms gain through inflation.

The wage-lag hypothesis has been advanced in a number of historical studies. The best known of these are E. J. Hamilton's studies of profit inflation as a cause of industrialization in Western Europe over the centuries of New World colonization, W. C. Mitchell's study of the North and E. Lerner's study of the South during the Civil War, and A. H. Hansen's work on wages and prices during World War I and its aftermath (cited in [89]). Each of these studies concluded that real wages declined during inflation and as a result of inflation.

These conclusions are strongly challenged (in several cases) by Alchian and Kessel and by Felix, either because of faulty methodology or because the observed wage lag was explainable in terms of changes in the underlying real variables. For example, Alchian and Kessel argue that the 30 per cent decline in real wages in Spain during the inflation of 1520-1600 does not represent a trend at all but results from the choice of end-years. Thus, the apparent decline is turned into a 4 per cent increase by the simple expedient of taking the period 1522-1602 as representative. Hansen's study of real wages during and after World War I is subject to a similar methodological deficiency.[93] Hamilton's study of Spanish inflation during the 18th century and the apparent decline in real wages in urban areas is a case in which the underlying real factors seem to provide an adequate explanation. Over this period, the Spanish population roughly doubled; associated with this, there was a notable migration to the cities. One cannot be confident of the wage-lag hypothesis in this case until the downward pressure on wage rates of the increased urban labor supply has been taken into account. In general, Felix shows that the trends in the ratios of wages to prices in most countries were largely explainable by demographic and institutional influences on the supply curve of wage labor. Real factors are also adduced by Alchian and Kessel to explain the decline in real wages in both North and South during the Civil War.

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[82] The preceding paragraphs were taken largely from Holzman [79].

Alchian and Kessel further argue in regard to a recent period in the United States that, if the wage-lag hypothesis is valid, then ". . . firms with large annual wage bills would experience an increase in profits (and wealth) relative to firms with smaller annual wage bills . . ." [87, pp. 59 f.]. This is tested by comparing, for 113 firms over the 1940-52 period, the wage-to-equity ratio with the percentage change in market value of stock outstanding. The correlation is negative, suggesting that the hypothesis is wrong at least in a period of strong labor organization.

Bach and Ando [7] and Conard [34] attempt to cast further light on the income-redistributive effects of inflation by examining the changes in factor shares in personal and national income over the past 20 years. With regard to wages and profits, the picture is very mixed, in some years apparently conforming to the wage-lag hypothesis (1946-48), in other years not (1949-53, 1955-58). Of course, to the extent that the distinction between "push" and "pull" inflations is valid, one would expect, other things equal, to find wages rising faster than profits in the former, and losing out to profits in the latter. Phelps [130] suggests that this may be the explanation of the opposing trends observed in the 1946-48 and 1955-57 inflations, respectively.

A difficulty in interpreting these data is that they take insufficient account of changes in factor proportions. For example, in the aggregate, labor income rises, whereas income from unincorporated businesses declines. However, adjusting these figures for the sharper relative increase in the number of wage earners relative to proprietors yields unit increases that are quite similar. To this must be added the fact, mentioned above, that profits tend to vary with sales and output as well as prices. Conard also found that, if one looked at the postwar period in cyclical terms, the relative share of profits tended to rise in the early stages of recovery, and of wages in the later stages. This observation is supported by Kuh, who explains it as follows: wages tend to increase steadily, relative to prices, over the cycle. Productivity, on the other hand, increases more rapidly in the earlier than in the later stages of recovery. (This happens for a number of reasons, but primarily because capacity is used more efficiently in the earlier stages.)

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[93] Hansen's study contains another methodological deficiency, viz. that weekly rather than hourly earnings are taken as the measure of changes in money wages in a period when hours of work were declining.

The foregoing does not imply that inflation has no impact on income distribution. With regard to factor returns, inflation in the postwar period seems to have eroded quite unambiguously the shares going to interest, rent, and dividends.[94] Hashimi's study [73] shows that the "active" elements (wages and profits) gained relative to the "passive" elements (rent and interest) whenever prices rose in the 1929-57 period. Perhaps even more important, however, within each group the incomes of many individuals are left behind as incomes and prices in general soar. In Bach and Ando's words, ". . . on the whole, the major losers from inflation on current income account—the relatively fixed income groups—were clearly scattered throughout the economy as pensioners, insurance recipients, college professors, and others, rather than concentrated in major income size or occupation groups" [7, p. 5].[95]

B. *Wealth*

The second hypothesis, that business gains from inflation, is based on at least two assumptions. First, as already noted, if it were true that wages lag behind prices, as Hamilton and others claimed, it would follow that business profits would rise during inflations. Second, a more common assumption, associated with J. M. Keynes and Irving Fisher (cited in [3]), is that business firms are debtors and gain during inflation by being able to repay debts in depreciated currency.[96] A corollary to this is the proposition that banks, being particularly large debtors (i.e., having a high ratio of debt to equity) should gain disproportionately in an inflation, although paradoxically this did not seem to be the case.

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[94] The figures for dividends are misleading because of low pay-out ratios that increased capital gains.

[95] Incidentally, Conrad points out that inflation does not appear to have had any particular redistributive effect in terms of income receivers classified according to *size* of income. His observations cover the entire period since World War I, as well as data adduced by other writers for other nations.

[96] That debtors gain during an inflation at the expense of creditors depends for its validity on the assumption, probably true, that when loans are made, bonds are bought, etc., the interest rates reflect an underestimate of the future rise in prices.

The studies of Alchian and Kessel [3] [87] tend to confirm the Keynes-Fisher mechanism demonstrating the importance for redistribution of debtor-creditor status in an inflation. They took issue, however, with the assumption that all firms are debtors. While this assumption appeared to have been generally true in the pre-World War I period, the Alchian-Kessel samples for post-World War II were evenly distributed between net debtors and net creditors. In each sample examined, the stock prices of debtor companies rose by much larger percentages than those of creditor companies. Kessel [85] resolved the "bank paradox" by carefully scrutinizing the balance sheets of a sample of 16 banks. He discovered that banks did indeed have large debt-equity ratios and in this sense might be considered large debtors. On the other hand, he found that their assets were almost exclusively monetary and exceeded, in every case, their monetary liabilities. Thus, banks should be expected to lose during inflations by the Keynes-Fisher mechanism; and they do.

Bach and Ando's work [7], covering the periods 1939-52 and 1952-54, substantiates that of Alchian and Kessel regarding the operation of the debtor-creditor mechanism. They did find, however, that the operation of this mechanism on the relative positions of enterprises is often obscured by the effects of other factors which operate in inflationary and noninflationary periods alike. Most significant of these in the determination of profits and stock prices was sales volume. Closely related to this, Kuh [96] found changes in productivity and output to be the most important determinants of fluctuations in corporate profits in the United States in the postwar period.

Bach and Ando, utilizing data collected by Raymond Goldsmith in his monumental *A Study of Saving in the United States*, analyze the effect of inflation on the distribution of wealth in terms of the net *monetary* (fixed price) assets or liabilities of different population groups.[97] The aggregative figures show that, in the United States over the 1939-49 period, households became the largest net creditor, the government (federal, state, and local) the largest net debtor, whereas unincorporated business and nonfinancial and financial corporations were on the whole neutral. To assess the impact of inflation on individuals, it is more revealing to look at the percentage of net assets held in monetary form by personal income group before taxes. All income groups with the exception of the lowest, "under $1,000," have a net monetary asset position which amounts to 13 to 15 per cent of their total assets, i.e., which is roughly equal to the average of 14.6 per cent noted above. The "under $1,000" group records 7.8 per cent.[98] While this indicates that the losses from inflation are likely to be smallest for the low-income group with all other groups about equally vulnerable, it hardly provides an index of the true burden of inflation. Another approach is to relate the losses on net monetary asset position from a given price rise to the estimated mean income of the different income groups. Crude calculations along these lines [79] suggest that, relative to income, the losses on net monetary account from inflation are largely regressive.

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[97] This figure is biased because of the understatement by households of some of their liquid assets and the exclusion of currency from the estimates [7, p. 7].

Pesek [129] carries the analysis further. Arguing that inflation can be considered an alternative to taxation, he compares the impact of inflation on the net monetary asset position of different income groups with the impact of equivalent income and sales taxes. His findings are similar to others': inflation turns out to be a more regressive form of tax, especially for the lower income groups, than either income or sales taxes. Perhaps the most comprehensive study along these lines is Brownlee and Conrad's [25], the full results of which await publication by the Commission on Money and Credit. They compare the distributive effects of inflation with the following alternative policies: tight money, increased taxes, and decreased government expenditures. They agree with Pesek that the burden of combatting inflation through higher taxes is more progressive than inflation. Inflation per se, they find, is not only relatively (to other techniques) regressive, hurting the poor proportionately more than the rich (as Pesek [129] showed), but is absolutely regressive in the sense that (for 1957) those with incomes under $6,000 suffer a real decline in net worth while those with incomes over $7,500 have their real net worth increased.

Utilizing data from Bach and Ando, it is also possible to calculate the percentage of net monetary assets to net worth, for households classified by size of net worth. The percentage increases with net worth except for the lowest and highest categories: the 33 per cent of households comprising the $0-1999 net-worth group hold 19 per cent of their assets in monetary form, and the 3 per cent of the households in the over-$60,000 group hold 14½ per cent. This suggests a regressive wealth effect of inflation in the highest and lowest net-worth classes and a progressive wealth effect in the remaining four classes.

The real locus of the burden of holding fixed-price assets in an inflation is revealed by making the same calculations by occupational and age groups. The highest percentage by far, 29.7, is recorded for the retired. Relatively high percentages are also indicated for professionals and semiprofessionals (24.3), and unskilled (19.6). Farm operators and managerial staff, on the other hand, suffer minimally, recording 1.1 and 6.4 per cent, respectively. The high figure for retired people, just noted, is emphasized by the age-group breakdown where those in the "55 and over" group, which includes most of the retired, record a much higher percentage, 19.7, than any others in this classification. The aged and retired also predominate in the low-income groups which, as we indicated earlier, bear a regressively heavy burden from the erosion in value of their monetary assets.[99]

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[98] This lowest income group is not quite comparable with others since it contains an unusually high proportion of negative "transitory" incomes.

VII. *Final Remarks*

We have viewed inflation theory from different policy viewpoints[100] but with a minimum of disagreement and, we hope, inconsistency. If our experience has any relevance for the future, inflation theory may soon reach a new synthesis around which an increasing proportion of students can find a meaningful basis of agreement.

As our long, but still incomplete, bibliography suggests, the literature on inflation is vast. We have found it impossible to encompass the whole field in the time at our disposal. We have undoubtedly overlooked important contributions by a number of our colleagues. We have omitted completely discussion of two important aspects of the inflation problem—international aspects and methods of control. We have understressed unintentionally the sociological, institutionalist, and structuralist literature, as well as materials from many foreign sources (particularly Dutch and Scandinavian), in favor of what was readily available in the English language. Nor did we see in time for extended use either Biacabe's historical study of inflation theory from the viewpoint of the French sociological school [15] or the papers presented at the Rio de Janeiro conference (January, 1963) on inflation in Latin America.

Gazing into our respective crystal balls, we guess that progress in inflation theory over the next decade or so will take two forms: (1) better quantitative estimates (for different times and places, also different lengths of run) of many effects we have noted only qualitatively; and (2) simpler and more usable syntheses of the three or four main streams of thought we have noted in this survey—demand inflation, supply or cost inflation, interactions with sociological economics, and interactions with the burgeoning theory of economic growth and development.

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[99] In 1957, of single individuals 65 years of age and over, 60 per cent had incomes of less than $1,000, and 80 per cent less than $2,000; in the same year, 35 per cent of families whose head was 65 years or over, had less than $2,000 annual income [98, pp. 8-9].

[100] Bronfenbrenner believes monetary and fiscal rules can control both demand and supply inflation without long-run unemployment, international complications, important sacrifices in economic growth, or important class conflicts over distributional issues. Holzman is less sanguine with respect to monetary and fiscal policy. His major interests have been in reducing the inequities of inflation by selective use of escalatory devices and, following the Scandinavians [171], in techniques of setting noninflationary limits within which otherwise free collective bargaining can operate.

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ENGINEERING ESTIMATES OF CAPITAL-LABOR SUBSTITUTION IN METAL MACHINING

By MORDECAI KURZ AND ALAN S. MANNE*

In attempting to predict the possibilities for direct substitution between machine tools, Markowitz and Rowe [4] published a comprehensive set of technical norms. These engineering estimates are intended to represent the number of pieces per day that can be produced by one worker utilizing alternative machine tools to perform a variety of metal-removal tasks. Unlike census data gathered from a cross section of establishments in a single country and point of time, the Markowitz-Rowe (hereafter M-R) estimates are intended to represent a set of feasible *alternatives* independent of the relative costs of labor and of individual types of capital equipment.

Shortly after the original M-R publication, Hollis Chenery [2] suggested that, by assigning an investment cost to each machine tool, it would be possible to use these data so as to construct capital-labor substitution functions for each individual task. With such functions, it should in turn be possible to make *ceteris paribus* predictions as to how methods of production might vary from one point in time or space to another, depending upon the relative prices of the two substitutable inputs, capital and labor.

Now if production functions could indeed be synthesized from engineering data, less reliance would have to be placed upon those indirect methods of estimation that are based upon the assumptions of universal competitive equilibrium.[1] Instead of starting with the assumption of competitive equilibrium and then estimating production functions from time-series observations on labor's share of the product [5], or deducing the form of such functions from intercountry observations on wage rates and man-hour inputs [1], the observations themselves

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* The authors are, respectively, acting assistant professor and professor of economics at Stanford University. Research for this project was supported jointly by the Stanford University Graduate School of Business and the Stanford Project for Quantitative Research in Economic Development. The authors are deeply indebted to Hajime Oniki for his careful work in numerical analysis of the data. They have benefited from informal discussions with Harry M. Markowitz and Alan J. Rowe. Gordon A. Harrison, Bulotti Machinery Company, provided advice on the estimation of machine-tool investment costs.

[1] The competitive equilibrium assumption does not square well with the marked differences in per cent return on investment in various industries reported in [1, p. 235].

could be utilized to check upon the consistency of the engineering production function with the competitive equilibrium assumption, and also with the assumption that a similar production function applies to all countries or all points in time. The present report contains only one comparison between our parameter estimates and actual observations on share-of-product. However, it is expected that further work will be conducted along these lines, utilizing both time-series and multicountry data for metal machining.

I. *The Markowitz-Rowe Framework*

M-R describe possibilities for direct substitution between individual machine tools. Thus, for the generation of small, semiprecision flat surfaces (no contours), they provide estimates of daily output per machine and worker, assuming these various degrees of automation: disc grinder, bench lathe, turret lathe, horizontal milling machine, and automatic chucking. Capital-labor substitution may indeed arise from more indirect sources than from the degree of automation of machine tools. For example, there may be important sources of such substitution in the queuing phenomena associated with semiautomatic machines [3]. There is also the possibility of alternative designs of the product, e.g., the use of aluminum instead of iron castings to reduce machining labor in the manufacture of automobile engine blocks. Whatever the importance of such indirect types of substitution, the only one considered here is that of the M-R type: direct substitution between machine tools.

The basic units of the M-R analysis are (a) machine tools, and (b) the "tasks" performed by machine tools.[2] Tasks are *not* stated in terms of an end product (e.g., a pump or a locomotive) assembled from piece parts, but rather in terms of the elementary operations needed in order to fabricate these piece parts. No estimates are provided by M-R for the number of task units required to produce various finished products. This is a serious gap that needs to be filled before it will be possible to make much direct use of what they have accomplished. However, if our statistical analysis of their data is correct, it should be possible to analyze aggregate capital-labor substitution possibilities without a detailed knowledge of the mix of various tasks needed to produce individual end products.

Appendix A contains an excerpt from the complete list of 115 general-purpose machine tools analyzed by M-R. Also in this appendix is an estimate of the capital investment required for the individual machine tools. Note that these investment figures relate to machine tools only, not to plant construction costs. According to the *Census of Manufactures (1958)* [*6*], construction costs in the United States have constituted a fairly small proportion of total new investment expenditures in metalworking ($198 out of $676 million in 1958 for S.I.C. industry 35, machinery except electrical). Hence, it is possible that no great error has been introduced into the following analysis through neglect of the plant construction costs associated with each machine-tool investment.

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[2] Note that the manufacture of fabricated metal products requires a large number of inputs other than machine tools, machinists, and primary metals. In addition to machine tools, the required capital investment typically includes buildings and office space, assembly lines, conveyors, metal-forming equipment, heat-treating equipment, etc. Similarly, machinists tend to be outnumbered by the balance of the blue- and white-collar work force.

M-R analyzed the capabilities of these 115 machine tools to perform a total of 129 alternative metalworking tasks.[3] Each task is defined in terms of four general attributes:

(R)—geometrical shape
(S)—size of piece
(T)—tolerance
(U)—size of lot[4]

Thus, task 2 is defined as follows:

$R1$—geometrical shape: flat surface—no contours
$S2$—size of piece: small
$T1$—tolerance: semiprecision
$U2$—size of lot: long-run

Since the M-R classification scheme allowed for 13 alternative geometrical shapes, five sizes of piece, three tolerances, and two lot sizes, they have, in effect, defined $13\cdot 5\cdot 3\cdot 2 = 390$ individual tasks. However, since productivity coefficients were provided for only 129 out of this total of 390, about two-thirds of the possible cells are empty. Table 1 defines each of the four attributes—shape, size, tolerance, and run length—and provides the reader with summary tabulations of the coverage of each of these attributes among the 129 tasks. Note the small proportion of tasks in certain cells: $R6$, $R8$, $R10$, $R11$, $S1$ and $S5$.

II. *Deletion of Inefficient Machines*

The original M-R data were intended to represent a set of feasible technological alternatives and included many possibilities that would be irrelevant except under short-term emergency conditions. For ex-

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[3] M-R actually presented data for 142 tasks, of which 13 referred to the machining of hardened materials. These 13 special tasks are neglected here. Our entire analysis refers to the machining of ordinary material, less than 375 Brinell hardness.

[4] Note that attribute (U) refers to the number of pieces produced in a single batch, not to the average rate of output over time. These are two distinct concepts, although closely related to each other.

TABLE 1—THE FOUR M-R ATTRIBUTES OF METAL MACHINING TASKS

Geometric Shape			Size of Piece		
Category	Ident. No. of Category	No. of Tasks Within Category	Category	Ident. No. Within Category	No. of Tasks Within Category
Flat surfaces— no contours	*R*1	15	Very small	*S*1	13
Flat surfaces—external contours	*R*2	15	Small	*S*2	47
Flat surfaces—internal contours	*R*3	15	Medium	*S*3	38
Cylindrical surfaces—external	*R*4	15	Large	*S*4	20
Cylindrical surfaces—internal	*R*5	16	Very large	*S*5	11
Drilled holes	*R*6	6			
Cylindrical forms—external	*R*7	8	Total		129
Standard screw threads	*R*8	2			
Standard gear shapes	*R*9	8			
Complex shapes	*R*10	6			
Irregular periphery—flat surface	*R*11	5			
Multiple surfaces	*R*12	10			
Multiple holes—drilled	*R*13	8			
Total		129			
Tolerance			**Size of Lot**		
Semiprecision	*T*1	58	Short	*U*1	88
Precision	*T*2	58	Long	*U*2	41
High Precision	*T*3	13			
			Total		129
Total		129			

ample, in the performance of task 2, M-R allowed altogether for 22 alternative machine tools. Figure 1 represents a plot of the M-R data for this one task: Q/L = daily output per worker and machine tool versus K/L = the capital investment per worker and machine tool (from our Appendix A). Much to the surprise of any economist, Figure 1 reveals no significant correlation between Q/L and K/L. This figure does not represent an isolated instance. Apparently, M-R utilized so much imagination in specifying machines to deal with short-run emergencies that their raw data are virtually useless for analyzing the long-run substitution possibilities between capital investment and direct labor.

Accordingly, we decided to delete many of the inefficient machines. The following censoring rule was applied separately for each task: If, in the performance of that task, one machine tool had a higher investment cost and not a higher output than a second machine tool, the first was said to be "inefficient" and was deleted from our analysis. Thus, in the case of task 2, Figure 1 shows that of the 22 initial machine-tool possibilities, only five (those denoted by crosses) were retained for purposes of the statistical analysis.[5] No attempt was made to impose convexity restrictions upon the efficient points.

The original M-R data include a total of 1,143 estimates of the productivity of various machines in performing the 129 individual tasks. After censoring the inefficient machines, the total drops to 290 such estimates. Hereafter we will refer to these 290 M-R estimates as statistical observations to be explained within the context of a least-squares regression analysis.

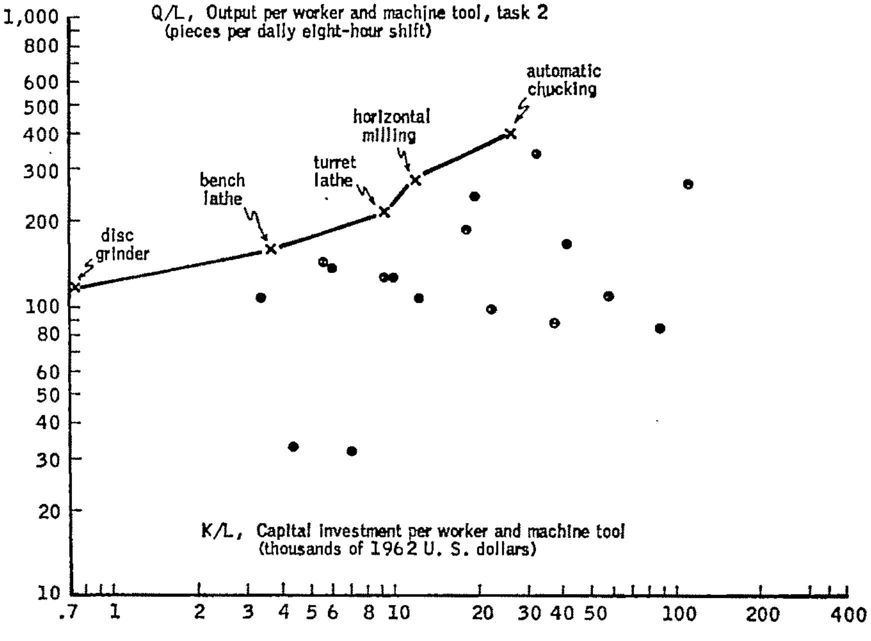

FIGURE 1. TASK 2. COMPARISON OF 22 ALTERNATIVE MACHINE TOOLS

It is to be noted that there are only six tasks with as many as five individual observations per task. Conversely, there are 41 tasks with a single observation apiece. These cell sizes immediately suggest the importance of analyzing *groups* of tasks rather than individual ones. A summary frequency distribution for the 129 tasks is shown in Table

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[5] A statistician would be entitled to object to this procedure. Given substantial errors in measurement, it is likely that our censored sample actually includes some inefficient machine tools and excludes some efficient ones. Hence, suggestions for a more refined censoring technique would be welcome. Whatever biases may be inherent in our crude procedure, this seems clearly preferable to no deletions at all.

TABLE 2

Frequency Distribution of Number of Observations Per Task

Number of efficient machine tools per task (i.e., observations)	Number of task	Total number of observations
1	41	41
2	43	86
3	23	69
4	16	64
5	6	30
Total	129	290

2, and an excerpt from the complete listing of 290 observations is in Appendix B. The job of statistical analysis is one of fitting plausible curves to these 290 observations.

Although the original M-R data were presented in the format of activity analysis—a series of discrete alternative machine tools for performing each task—we have found it more convenient to utilize a neoclassical framework. For statistical estimation purposes, we have analyzed their engineering data in the form of continuously differentiable production functions.

III. *Statistical Model 1: Cobb-Douglas Production Function*

The M-R framework emphasizes the task as the basic unit of analysis. One could insist that the production function for each task is different, and that 129 separate functions be estimated accordingly. This, however, could lead to a considerable waste of information. The basic idea underlying our statistical model is that, instead of the task, it is the *characteristics* of the task that determine the input-output relationship. In other words, the fact that we are making, say, smaller or larger pieces or performing an operation with wider or narrower tolerance enters directly as an independent variable within the production function. By looking at metal machining in this way, all of the M-R data are utilized simultaneously for the estimation of a single production function.

As an example, compare task 2 (characterized by $R1$, $S2$, $T1$, and $U2$) with task 7 (characterized by $R1$, $S3$, $T1$, and $U2$). These differ only in respect to attribute S, size of piece. The over-all production function predicts the difference in output rate of the two tasks (holding labor and capital inputs constant) on the basis of the difference in their engineering characteristics—size of piece. It is instructive to note that although M-R give information for only 129 tasks, there are 390 conceivable tasks (combinations of the four characteristics). This means that our production function will predict the productivity of labor and capital in performing tasks that do not appear in the M-R list. To state this formally, we introduce the following notation:

Q = output (pieces per daily eight-hour shift)
K = capital investment (thousands of 1962 U. S. dollars)
L = labor input (number of men per eight-hour shift)

$$r_i = \begin{cases} 1 \text{ if observation has the geometric characteristic } R_i \\ 0 \text{ otherwise} \end{cases} \quad i = 1, \cdots, 13$$

$$s_i = \begin{cases} 1 \text{ if observation has the piece-size characteristic } S_i \\ 0 \text{ otherwise} \end{cases} \quad i = 1, \cdots, 5$$

$$t_i = \begin{cases} 1 \text{ if observation has the tolerance characteristic } T_i \\ 0 \text{ otherwise} \end{cases} \quad i = 1, 2, 3$$

$$u_i = \begin{cases} 1 \text{ if observation has the lot-size characteristic } U_i \\ 0 \text{ otherwise} \end{cases} \quad i = 1, 2$$

ϵ = random disturbance (normally distributed, with zero mean).

Using this notation, we write the production function in Cobb-Douglas form, with the unknown parameters a, b, c_i, d_i, e_i, and f_i:

$$\text{(1)} \qquad \log Q = a + (b)\log K + (1 - b)\log L + \sum_{i=1}^{13} c_i r_i + \sum_{i=1}^{5} d_i s_i + \sum_{i=1}^{3} e_i t_i + \sum_{i=1}^{2} f_i u_i + \epsilon.$$

We shall refer to this as "Model 1."[6] The capital coefficient b and the labor coefficient (1-b) are assumed to be common for all observations, regardless of their engineering characteristics. Note that these characteristics are assumed to operate additively with one another, and with log K and log L, in the determination of log Q. Subsequently, Model 1 will be modified to allow the coefficient b to vary with the engineering characteristics.

IV. *Estimation of Model 1*

The least-squares regression analysis of all 290 observations in terms of Model 1 is summarized in the left-hand columns of Table 3.[7] The multiple correlation coefficient turns out to be highly significant

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[6] In (1), we are assuming the over-all error to be proportional to the output rate Q. It was mentioned earlier that in the censoring procedure an unquantified type of error was introduced in the sense that some observations appear in the sample which do not properly belong to the sets of efficient points. (The converse also holds.) The actual error term is compounded of this type of error, along with the conventional error of output measurement attributed to M-R.

[7] Since all of the M-R data are normalized in terms of output per worker, equation (1) is estimated with $L = 1$. Hence, the term (1-b) log L conveniently disappears.

($R^2 = .861$ and F ratio $= 83.587$). Not only does the equation give a good fit but the capital coefficient b is of a plausible order of magnitude, $+ .307$, and has a low standard error.

Given the definition of the dummy variables r_i, s_i, t_i, and u_i, in order to permit estimation, we have arbitrarily eliminated from equation (1) the dummy variables r_1, s_5, t_3, and u_1. In other words, the "standard intercept," $a = -.217$, refers to the specific task of producing geometric shape 1, piece size 5, tolerance 3, and lot size 1.

The regression coefficients for all dummy variables other than r_1, s_5, t_3, and u_1 are to be interpreted as corrections to the standard intercept. Thus, according to Table 3, the point estimate of d_1 is 5.284. This means that if capital investment K is held constant and the dummy variables r_i, t_i, and u_i are also held constant, the production of very small pieces ($s_1 = 1$) will lead to an increase of 5.284 in log Q. That is, the output Q for this task is to be multiplied by a factor of exp (5.284) in comparison with the standard case of producing very large pieces. Note that the estimated regression coefficients d_i, e_i, and f_i are in accord with a priori knowledge. Other things being constant, a higher output rate will be associated with the production of smaller pieces, less precise tolerances, and longer run-lengths.

V. *Considerations Leading to the Modification of Model 1*

Among the 290 observations utilized within this regression analysis, 41 relate to tasks produced by a single efficient machine. It is quite possible that these are actually 41 cases of fixed capital-labor proportions per unit of output. Alternatively, it may be that M-R did not exercise a sufficient amount of imagination in describing alternative machine tools in these cases. It may also be that our capital investment estimates were incorrect, and that some efficient processes were deleted accordingly. To test for the effect of these 41 single-machine observations, equation (1) was re-estimated without them. The results are presented on the right-hand side of Table 3.

With one major exception, there is little difference between the parameter estimates for the cases of 290 and 249 observations. This exception occurs in the case of the capital coefficient b, which increases from .307 to .475, and again has a low standard error.[8]

It was suggested earlier that Model 1 could be modified to allow the capital coefficient b to vary with the task characteristics. The increase in b that arises from deleting the 41 single-observation tasks may be related to an incorrect specification of Model 1. It is altogether possible that the hypothesis of a uniform capital coefficient is incorrect, and that instead this coefficient varies among tasks with different engineering characteristics. If this is true and the 41 tasks include a disproportionately large number with engineering characteristics that imply low capital coefficients, then our problem is solved.

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[8] Statistically significant increases also occur in connection with three of the geometric shape parameters: c_6, c_8, and c_{13}.

Turning now to the possible variations in the capital coefficient b, with the different engineering characteristics, we have constructed a modified multiple-regression model that allows for such variations. On the basis of a series of calculations yielding negative results, it is our conclusion that geometric shape is the only attribute that exercises a statistically significant effect upon the capital coefficient. Accordingly,

TABLE 3—ESTIMATION OF MODEL 1

		290 Observations		249 Observations	
	Coefficient	Estimate	Standard Error	Estimate	Standard Error
Standard Intercept	a	− .217		−1.133	
Capital Coefficient	b	.307*	.036	.475*	.036
Geometric Shape	c_2	.033	.129	.025	.118
	c_3	− .017	.138	.060	.138
	c_4	.057	.129	.134	.124
	c_5	.293**	.121	.345*	.117
	c_6	.147	.205	.362	.198
	c_7	−1.194*	.129	−1.206*	.115
	c_8	.556**	.256	1.059*	.230
	c_9	−2.270*	.174	−2.459*	.169
	c_{10}	−1.786*	.221	−1.486*	.361
	c_{11}	−1.153*	.202	−1.182*	.211
	c_{12}	−2.043*	.145	−1.873*	.130
	c_{13}	−1.062*	.204	.443	.245
Size of Piece: Very small	d_1	5.284*	.240	5.263*	.242
Size of Piece: Small	d_2	4.185*	.182	4.578*	.201
Size of Piece: Medium	d_3	3.023*	.173	3.348*	.193
Size of Piece: Large	d_4	1.334*	.172	1.351*	.211
Tolerance: Semiprecision	e_1	.731*	.141	.874*	.156
Tolerance: Precision	e_2	.415*	.141	.522*	.155
Size of Lot: Long-run	f_2	.240*	.071	.250*	.064
		$R^2 = .861$ F ratio = 83.587		$R^2 = .889$ F ratio = 91.431	

* Significantly different from 0 at the 1 per cent level.
** Significantly different from 0 at the 5 per cent level.

this is the only attribute for which numerical estimates are presented here.[9]

As a first step designed to serve as an intuitive guide, we divided the observations into 13 subgroups, those belonging to $R_1, R_2, \cdots, R_{13}$. In order to compensate for the lack of degrees of freedom, the estimated model was of the form:

$$\log Q = a_j + b_j \log K + \sum_i g_{ij} y_{ij} + \epsilon \qquad (j = 1, 2, \cdots, 13) \tag{2}$$

and

$$y_{ij} = \begin{cases} 1 & \text{if observation refers to task } i \text{ in geometric group } j \\ 0 & \text{otherwise} \end{cases}$$

In other words, within each geometric group there are tasks that differ with respect to size of piece, tolerance, and size of lot. By postulating an individual regression coefficient, g_{ij}, for each task i within group j, it was assumed that this coefficient would compensate for those differences attributable to factors other than geometric shape. (With such a hypothesis, it is clear that the 41 single-machine tasks contribute no useful information, and they were deleted accordingly.) This procedure requires the estimation of 101 parameters a_j, b_j, and g_{ij} instead of the 21 utilized for Model 1. Because of the difference in complexity of the two hypotheses, and also because of the large standard errors and low degrees of freedom encountered in the estimation of equation (2), Table 4 is to be taken with considerable skepticism. However, it does suggest the possibility that there are variations in the capital coefficient attributable to the factor of geometrical shape.

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[9] Upon hearing of our negative results with respect to engineering characteristics other than geometric shape, a number of colleagues have expressed their surprise. It is generally believed that there is a high positive correlation between the setup time for an individual lot and the capital investment for a machine tool. Hence, in the case of extremely small production lots, the machine with the lowest initial investment will be optimal, regardless of the relative price of capital and labor. According to this viewpoint, it follows that the smaller the lot size, the lower should be the capital coefficient b.

In reply, we would stress two points: (1) There is not a perfect positive correlation between the estimates of setup time and of capital investment per machine tool. (2) It is likely that even the short-lot sizes defined by M-R are still too large for the setup time element to be dominant. The M-R definition of "short" and "long" runs varies as follows with the size of piece:

	Number of pieces per lot	
Size of piece	Short run	Long run
Very small	500	10,000
Small	200	2,000
Medium	50	500
Large	10	100
Very large	3	—

TABLE 4—ESTIMATION OF INDIVIDUAL CAPITAL COEFFICIENTS FOR 13 GEOMETRIC GROUPS; MODEL 2

Geometric Group	Estimated b_j	Standard Error of b_j	Degrees of Freedom	R^2 (unadjusted)
$R1$	.456	.053	24	.969
$R2$	.939	.127	15	.953
$R3$	1.074	.230	8	.801
$R4$	.715	.223	14	.653
$R5$	.734	.088	22	.862
$R6$	.104	.068	3	.976
$R7$	.346	.052	24	.944
$R8$	.187	.033	3	.940
$R9$	.658	.141	4	.994
$R10$	.221	—	0	—
$R11$	.676	.088	3	.992
$R12$	.674	.067	25	.921
$R13$	.452	.079	3	.953

VI. *Estimation of Model 2*

In order to modify Model 1 to allow for capital coefficients that differ among geometric shapes and yet avoid the complexities of 13 individual capital coefficients, an aggregation scheme was tried. Except for the catch-all category $AR3$, there are technological considerations which justify the following aggregation of geometric shapes:

$AR1$—all flat surfaces; tasks in groups $R1$, $R2$, $R3$

$AR2$—all cylindrical surfaces and forms; tasks in groups $R4$, $R5$, $R7$

$AR3$—miscellaneous shapes; tasks in groups $R8$, $R9$, $R10$, $R11$, $R12$

$AR4$—all drilled holes; tasks in groups $R6$, $R13$.

With this aggregation of geometric groups, we proceeded to estimate the following model, allowing for four different capital coefficients b_i:

$$\log Q = a + \sum_{i=1}^{4} b_i \log K_i + \sum_{i=1}^{13} c_i r_i + \sum_{i=1}^{5} d_i s_i + \sum_{i=1}^{3} e_i t_i + \sum_{i=1}^{2} f_i u_i + \epsilon. \tag{3}$$

In equation (3) the variables r_i, s_i, t_i, u_i, and ϵ have the same meaning as in Model 1, and the new variables K_i are defined as follows:

$$K_i = \begin{cases} K, & \text{capital investment per machine tool, if observation belongs to aggregate geometric group } i \\ 1, & \text{otherwise; hence } \log K_i = 0 \end{cases}$$

TABLE 5—ESTIMATION OF MODEL 3

			290 Observations		249 Observations	
			Estimate	Standard Error	Estimate	Standard Error
Standard Intercept		a	-1.054		-1.407	
Capital Coefficients	$AR1$	b_1	.495*	.052	.522*	.057
	$AR2$	b_2	.443*	.052	.582*	.064
	$AR3$	b_3	.527*	.059	.504*	.062
	$AR4$	b_4	$-$ .169*	.061	.066	.099
Geometric Shape		c_2	$-$ .043	.114	.012	.115
		c_3	$-$ .096	.122	.031	.135
		c_4	.151	.197	$-$ .011	.222
		c_5	.362**	.196	.172	.228
		c_6	1.248*	.231	1.091*	.274
		c_7	-1.137*	.192	-1.389*	.231
		c_8	1.013*	.256	1.165*	.253
		c_9	-2.488*	.262	-2.444*	.279
		c_{10}	-1.874*	.281	-1.419*	.464
		c_{11}	-1.180*	.238	-1.178*	.271
		c_{12}	-2.015*	.213	-1.823*	.221
		c_{13}	$-$ .236	.216	.304	.255
Size of Piece	Very small	d_1	5.323*	.214	5.321*	.251
	Small	d_2	4.682*	.173	4.731*	.217
	Medium	d_3	3.414*	.161	3.460*	.208
	Large	d_4	1.455*	.152	1.382*	.204
Tolerance	Semi-precision	e_1	.720*	.123	.926*	.152
	Precision	e_2	.407*	.124	.590*	.152
Size of lot	Long-run	f_2	.224*	.062	.221*	.062
			$R^2=.895$ F Ratio$=98.457$		$R^2=.899$ F Ratio$=86.342$	

* Significantly different from 0 at the 1 per cent level.
** Significantly different from 0 at the 5 per cent level.

Table 5 presents the regression-analysis results for equation (3), both with and without the 41 single-observation tasks. Apparently the capital coefficients b_1, b_2, and b_3 do not differ significantly from .500, regardless of the inclusion or exclusion of single-observation tasks. It is to be noted that with Model 2 applied to all 290 observations, the parameter b_4 is estimated to be $-.169$, which is statistically significantly different from zero. On a priori economic grounds, we reject this negative coefficient. Further investigation reveals that a disproportionate number of the single-observation tasks are characterized by attribute

*AR*4, i.e., drilled holes.[10] This apparently explains the radical change in the capital coefficient of Model 1 from .307 to .475, depending on the inclusion or exclusion of the 41 tasks.

Before proceeding to an economic interpretation of the unusual behavior of geometric shapes 6 and 13 (drilled holes), we re-estimate Model 3, allowing for a different capital coefficient as between these two shapes. Estimates of the five capital coefficients are presented in Table 6. It is clear that this refinement in classification does not improve the over-all fit, but the table does suggest that the capital coefficient for group 6 is close to zero. The table also suggests that the capital coefficient for group 13 is highly sensitive to the inclusion or exclusion of the single-observation tasks. Since geometrical group 13 is "multiple drilled holes," one should not be surprised to find that such an operation offers some capital-labor substitution in comparison with group 6, "single drilled holes." Visual investigation of the four observations of group 13 among the 41 single-task observations showed that they are substantially out of line, and these four observations may be the source of the sensitivity observed in Table 6.

TABLE 6—RE-ESTIMATION OF MODEL 3; INDIVIDUAL CAPITAL COEFFICIENTS FOR GEOMETRIC GROUPS 6 AND 13

Capital Coefficients for the Respective Geometric Groups	290 Observations		249 Observations	
	Estimate	Standard Error	Estimate	Standard Error
*AR*1 b_1	.496	.052	.523	.056
*AR*2 b_2	.443	.052	.583	.063
*AR*3 b_3	.527	.059	.505	.061
*R*6 b_6	−.124	.080	−.096	.117
*R*13 b_{13}	−.221	.085	.452	.180
	$R^2 = .895$ F ratio = 94.298		$R^2 = .901$ F ratio = 85.038	

In order to verify this guess, Model 3 was re-estimated once more, allowing for a different capital coefficient as between geometric shapes 6 and 13, and also eliminating the four single-task observations of group 13. The results will not be reproduced here. They are virtually identical to Table 6, except that the capital coefficient for group 13 is now estimated to be .452, regardless of the inclusion or exclusion of the remaining 37 single-observation tasks.

It is our conclusion, therefore, that a Cobb-Douglas function with capital and labor exponents of .5 provides a good fit to the M-R data.

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[10] Altogether, there are only 41 single-observation tasks out of the total of 290 observations. Within *AR*4 (drilled holes), there are seven out of a total of 22 observations.

The chief exception to this conclusion would appear to be geometric shape 6, where we have been unable to find statistical evidence of capital-labor substitution.

VII. *Model 3: The C.E.S. Production Function*

As an alternative to the Cobb-Douglas production function, we have considered the constant-elasticity-of-substitution (C.E.S.) function [1]. Consider the model:

$$Q = A(\alpha K^{\beta} + (1 - \alpha)L^{\beta})^{1/\beta}, \tag{4}$$

where the parameter A depends upon the four characteristics of a task in the same way as in Model 1, namely:

$$\log A = a + \sum_{i=1}^{18} c_i r_i + \sum_{i=1}^{5} d_i s_i + \sum_{i=1}^{3} e_i t_i + \sum_{i=1}^{2} f_i u_i. \tag{5}$$

The major problem is to find an estimation procedure for the parameters of equation (4) without relying upon information on market conditions. In other words, there are engineering data directly available on Q, K, and L, and we wish to find estimates for the parameters of equation (4). We have utilized an iterative procedure for this purpose.

A. *A General Iterative Procedure*

Consider the general form:

$$Q = A[\alpha K^{\beta_1} + (1 - \alpha)L^{\beta_1}]^{1/\beta_2}. \tag{6}$$

Start with arbitrary initial values $\alpha^{(1)}$, $\beta_1{}^{(1)}$. Let

$$Z^{(1)} = [\alpha^{(1)}K^{\beta_1^{(1)}} + (1 - \alpha^{(1)})L^{\beta_1^{(1)}}].$$

We have then:

$$\log Q = \log A + \frac{1}{\beta_2} \log Z^{(1)}. \tag{7}$$

From least-squares estimates of equation (7), we obtain $A^{(1)}$ and $\beta_2{}^{(1)}$. Using these estimates, the quantity $Y^{(1)}$ can be computed:

$$Y^{(1)} = \left[\left(\frac{Q}{A^{(1)}}\right)^{\beta_2{}^{(1)}} - (1 - \alpha^{(1)})L^{\beta_1^{(1)}}\right] = \alpha K^{\beta_1}. \tag{8}$$

Then new estimates of $\alpha^{(2)}$ and $\beta_1{}^{(2)}$ can be obtained from the regression

$$\log Y^{(1)} = \log \alpha + \beta_1 \log K.$$

Using these new estimates, $(\alpha^{(2)}, \beta_1{}^{(2)})$, the process can be repeated. Note that in the last step we do not have to choose αK^{β_1} to remain on the right-hand side, and may choose $(1-\alpha)L^{\beta_1}$. Note also that we have not constrained equation (6) to be homogeneous of degree 1.

The convergence properties of this procedure were not investigated. We believe that convergence depends upon the "true" values of the parameters.

B. *Application to the M-R Data*

In using the above procedure we have constrained $\beta_1=\beta_2$ so that constant returns to scale holds. This is done by redefining

$$Z^{(1)} = [\alpha^{(1)}K^{\beta^{(1)}} + (1-\alpha^{(1)})L^{\beta^{(1)}}]^{1/\beta^{(1)}}$$

and ignoring the regression coefficient of log $Z^{(1)}$ in equation (7). In our case, the problem was simplified by the fact that $L\equiv 1$ for all observations. In the process of iteration we have regarded a successive difference of .01 of the parameters α and β to be sufficient for a discontinuation of the search.

Applying the procedure above, we obtained, after seven iterations, the estimates of the parameters. Surprisingly, the estimates of the coefficients a, c_i, d_i, e_i, and f_i were essentially the same as those presented in Table 3. Even more surprising was the fact that the estimate of β came out to be $\beta=-.011$ and $\alpha=.531$, and the multiple correlation coefficient $R=.928$. Note that since the elasticity of substitution σ is

$$\sigma = \frac{1}{1-\beta}, \qquad \text{we have } \hat{\sigma} = \frac{1}{1+.011} = .989.$$

The fact that $\hat{\sigma}$ did come out so close to 1, and that $\alpha=.53$ is close to our earlier estimate of the capital coefficient (.500), means that the original specification of the Cobb-Douglas production function was essentially a good one. This independent test for the usefulness of the Cobb-Douglas function encouraged us to go to our next step of the analysis, the implications with respect to relative shares.

VIII. *Relative Shares*

If the metal-machining industries were indeed characterized by a Cobb-Douglas production function with capital and labor coefficients of .5, and if this sector purchased its inputs and sold its products in competitive markets, the equilibrium shares of labor and capital should each be 50 per cent of the value of the product. It is therefore of some interest to check the engineering estimates of these coefficients against the relative shares actually recorded in this sector.

Unfortunately, the *U. S. Census of Manufactures* has not collected accounting data for individual departments (e.g., machine shops, die-

TABLE 7—RELATIVE SHARES IN S.I.C. 35, MACHINERY, EXCLUDING ELECTRICAL

	1954	1958
Department (1):		
W_1, payroll costs of nonproduction workers, plus estimated supplementary employee costs.[a]	\$ 2,386 million	\$ 2,867 million
Department (2):		
W_2, wages of production workers, plus estimated supplementary employee costs.[a]	5,373	5,014
V_2, residual share of capital; amount available for property insurance, depreciation, rental payments, interest, corporate income tax, and net earnings.	4,424	4,510
Total value added by manufacture, adjusted	\$12,183 million	\$12,391 million
Relative shares in Department (2):		
Labor's share $= \dfrac{W_2}{W_2+V_2}$	.548	.526
Capital's share $= \dfrac{V_2}{W_2+V_2}$	.452	.474

[a] Supplementary employee cost data (e.g., payroll taxes for legally required employee insurance plans) are available within the *Census* for 1957, but not for 1954 or 1958. In 1957, these items amounted to \$716 million, in comparison with total payrolls of \$9,050 million in S.I.C. 35. Accordingly, the direct payroll costs for 1954 and 1958 were adjusted upward by 7.9 per cent to allow for this component of labor cost.

Source: [6].

casting shops, heat-treating, etc.), but rather for *establishments*. Typically, a metalworking establishment will include several such production departments and also will employ a significant number of "nonproduction" workers (administrative, clerical, industrial engineers, etc.). For these reasons, the *Census* data on establishments cannot be compared directly with the M-R estimates of metal-machining productivity.

For lack of detailed departmental data it seemed desirable to compute relative shares only within S.I.C. industry 35 (machinery, excluding electrical). In this sector, 33 per cent of all production workers were employed within machine-shop departments during 1958. There was no other two-digit metalworking industry in which the 1958 machine-shop employment exceeded 15 per cent of the total number of production workers. Moreover, S.I.C. industry 35 employed 314,947 out of the total of 643,739 workers employed by machine shops in the major metalworking industries, S.I.C. 33–38.

The comparison with relative shares in S.I.C. 35 depends heavily upon one's assumptions with respect to the nonproduction work force.[11] The M-R data do not purport to deal with the white-collar phase of the metalworking business, and we have no direct knowledge of the possibilities for capital-labor substitution here. It is our conjecture that these employees work directly with only a small fraction of the total capital equipment in the sector.

To compare the recorded relative shares with the capital and labor exponents estimated from the M-R data, we have therefore decided to view S.I.C. 35 as though it consisted of two major departments: (1) nonproduction workers, utilizing no capital goods directly, and (2) production workers, utilizing all of this sector's capital equipment. The results of this calculation are shown in Table 7. The table suggests a close agreement between the accounting data gathered by the *Census* and the engineering estimates based upon the M-R data.

IX. *A Note on Automation*

The M-R data—whatever their other deficiencies—possess one virtue that is typically lacking in time-series or intercountry comparisons of labor and capital productivity: they relate to a single state of technological knowledge. Therefore, in referring to a higher- or lower-priced machine tool (different degrees of automation), we are still dealing with movements along a fixed production function, utilizing capital and labor as the basic inputs. If the Cobb-Douglas form provides a good fit to the M-R data on metal machining, then, say, a 10 per cent rise in the price of labor relative to capital will result in a 10 per cent drop in the employment of labor, provided that total output remains constant.

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[11] Wages paid out to production workers totaled $4,647 million, in comparison with the total payroll cost of $7,304 million for S.I.C. 35 in 1958. That is, nonproduction workers received 36 per cent of the total payroll.

6. U. S. DEPT. OF COMMERCE, *Census of Manufactures*, 1954 and 1958, Washington.
7. U. S. OFFICE OF PRICE STABILIZATION, *Base Prices of Machine Tools*. Washington, Feb. 1953.

APPENDIX A. MACHINE-TOOL INVESTMENT COSTS (Excerpt*)

The following estimates were drawn up under the general supervision of Gordon A. Harrison, Bulotti Machinery Company, in San Francisco. For many of the common machine-tool types, these were based directly on 1962 list prices. However, a majority were obtained from *Base Prices of Machine Tools*, Office of Price Stabilization, February 2, 1953, U. S. Government Office, Washington, D. C. In order to convert from 1953 to 1962 prices, a factor of 30 per cent was generally added on.

The figures are intended to represent 1962 list prices f.o.b. the U. S. manufacturer, including the local distributor's markup. These prices exclude shipping, installation, and construction costs for the machine-tool user. Since the Markowitz-Rowe estimates were drawn up in 1953, an effort was made to select 1962 equipment types that were comparable in productivity to those available in 1953.

Wherever a range of sizes was specified by Markowitz and Rowe, a representative machine tool was selected from the middle to upper end of this size range. All prices include electrical equipment, but exclude special-purpose attachments.

[The Appendix continues on the following page.]

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* The complete version of both Appendices A and B may be obtained upon request for Memorandum 23, Research Center in Economic Growth, 423, Encina Hall, Stanford University, Stanford, California.

M-R Machine Number	Description	Investment Costs (thousands of dollars)
	Boring machines—horizontal	
1	(a) under 3″ spindle—under 36″ bed	29.3
2	(b) 3–5″ spindle—36–120″ bed	41.9
3	(c) 6–10″ spindle—10′–23′ & over	97.5
	Boring machines—vertical	
4	(a) under 36″ swing	36.5
5	(b) 36–120″ swing	85.9
6	(c) 10′–25′ swing & over	141.2
	Precision boring machines	
7	(a) under 8″ bore—18″ table	9.5
8	(b) 8–14″ bore—18″ table	15.7
	Jig boring—horizontal & vertical	
9	(a) under 15″ table	28.0
10	(b) 15–40″ table	42.0
11	(c) 40–120″ table & over	100.0
	Broaching—horizontal & vertical—internal	
12	(a) under 15″ stroke—under 5 tons	6.6
13	(b) 18–36″ stroke—5–35 tons	25.0
14	(c) 36–120″ stroke—35–60 tons	44.6
	Broaching—external	
15	(a) under 15″ stroke—under 5 tons	6.6
16	(b) 18–36″ stroke—5–25 tons	25.0
17	(c) 36–120″ stroke—over 25 tons	44.6
	Sawing and cut-off machines	
18	(a) under 12″ work	4.3
19	(b) 12–30″ work	6.9
	Bandsaws	
20	(a) under 12″ work	1.0
21	(b) 12–30″ work	2.5
	Tapping & threading machines	
22	(a) under 12″ swing—2″ tap capacity, single spindle	5.6
23	(b) 12–50″ swing—2″ tap capacity, single spindle	5.6
24	(c) under 12″ swing—2″ tap capacity, multiple spindle	20.7
25	(d) 12–36″ swing—2″ tap capacity, multiple spindle	20.7
	Sensitive and upright drills-single spindle	
26	(a) under 12″ swing	0.2
27	(b) 12–32″ swing	0.9

APPENDIX B (Excerpt)

Obs. No.	Task No.	Mach. No.	Geom. Shape	Size of Piece	Toler-ance	Size of Lot	Log K, Investment Cost	Log Q, Pieces/ Day
1	1	63	*R*1	*S*2	*T*1	*U*1	−0.357	4.752
2	1	77	*R*1	*S*2	*T*1	*U*1	1.281	5.031
3	1	84	*R*1	*S*2	*T*1	*U*1	2.197	5.296
4	1	96	*R*1	*S*2	*T*1	*U*1	2.460	5.551
5	2	63	*R*1	*S*2	*T*1	*U*2	−0.357	4.775
6	2	77	*R*1	*S*2	*T*1	*U*2	1.281	5.072
7	2	84	*R*1	*S*2	*T*1	*U*2	2.197	5.365
8	2	96	*R*1	*S*2	*T*1	*U*2	2.460	5.649
9	2	87	*R*1	*S*2	*T*1	*U*2	3.258	6.023
10	3	77	*R*1	*S*2	*T*2	*U*1	1.281	4.442
11	3	59	*R*1	*S*2	*T*2	*U*1	1.705	4.992
12	4	77	*R*1	*S*2	*T*2	*U*2	1.281	4.471
13	4	59	*R*1	*S*2	*T*2	*U*2	1.705	5.094
14	4	87	*R*1	*S*2	*T*2	*U*2	3.258	5.675
15	5	59	*R*1	*S*2	*T*3	*U*1	1.705	5.229
16	6	64	*R*1	*S*3	*T*1	*U*1	1.194	3.428
17	6	78	*R*1	*S*3	*T*1	*U*1	1.705	4.097
18	6	85	*R*1	*S*3	*T*1	*U*1	2.862	4.380
19	6	97	*R*1	*S*3	*T*1	*U*1	2.939	4.644
20	7	64	*R*1	*S*3	*T*1	*U*2	1.194	3.462
21	7	78	*R*1	*S*3	*T*1	*U*2	1.705	4.167
22	7	85	*R*1	*S*3	*T*1	*U*2	2.862	4.514
23	7	97	*R*1	*S*3	*T*1	*U*2	2.939	4.846
24	7	88	*R*1	*S*3	*T*1	*U*2	3.440	4.934
25	8	78	*R*1	*S*3	*T*2	*U*1	1.705	3.865
26	8	60	*R*1	*S*3	*T*2	*U*1	2.708	4.346
27	9	78	*R*1	*S*3	*T*2	*U*2	1.705	3.944
28	9	60	*R*1	*S*3	*T*2	*U*2	2.708	4.457
29	9	88	*R*1	*S*3	*T*2	*U*2	3.440	4.738
30	10	60	*R*1	*S*3	*T*3	*U*1	2.708	4.048
31	11	95	*R*1	*S*4	*T*1	*U*1	3.892	2.766
32	11	107	*R*1	*S*4	*T*1	*U*1	4.997	3.483
33	12	95	*R*1	*S*4	*T*1	*U*2	3.892	2.889
34	12	107	*R*1	*S*4	*T*1	*U*2	4.997	4.273
35	13	61	*R*1	*S*4	*T*2	*U*1	3.401	3.437

AN ECONOMETRIC GROWTH MODEL OF THE UNITED STATES

By PAUL E. SMITH*

There has been increasing concern among academic economists and other interested parties during the past several years about the related problems of economic growth and what appears to be an undesirably high plateau of persistent unemployment in the economy. The rates of growth of capital formation and output in the United States have been compared unfavorably with those of both the Soviet Union and the resurgent economies of Western Europe. At the same time, the recent tendency of the unemployment level in the United States to remain above 5 per cent of the labor force even during comparative boom periods has been a matter of some consternation. Whether the first problem exists or can be simply attributed to the free market's reflection of a utility function which possesses a positive time preference for current output as opposed to the future production of goods and services, and whether the second problem can be blamed upon such disaggregated factors as technological changes, shifts in demand, and labor immobility are questions which it is hoped can be at least partially answered in this paper. A related question is whether these conditions can be expected to continue into the future, given the existing and past structure and behavior of the U. S. economy.

Consequently, this paper is largely analytical, although a few tentative conclusions with respect to policy considerations are implied in the statistical results and will be briefly discussed. Nonetheless, no attempt is made to include such policy parameters as the amount and direction of government expenditures, the tax structure, the monetary policies. Moreover, the aggregative nature of the following model prohibits any interregional or interindustry analysis of the afore-mentioned problems.

The paper is divided into four parts. The first part specifies a theoretical model of economic growth. The second is concerned with the actual statistical estimation of the parameters of the model. Part III extrapolates the model into the future via long-range forecasts by using the results of the previous section, and the fourth part summarizes and analyzes the conclusions.

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* The author, associate professor of economics at Michigan State University, gratefully acknowledges the assistance of Glen Downing with some of the computational work and unwillingly accepts the responsibility for any errors and omissions. In addition, the author wishes to acknowledge the helpful suggestions of Professors Maurice Dance and James C. LaForce of San Fernando Valley State College and the University of California at Los Angeles, respectively.

I. *The Model*

A. *The Variables of the Model*

The growth model presented here is a modification and extension of one suggested by Tinbergen and Bos [6, pp. 32–37]; the only major alterations are the addition of explanatory variables to some of the relationships and the elimination of an assumption with respect to a perfectly competitive labor market. The notation to be used is as follows:

Y = gross national product,
P = population,
N = labor supply,
L = level of employment,
U = level of unemployment,
K = stock of national wealth or capital,
I = net investment or the change in national wealth,
W = a subjectively determined "normal" wage rate, and
M_L = marginal productivity of labor.

The subscript t has the usual meaning, and all income and wealth variables are stated in "real" terms.

B. *The Relations of the Model*

As a convenient way of achieving an exponential rate of growth, we assume constant returns to scale and unitary elasticity of substitution between capital and labor and hence use the familiar Cobb-Douglas production function of a form given by

$$Y_t = a(1 + e)^t K_t^g L_t^h \tag{1}$$

where a is a constant, and e provides an estimate of the rate of increase in output, holding the inputs of capital and labor constant, and therefore reflects the effects of changes in working hours, technology, etc. Moreover, we note that $h = 1 - g$.

Disregarding Malthus, total population at time t is assumed to be exogenously determined, i.e.,

$$P_t = P_0(1 + r)^t \tag{2}$$

where P_0 is the population in the initial time period, and r is the rate of population growth.

The level of employment or labor supply is taken to be a constant proportion of population. Hence

$$\begin{aligned} N_t &= bP_t \\ &= bP_0(1+r)^t. \qquad (3) \end{aligned}$$

Likewise, the wage rate which is held to be "normal" or "expected" is determined exogenously by

$$W_t = W_0(1+i)^t \qquad (4)$$

where W_0 is the "normal" wage rate in the initial time period, and i is the rate of increase of the wage rate.

If perfect competition in hiring is assumed to exist among employers, the demand for labor is determined by equalizing labor's marginal productivity with the "normal" wage rate. Hence

$$M_{Lt} = \frac{\partial Y}{\partial L} = ha(1+e)^t\left(\frac{K_t}{L_t}\right)^a \qquad (5)$$

and

$$W_t = ha(1+e)^t\left(\frac{K_t}{L_t}\right)^a. \qquad (6)$$

Solving equation (6) for L_t, the demand for labor in the classical model is given by

$$L_t = \left(\frac{ha(1+e)^t K_t^a}{W_t}\right)^{1/a}. \qquad (7)$$

However, in this model we assume that discretionary government policy, built-in stabilizers, etc., may be directed toward the maintenance of full employment. Hence an alternative labor-demand equation is utilized and may be written

$$L_t = F(N_t, Z_t) \qquad (7a)$$

where Z_t is the right-hand member of equation (7).

The level of unemployment is given by the identity

$$U_t = N_t - L_t. \qquad (8)$$

The stock of national wealth or capital at the beginning of any time period is obtained by adding net investment during the previous period to the existing stock of wealth at the outset of that period, i.e.,

$$K_{t+1} = K_t + K_t. \qquad (9)$$

Finally, net investment is dependent upon the level of gross national product during the current period and the stock of national wealth at the beginning of the period. Therefore,

$$\dot{K}_t = I_t = k_0 + k_1 Y_t + k_2 K_t \tag{10}$$

where we would normally expect $0 \leq k_1 < 1$ and $k_2 < 0$.

C. *The Rate of Growth*

While a general solution to the model in terms of its parameters and the initial values of the variables is extremely complicated, some general statements about their effects upon the rate of growth can be made. Substituting equations (4) and (7) into equation (1), we obtain

$$Y_t = a(1+e)^t K_t^{(1+g)} \left(\frac{ha(1+e)^t}{W_0(1+i)^t}\right)^{1/g}. \tag{11}$$

Converting into logarithms,

$$\log Y_t = \log a + t \log(1+e) + (1+g)\log K_t + \frac{1}{g}\log h + \frac{1}{g}\log a + \frac{t}{g}\log(1+e) - \frac{1}{g}\log W_0 - \frac{t}{g}\log(1+i), \tag{12}$$

and differentiating with respect to time,

$$\frac{\dot{Y}_t}{Y_t} = \frac{1+g}{g}\log(1+e) + (1+g)\left(k_1\frac{Y_t}{K_t} + k_2 K_t\right) - \frac{1}{g}\log(1+i). \tag{13}$$

Moreover, the initial rate of growth is given by

$$\left(\frac{\dot{Y}}{Y}\right)_0 = (1+g)\left[\log(1+e)\frac{1}{g} + k_2 K_0 + k_1\frac{Y_0}{K_0}\right] - \frac{1}{g}\log(1+i). \tag{14}$$

In general, the rate of growth of GNP will be higher, the larger are g, e, k_1, k_2, and the initial level of output, and the smaller are the rate of increase of the expected wage rate and the beginning stock of wealth. If the procedure were repeated with equation (7a) instead of equation (7), it is clear that the rate of growth will be higher, the more vigorously fiscal and monetary authorities pursue a policy of full employment.

D. *The Rate of Unemployment*

The level of unemployment can be obtained by substituting equations (3), (6), (7), and (9) into equation (8), i.e.,

$$U_t = bP_0(1+r)^t - \left(\frac{ha(1+e)^t}{W_0(1+i)^t}\right)^{1/g} K_t. \tag{15}$$

Dividing by N_t, the rate of unemployment is

$$\frac{U_t}{N_t} = 1 - \left(\frac{ha(1+e)^t}{W_0(1+i)^t}\right)^{1/g} \frac{K_t}{bP_0(1+r)^t}. \tag{16}$$

Converting into logarithms and differentiating with respect to time as before, we get

$$\frac{\dot{U}_t}{N_t} = \frac{1}{g}\left[\log(1+i) - \log(1+e)\right] + \log(1+r) - \frac{\dot{K}_t}{K_t}. \tag{17}$$

Finally, the rate of change in the unemployment rate during the initial period is

$$\begin{aligned} \left(\frac{U}{N}\right)_0 = {} & \frac{1}{g}\left[\log(1+i) - \log(1+e)\right] \\ & + \log(1+r) - k_2K_0 - k_1\frac{Y_0}{K_0}. \end{aligned} \tag{18}$$

Inspection reveals that the rate of change, say an increase, in the unemployment rate will be greater, the larger are the rates of increase of the normal wage rate and population and the smaller are e and the two coefficients of the investment-demand function.

If the demand for labor takes the form of equation (7a), the above results must be modified by the obvious assertion that the rate of change in the unemployment ratio will be smaller, the larger is the positive relationship between the level of employment and the size of the labor force.

II. *Statistical Estimation*

In this section the parameters of equations (1) through (10) are estimated by fitting the equations to data from 1910 through 1959. The income and wealth variables are stated in 1954 prices, the price index for the 1910–28 period being estimated from a linear regression of a 1954 GNP deflator against a 1929 price index during the years for which both series are available and then predicting the 1954 base index for the remaining years from the regression equation and the 1929 base index. Hourly manufacturing wages are taken as an approximation of the normal wage rate. The data are shown in Table 1.

A. *The Parameters*

The stochastic version of equation (1) is

TABLE 1—THE DATA OF THE MODEL[a]

Year	Gross National Product	National Wealth	Population	Labor Force	Employment	Normal Wage Rate
1910	$115.2	$367.9	92.4	37.9	35.7	$0.53
1911	114.5	379.1	93.9	38.6	36.3	0.55
1912	115.1	390.4	95.3	39.3	37.3	0.56
1913	119.2	401.8	97.2	39.6	37.9	0.58
1914	116.4	413.5	99.1	40.6	37.5	0.57
1915	122.5	427.9	100.5	41.5	37.7	0.56
1916	126.4	438.7	102.0	42.0	40.1	0.57
1917	128.0	477.8	103.4	43.4	41.5	0.57
1918	129.2	492.2	104.6	44.6	44.0	0.65
1919	132.0	497.3	105.1	43.3	42.3	0.80
1920	134.2	499.5	106.5	43.2	41.5	0.81
1921	125.0	508.2	108.5	44.5	39.4	0.87
1922	131.0	516.3	110.1	44.6	41.4	0.86
1923	150.0	534.3	112.0	45.3	43.9	0.91
1924	153.4	550.3	114.1	45.7	43.3	0.94
1925	157.1	571.1	115.8	46.3	44.5	0.94
1926	168.2	594.3	117.4	46.7	45.8	0.94
1927	168.7	615.1	119.0	47.8	45.9	0.96
1928	171.1	637.3	120.5	48.5	46.4	0.98
1929	181.8	657.0	121.8	49.2	47.6	0.99
1930	164.5	668.6	123.1	49.8	45.5	1.00
1931	153.0	673.0	124.0	50.4	42.4	1.03
1932	130.1	661.6	124.8	51.0	38.9	0.99
1933	126.6	639.9	125.8	51.6	38.8	1.00
1934	138.5	629.5	126.4	52.2	40.9	1.13
1935	152.9	609.5	127.2	52.9	42.3	1.16
1936	173.3	627.5	128.1	53.4	44.4	1.17
1937	183.5	629.3	128.8	54.0	46.3	1.26
1938	175.1	634.1	129.8	54.6	44.2	1.29
1939	189.3	637.0	130.9	55.2	45.8	1.32
1940	205.8	646.3	132.1	55.6	47.5	1.35
1941	238.1	673.3	133.4	55.9	50.4	1.38
1942	266.9	688.3	134.9	56.4	53.8	1.43
1943	296.7	693.4	136.7	55.5	54.5	1.48
1944	317.9	680.8	138.4	54.6	54.0	1.53
1945	314.0	671.9	139.9	53.9	52.8	1.50
1946	282.5	677.5	141.4	57.5	55.2	1.46
1947	282.3	689.0	144.1	60.2	58.0	1.49
1948	293.1	713.0	146.6	61.4	59.4	1.53
1949	292.7	752.1	149.2	62.1	58.7	1.59
1950	318.1	771.8	151.7	63.1	60.0	1.64
1951	341.8	810.8	154.4	62.9	61.0	1.65
1952	353.5	837.8	157.0	63.0	61.3	1.07
1953	369.0	865.8	159.6	63.8	62.2	1.79
1954	363.1	885.3	162.4	64.5	61.2	1.81
1955	392.7	901.0	165.3	65.8	63.2	1.86
1956	400.9	941.4	168.2	67.5	65.0	1.89
1957	408.6	976.7	171.2	67.9	65.0	1.91
1958	401.3	1,006.2	174.1	68.6	64.0	1.92
1959	428.6	1,024.5	177.3	69.4	65.6	1.97

[a] GNP and the stock of national wealth are both stated in billions of dollars, and the population variables are in millions of people.

Sources: Historical Statistics of the United States, Statistical Abstract of the United States, The Survey of Current Business, and J. W. Kendrick [2, pp. 320–22].

$$Y_t = a(1+e)^t K_t^g L_t^h e^{u_t} \qquad (t = 1, 2, \cdots, T) \tag{19}$$

where the error term u_t is assumed to be normally distributed with mean zero and finite variance, independent of K and L, and serially uncorrelated. The estimators of g and h are obtained from

$$\hat{g} = \left[\prod_{t=1}^{T} p_t\right]^{1/T} \tag{20}$$

and

$$\hat{h} = \left[\prod_{t=1}^{T} q_t\right]^{1/T} \tag{21}$$

where p_t and q_t are the factor shares accruing to capital and labor, respectively.[1] Hence $\hat{g}$ and $\hat{h}$ are merely the geometric means of the p_t and q_t and are estimated from

$$\log \hat{g} = \frac{1}{T}\sum_{t=1}^{T} \log p_t \tag{22}$$

and

$$\log \hat{h} = \frac{1}{T}\sum_{t=1}^{T} \log q_t. \tag{23}$$

The constant term is estimated by fitting the results to the initial time period, and e is determined from the resulting unexplained residuals.[2] The final result is

$$Y_t = 1.943 \underset{(0.002)}{(1.0136)^t} K_t^{.2177} L_t^{.7823} \qquad R^2 = 0.94$$

The population equation is

$$P_t = P_0 \underset{(0.0004)}{(1.0134)^t}\,{}^{3} \qquad R^2 = 0.98$$

and the size of the labor force is given by

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[1] The distributive shares data are from [4, p. 919] and are estimated on an economy-wide basis.

[2] For a discussion of this method, see [3, pp. 191–94]. An alternative approach which is more likely to yield an unbiased estimator is given in [1].

[3] P_0 (and w_0 below) was arbitrarily selected as a constant term, and the term in the parenthesis was estimated via least squares according to

$$P_t - P_0 = (1+r)^t$$

or

$$\log (P_t - P_0) = r't.$$

The constant term in the logarithmic regression was not found to differ significantly from zero.

TABLE 2—THE LABOR-DEMAND VARIABLE

Year	Z	Year	Z
1910	46,510	1935	8,962
1911	42,820	1936	9,399
1912	43,020	1937	6,994
1913	39,960	1938	6,809
1914	47,170	1939	6,522
1915	56,090	1940	6,324
1916	56,170	1941	6,314
1917	64,820	1942	5,804
1918	38,720	1943	5,291
1919	15,970	1944	4,725
1920	16,050	1945	5,412
1921	12,490	1946	6,372
1922	14,150	1947	6,424
1923	11,970	1948	6,237
1924	11,260	1949	5,843
1925	12,370	1950	5,511
1926	13,650	1951	5,965
1927	13,580	1952	5,694
1928	13,560	1953	4,919
1929	14,150	1954	5,064
1930	14,560	1955	4,818
1931	13,360	1956	4,957
1932	17,750	1957	5,192
1933	16,570	1958	5,532
1934	9,854	1959	5,304

$$\begin{aligned} N_t &= \underset{(0.008)}{0.3956} P_t \qquad R^2 = 0.98 \\ &= 0.3956 P_0 (1.0134)^t. \end{aligned}$$

The normal or expected wage rate is

$$W_t = W_0 \underset{(0.0078)}{(1.0271)^t} \qquad R^2 = 0.96.$$

The variable on the righthand side of equation (7) is

$$Z_t = \left(\frac{1.5200(1.0136)^t K_t^{.2177}}{W_t} \right)^{1/.2177}.$$

The variables' values were calculated for the sample period and are shown in Table 2. The time trend suggests that the normal wage rate has been increasing more rapidly than labor's marginal value product, hence exerting a depressing influence upon the demand for labor. Evidence is lacking as to whether this finding suggests that labor is now being paid a wage greater than its marginal product or whether labor was being exploited during the early portion of the sample period.[4]

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[4] These results also suggest the existence of a money illusion on the part of workers. It should be noted that the periods of rapid increase in real wages frequently coincide with periods of

TABLE 3—LONG-RUN PROJECTIONS

Year	Gross National Product	National Wealth	Population	Labor Force	Normal Wage Rate	Employment
1960	$443.3	$1,055.0	179.8	71.1	$2.02	66.5
1961	457.0	1,086.3	182.2	72.1	2.07	67.5
1962	470.1	1,118.0	184.6	73.0	2.13	68.3
1963	483.9	1,150.1	187.1	74.0	2.19	69.3
1964	499.2	1,182.6	189.6	75.0	2.25	70.3
1965	513.7	1,215.8	192.1	76.0	2.31	71.2
1966	529.1	1,249.5	194.7	77.0	2.37	72.2
1967	544.9	1,283.8	197.3	78.1	2.43	73.2
1968	560.9	1,318.7	200.0	79.1	2.50	74.2
1969	576.7	1,354.3	202.6	80.1	2.57	75.1
1970	594.7	1,390.5	205.4	81.3	2.64	76.3
1971	611.3	1,427.6	208.1	82.3	2.71	77.2
1972	628.9	1,465.3	210.9	83.4	2.78	78.2
1973	647.5	1,503.8	213.7	84.5	2.86	79.3
1974	666.6	1,543.2	216.6	85.7	2.93	80.4
1975	685.5	1,583.5	219.5	86.8	3.01	81.4
1976	706.1	1,624.7	222.4	88.0	3.10	82.6
1977	726.5	1,667.0	225.4	89.2	3.18	83.7
1978	747.5	1,710.3	228.4	90.4	3.27	84.8
1979	768.9	1,754.6	231.5	91.6	3.35	85.9
1980	790.8	1,799.9	234.6	92.8	3.45	87.0

The level of employment is determined from the linear-regression equation

$$L_t = 4.45 + \underset{(0.120)}{0.8994}N_t - \underset{(0.000207)}{0.000364}Z_t \qquad R^2 = 0.85$$

Finally, the level of net investment is given by

$$K = I_t = 18.927 + \underset{(0.026)}{0.1863}Y_t - \underset{(0.016)}{0.0666}K_t \qquad R^2 = 0.60$$

III. *Forecasting*

The data presented in Table 1 and the parameters estimated in Part II provide sufficient information for predicting the values of the variables into the future, provided it is assumed that none of the structural relationships changes during the forecast period, and that the fact that the values of the independent variables in the two linear equations

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declining farm prices. Hence, in determining a real wage rate for use in the demand for labor, it might be more appropriate to deflate with a price index of manufactured items.

TABLE 4—THE MODEL AS A FORECASTER

Gross National Product
(in 1954 prices and billions of dollars)

Year	Actual	Naive	Model	K.G.S.A.	Model
1960	$440.2	$428.6 (11.6)	$443.3 (−3.1)	$432.0	(8.0)
1961	447.9	440.2 (7.7)	457.0 (−9.1)	450.1	(−2.2)
1962	469.9[a]	447.9 (22.0)	470.1 (−0.2)	474.3	(−4.4)

[a] Preliminary, based on the assumption that GNP for the fourth quarter of 1962 will be the same as the average for the first three quarters.

Sources: [5] [7].

fall outside the sample range does not lead to large forecast errors.[5] The results of the extrapolations are shown in Table 3 for the period through 1980.

While the model's solution does not contain an oscillation and hence cannot forecast downturns, it does predict reasonably well for the first three years as shown in Table 4. The forecasts are compared with those obtained from a naive model in which current GNP is assumed to be the same as that of the previous year and with the familiar Klein-Goldberger-Suits-Anderson model of the Research Seminar in Quantitative Economics at the University of Michigan. In spite of the fact that our model's 1961 and 1962 forecasts are based on the lagged forecasts for the preceding year rather than actual values, it predicts GNP for the three years much better than does the naive model and at least as well as does the K.G.S.A. model. The 1963 forecasts for our model and the K.G.S.A. model are $483.9 billion and $484.5 billion, respectively; and if the preliminary GNP estimate for 1962 of $469.9 billion is used, the K.G.S.A. forecast is $484.0 billion.

In answer to the question as to whether the rate of growth of the economy has declined and can be expected to continue declining, percentage changes in GNP are shown for selected time periods in Table 5. While the projected growth rate of about 2½ per cent per year does not compare favorably with the rapid rates of increase in output during the 1920s and the 1950s, it is fairly typical of the period as a whole, as would be expected from the estimation techniques. Finally, the projected rates of unemployment average slightly above 6 per cent or somewhat higher than the postwar period to date, suggesting that the recent concern about hard-core unemployment may have some justification.

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[5] Also, the cumulative effects of the random disturbances can be expected to reduce the accuracy of the long-run forecasts. A possible adjustment would be to impose random shocks having the same characteristics as the error terms during the sample period upon the relations during the forecast period. While this procedure might make the forecasts appear a little more "realistic," there is no a priori reason to anticipate any improvement in their accuracy.

IV. *Summary and Conclusions*

It is a courageous undertaking to attempt to approximate the growth structure of a modern industrial economy by an extremely oversimplified macroeconomic model. In particular, the exclusion of explicit policy variables, e.g., government expenditures, taxes, and the money supply, even in such a simple model as the one presented here, makes it very difficult to evaluate the effects of alternative discretionary policies upon long-run economic development. Indeed, it is difficult to visualize just how such a model might be specified. Yet the model suggests, as would be intuitively obvious, that the rate of growth will be larger, the more vigorously fiscal and monetary authorities pursue a program of full employment.

TABLE 5—PERCENTAGE INCREASES IN REAL GROSS NATIONAL PRODUCT FOR SELECTED PERIODS, 1910–1980[a]

Time Period	Percentage Change in G.N.P.	Time Period	Percentage Change in G.N.P.
1910–1919	14.6%	1945–1949	−6.8%
1920–1929	35.5	1950–1954	14.1
1930–1939	13.1	1955–1959	9.1
1940–1949	42.2	1960–1964	12.6
1950–1959	34.7	1965–1969	12.3
1960–1969	30.1	1970–1974	12.1
1970–1979	29.3	1975–1979	12.2

[a] Predicted increases for 1960–1979.

Several additional features would be useful, even in an aggregated model. These include the specification of a production function that would account for more limited substitution between factors, technological progress, and more disaggregation of the inputs. Other considerations to be taken into account are the inclusion of gestation lags (possibly distributed), an open economy, depreciation and replacement, the natural resource base, and the disaggregation of gross output, the stock of wealth, and the labor force into their various components. Most of these modifications seem to be technically feasible, albeit ambitious, undertakings at this time.[6]

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[6] A further consideration which might be taken into account would be the estimation of an aggregate utility function that would seek to explain the set of preferences underlying either the realized or desired rates of growth. Although such an approach trips over the familiar problems of interpersonal comparisons in the case of cardinal utility and deriving social welfare orderings in the case of ordinal utility, at least an implicit, and preferably explicit, recognition of the function's existence is necessary for any development planning. The function would presumably take the form $U_t = F(Y_t, r)$ for $T = t, t+1, \ldots, t+n$, where Y_T is the future stream of output, and r is an appropriate discount factor. Hence an adequate development program would maximize U_t, subject to various technological and resource constraints.

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LABOR MOBILITY, RESOURCE ALLOCATION, AND STRUCTURAL UNEMPLOYMENT

By LOWELL E. GALLAWAY*

There is undoubtedly no economic mechanism that is more maligned as a resource-allocation device than the labor market in the United States. The wealth of institutional material describing various restrictive practices in combination with apparent evidence of lack of labor mobility between markets has generated the impression that the United States has a myriad of relatively independent labor markets whose relationship to one another (particularly as allocators of resources) is somewhat remote. (For example, see [6, p. 95].) This attitude towards the role of the labor market in the economic complex can be dated from J. E. Cairnes' [1] doctrine of "non-competing groups" with its suggestion that in reality there are numerous markets for labor rather than a single general market.

The possibility that the labor market in the United States is failing to perform its role as an allocator of resources has significant policy implications. This is particularly true at a time when there is concern about the secular drift upward in the unemployment rate in the U.S. economy, for one possible explanation of such a drift is the failure of the allocative mechanism (i.e., the labor market) to shift resources efficiently from sector to sector.[1] This failure could take one of two forms: (1) a failure of the labor market to adjust the general price of labor relative to other factor prices so as to clear the market of labor (i.e., a failure of the interfactor allocative process); or (2) an inability on the part of the labor market to allocate resources on an intrafactor basis (i.e., to shift labor from one sector of the labor market to another). The pronounced difference in the policy implications of the two types of failures in the allocative mechanism makes a distinction between them quite proper. A failure of the allocative mechanism at the interfactor level is much more susceptible to a policy solution embracing the use of general controls of a monetary fiscal nature than is a failure of that same mechanism at the intrafactor level.[2] Consequently, some knowledge of the extent to which the labor market either succeeds or fails as an allocative device at these levels is significant from a policy standpoint. In this paper an attempt will be made to provide some insight into this question through an empirical evaluation of the effectiveness of the labor market as an intrafactor allocative mechanism.

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* This paper was written while the author was visiting associate professor of economics at the University of Minnesota on leave from San Fernando Valley State College. He is presently Chief, Analytic Studies Section, Social Security Administration. Selig Lesnoy and O. H. Brownlee deserve thanks for their helpful comments. The author assumes full responsibility for the content of the paper; the interpretations and conclusions are his and do not necessarily reflect the views of the Social Security Administration or the Department of Health, Education, and Welfare.

[1] For a description of this problem, see [11].

I. *The Theory of the Labor-Market Mechanism*

As background for the empirical examination to follow let us review briefly the theory of the operation of the labor market as an intrafactor allocative mechanism. Assume initially a fully employed two-sector economy in which the following conditions exist: (1) there is perfect mobility of workers between sectors; (2) there are no economic costs, either private or social, associated with this mobility; (3) the units of labor are homogeneous; (4) workers attempt to maximize the utility function which depicts their leisure-income preferences; (5) there are no differences in workers' preference functions in the two sectors; and (6) there are no non-wage elements of money income associated with employment.

In order to clarify these assumptions, note that perfect mobility is defined as an absence of noneconomic barriers (i.e., barriers which do not reflect themselves in the form of opportunity costs) to the free flow of labor between markets. Consequently, the first two assumptions amount to an absence of both noneconomic and economic barriers to the transfer of labor between the sectors. In addition, assume that traditional labor supply and demand relationships exist in both sectors. Now, if a wage differential should exist between the two sectors, maximizing behavior by workers implies an increase in the quantity of labor employed in the high-wage sector and a decrease in the low-wage sector until an equilibrium is reached at which the wage rates are equated. Thus, the logical implication of these conditions is an equalization of wage rates in the respective sectors.[3]

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[2] The distinction between an allocative failure at the interfactor or intrafactor level is crucial for postulating the nature of the Phillips curve [7] which is appropriate for policy considerations.

[3] This model makes no specification as to whether the production functions in the two sectors are similar or dissimilar or as to whether capital is mobile between the two sectors. Such assumptions are not crucial for the analysis. If all the possible combinations of assumptions regarding the similarity of production functions and the mobility of capital and labor are considered, it is apparent that mobility of labor is a sufficient condition for an equalization of wage rates and an elimination of involuntary unemployment in the sectors, while immobility of labor is a necessary condition for a nonequalization of wage rates and/or existence of involuntary unemployment. There is one situation (similar production functions and mobile capital) in which immobility of labor may be accompanied by equalization of wage rates and no involuntary unemployment.

It is quite obvious that in reality the labor market fails to correspond to this theoretical construct. After all, the literature is replete with discussions of the existence of persistent wage differentials in the economy, e.g., regional differentials, interindustry differentials, and occupational differentials. Therefore, the flows of labor between the sectors are not producing an equalization of wage rates throughout the system. This may occur for several reasons: (1) the existence of noneconomic barriers to mobility of workers; (2) the existence of positive private economic costs associated with the movement of labor from sector to sector; (3) nonhomogeneity of the labor units involved; (4) a failure of workers to maximize their utility function; and/or (5) differences in workers' preference functions.

The impact of these phenomena on the labor-market model previously discussed are easily analyzed. Assume an initial position of equilibrium (i.e., equalized wage rates) in the two sectors. Now, introduce an exogenous leftward shift of the labor-demand curve in one sector. This shift will produce one (or both) of two effects: a downward movement in the wage rate and/or involuntary unemployment in the affected sector.

Under the conditions initially postulated, the free flow of labor between the sectors would eliminate any wage differential that had developed and any involuntary unemployment. However, if we introduce the presence of a restrictive noneconomic barrier, such as a powerful trade union which prohibits entry into a high-wage sector, either a permanent wage differential or involuntary unemployment (or a measure of both) will develop.

On the other hand, if the barriers to worker mobility are not completely exclusive, two possible results may occur. If the barriers to mobility are economic in origin (i.e., generated by the private opportunity costs associated with a labor-market transfer), an equilibrium may be reached which reflects the objective opportunity costs implicit in these barriers. For example, the cost involved in moving from one geographic area to another or the cost involved in acquiring the skills necessary to enable a worker to move from one sector to another may justify the existence of a wage differential even though workers otherwise move with complete freedom from sector to sector. Under these circumstances no involuntary unemployment would exist. A second possibility, though, is that a wage differential may be generated which is not based on the objective opportunity costs associated with labor mobility. This could result if there were noneconomic barriers to mobility (such as partial restriction). Such a situation might or might not be accompanied by involuntary unemployment.

The other possible sources of difficulty in the adjustment process produce results which are simply variations of those associated with the imperfections already discussed. Nonhomogeneity in the labor force may be either of a type that completely excludes a worker from certain labor markets (such as a worker of below-normal intelligence being excluded from becoming a member of the medical profession) or of a type that can be overcome at some positive private opportunity cost (such as the cost of education). Consequently it may be subsumed under either the economic or noneconomic heading depending upon the circumstances. The fourth possibility, nonmaximizing behavior by workers, implies inconsistent actions on their part (given the subjective character of the utility function which describes each worker's leisure-income preferences). Such inconsistent actions would tend to generate either wage differentials or involuntary unemployment in the affected sector, although the magnitude of the effects is somewhat unpredictable. And finally, differences in workers' preference functions will produce results identical to those obtained in the case of the existence of objective opportunity costs. If these subjective differences have any influence, they will be quantified in the same fashion as the objective opportunity costs and consequently have the same impact.

The results of the various imperfections which have been described can be grouped into two distinct patterns. First, there are those situations in which there are only economic barriers to mobility, situations which result in differentials based on the private costs (both objective and subjective) necessary to overcome these barriers and an absence of involuntary unemployment. Secondly, there are instances in which noneconomic barriers or nonmaximizing behavior occur. In these cases wage differentials not based on opportunity casts exist, and they may or may not be accompanied by involuntary unemployment.

Classifying the possible results of imperfections in this fashion is extremely useful. In effect, it treats imperfections as being one of two types: either they result from behavior which is or behavior which is not the product of the individual worker evaluating the opportunity costs that he must meet in effecting a labor-market transfer. The significance of such a dichotomization lies in the implications of the different types of imperfections for the process of resource allocation. Quite obviously, situations in which the worker fails to move from one sector to another due to the opportunity costs attendant on such a move are analogous to the introduction of transportation costs into an international-trade model. While the existence of such costs alters the pattern of international trade, it by no means indicates that international market mechanisms are not functioning effectively as resource allocators. Quite the contrary, the test of a market mechanism's efficiency in allocating resources is its ability to take into consideration all relevant opportunity costs. Therefore, in the case of labor markets the opportunity-cost-induced imperfections cannot be said to result in a misallocation of labor among the various sectors. On the other hand, the operation of imperfections which do not reflect opportunity-cost considerations by individual workers can be said to involve some threat to the efficiency of the labor market as a resource-allocation device.

II. *A Framework for Testing*

So much for the theory of the labor-market mechanism. The basic problem that remains is one of converting these theoretical conclusions into a form which will permit them to be tested empirically. Essentially, this is a problem of distinguishing between the "economic" and "noneconomic" imperfections in the labor market. From our analysis we know that both are accompanied by wage differentials and that there will be no involuntary unemployment accompanying the economic imperfections, although there may be some such unemployment with noneconomic imperfections. By itself this is insufficient information to enable us to determine clearly which types of barriers to labor mobility exist in the U.S. economy. However, by considering the operation of the labor market over a longer time period and introducing the concept of "frictional" unemployment, progress can be made along these lines.

Consider an economy which is composed of n sectors and which is subject to a substantial degree of dynamic economic change. Now, through time each of the n sectors will be the recipient of a series of exogenous shocks which will necessitate labor-market readjustments. If the pattern of exogenous shocks differs from sector to sector (a not unreasonable assumption), different adjustment patterns will develop depending upon the type of barriers to labor mobility which confront the market mechanism. Assuming (1) instantaneous adjustment in the labor market, (2) money illusion on the part of workers, and (3) no substantial shifts in the opportunity costs associated with mobility, economic barriers to adjustment should produce a history of relatively constant money-wage differentials between the sectors.[4] This, in turn, implies that sectoral wage rates will show a high positive correlation through time. In addition, the adjustment pattern should include a history of no involuntary unemployment. Obviously, this last condition must be interpreted in the light of two considerations: (1) the existence of a substantial amount of frictional unemployment in the system, and (2) the possibility of periodic deviations from the full-employment assumption that is in force. By frictional unemployment we mean a certain irreducible minimum of unemployment that is the result of such factors as workers in transit. Introducing this consideration into the discussion modifies the statement relating to the existence of involuntary unemployment. Rather than reading "no involuntary unemployment," it should read "involuntary unemployment not in excess of the level of frictional unemployment appropriate to the sector." Thus, even under situations in which workers face only economic barriers to labor mobility, we would expect to find a certain amount of involuntary unemployment.

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[4] Obviously the assumptions that have been made are somewhat heroic. However, they will suffice to provide a first approximation of the necessary conditions for concluding that intrafactor resource allocation is efficient. At that point their implications for the testing process will be examined.

Relaxing the full-employment assumption modifies the expected pattern of involuntary unemployment with purely economic barriers still further. We would now expect to find a cyclical component superimposed on the frictional unemployment rate. Given a certain rate of unemployment on the national level that is in excess of the amount of frictional unemployment that occurs in the system, a highly efficient labor-market mechanism would allocate it evenly throughout the various sectors. Thus, all sectors would reflect the cyclical component of national unemployment to the same degree.[5] Consequently, if the labor-market mechanism is an efficient intrafactor allocative device, sectoral unemployment should show a high positive correlation. In summary, as a first approximation two necessary conditions for efficient intrafactor resource allocation by the labor market are a high degree of positive correlation between sectoral wage rates and a high degree of positive correlation between sectoral unemployment rates.

At this point some consideration of the implications of the assumptions with respect to instantaneous adjustment and money illusion is in order. Obviously the impact of the instantaneous-adjustment assumption is to eliminate the effect of lags in the adjustment process. For example, given a positive shift in demand for a particular product, a higher wage would be expected in the market for labor employed in the production of that product. An instantaneous labor-market adjustment would eliminate the resultant change in wage differential immediately and maintain the pattern of constant differentials. However, if we introduce the presence of lags in the adjustment process, the historical pattern of differentials will be altered by the impact of exogenous shifts in demand. Therefore, the statement that an efficient labor-market mechanism will produce a history of constant wage differentials exaggerates in the direction of expecting too much of the labor-market mechanism. Of course, similar reasoning with respect to a negative shift in demand leads to the same conclusion regarding negative wage changes and increases in unemployment. Consequently the necessary conditions for efficient intrafactor resource allocation that have been postulated provide a basis for tests which are biased in the direction of demonstrating inefficient allocation, i.e., they will be "strong" tests of efficiency and "weak" tests of inefficiency. However, the importance of this defect in the testing procedure is reduced by the fact that the lags themselves may partially reflect the presence of noneconomic barriers.

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[5] The contention that cyclical unemployment will be apportioned evenly throughout the system ignores the possible differential impact of cyclical unemployment on various sectors. To this extent, it biases the tests derived from this proposition in favor of showing an inefficient market mechanism.

Unfortunately, the money-illusion assumption cannot be disposed of so simply. If prices had fluctuated cyclically about a relatively stable level during the period for which data will be examined (1948-1960), the impact of price changes could be ignored. However, the existence of a strong upward secular trend in prices combined with a similar trend in real-wage rates in the post-World War II United States presents the disturbing possibility that noneconomic barriers to the allocative process can exist even though the necessary conditions which have been postulated are satisfied.[6] Consider the possibility of a powerful monopoly element in one sector of the labor market, a monopoly element strong enough to exact wage increases in excess of those warranted by a competitive labor market while simultaneously restricting entry into the sector. If there is a strong secular trend (particularly an upward one) in the wage level, it may be sufficient to produce a situation in which wage rates in both the monopoloid and nonmonopoloid sectors move upwards, although the monopoloid sector gains more rapidly than the nonmonopoloid. Under these conditions wage rates in the monopoloid sector may be highly correlated in a positive fashion with those in the nonmonopoloid, despite the presence of the restrictive barrier. However, if this is the case, through time one (or both) of two things will occur: either

1) the dispersion of sector wage rates will change appreciably; or
2) the rank order of the various sector wage rates will be markedly altered.

The first possibility is indicative of a situation in which the monopoloid sectors are initially concentrated at one extreme of the distribution of sector wage rates. If this occurs, the impact of the restrictive barriers is either to increase or to decrease the range of sector wage rates (depending on whether the monopoloid sectors are concentrated at the high or low end, respectively, of the distribution). On the other hand, if the monopoloid sectors are not concentrated at one extreme of the distribution, relative gains by these sectors at the expense of nonmonopoloid ones are likely to produce alterations in the rank order of the sector wage rates. Therefore, in order to guard against the possibility of the two previously defined necessary conditions being satisfied in the face of restrictive barriers in the allocative mechanism, two additional conditions for concluding that the labor market is an efficient intrafactor allocative mechanism must be postulated:

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[6] Only wage rates are considered in this respect on two grounds: (1) the secular trend in unemployment rates since World War II has not been as marked or as persistent as that in wage rates; and (2) it is by no means clear that efficient intrafactor resource allocation implies maintaining absolutely constant unemployment-rate differentials. On the other hand, the discussion of wage-rate differentials is in terms of constant absolute wage differentials.

3) no appreciable change through time in the distribution of sector wage rates, and
4) no appreciable change through time in the rank order of the sector wage rates.

Of course, depending on whether the money-illusion assumption is retained, the distribution of wage rates referred to in these conditions will consist of either real- or money-wage rates. In order to be consistent with our labor-market model (which includes maximizing behavior on the part of workers) the money-illusion assumption is discarded and, consequently, the distribution of real-wage rates will be employed for purposes of determining whether conditions (3) and (4) are satisfied.[7]

To summarize, satisfaction of all four of the conditions which have been postuated is sufficient to demonstrate efficient intrafactor resource allocation. A similar analysis of the impact of noneconomic barriers to labor mobility leads to the conclusion that a failure to satisfy any one of these conditions is sufficient evidence of the presence of such barriers.[8]

III. *Problems of Testing*

The statistical technique to be employed in estimating the degree of correlation between the various sectors is an extension of a device employed by R. M. Solow in his discussion of the constancy of relative shares [9]. To understand it, consider expressions (1) and (2):

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[7] Discarding the money-illusion assumption does not affect the validity of conditions (1) and (2).

[8] One could argue that this is not the case on the grounds that a series of demand shifts in a particular sector accompanied by adjustment lags could result in a failure to satisfy conditions (3) and (4). This requires assuming first of all that adjustment lags do not reflect noneconomic barriers. In addition, if the demand shifts in question represent a long-run trend in operation prior to the beginning of the period of observation, they must become progressively greater in order to produce a change in the dispersion of wage rates. On the other hand, if the series of shifts originates within the period of observation (thus producing an initial displacement of wage rates), continual reinforcement of these shifts is required in order to produce a change in the dispersion of wage rates.

$$\sigma^2_{xy} = \alpha^2_x\sigma^2_x + 2\alpha_x\alpha_y \operatorname{cov}_{xy} + \alpha^2_y\sigma^2_y \tag{1}$$

$$r_{xy} = \frac{\operatorname{cov}_{xy}}{\sigma_x\sigma_y} \,. \tag{2}$$

Expression (1) details the method of determining the aggregate variance (σ^2_{xy}) of two series of numbers (denoted by subscripts x and y) whose variances (σ^2), weight (α) in the aggregate series, and covariance (cov_{xy}) are known. Expression (2) is merely that for the correlation coefficient (r) between the two series, x and y.

Now, let us assume that r_{xy} equals zero, i.e., that the two series are uncorrelated. Solving (2) for cov_{xy} and substituting the result in (1) produces

$$\hat{\sigma}^2_{xy} = \alpha^2_x\sigma^2_x + \alpha^2_y\sigma^2_y \tag{3}$$

where $\hat{\sigma}^2_{xy}$ denotes a predicted variance (in this case on the basis of an assumption of zero correlation between the two series). Similarly, it is possible to assume a value of one for r_{xy} and perform a like substitution. The result is:

$$\hat{\sigma}^2_{xy} = \alpha^2_x\sigma^2_x + 2\alpha_x\alpha_y\sigma_x\sigma_y + \alpha^2_y\sigma^2_y. \tag{4}$$

The two predicted variances constitute boundary values which permit an evaluation of the actual variance (σ^2_{xy}). The more closely correlated the two series, the closer the actual variance will be to the predicted variance for an assumed r_{xy} of one. Obviously, this approach is inferior to a normal calculation of r_{xy} if only two series of numbers are involved. However, expressions (3) and (4) can be generalized to deal with n series of numbers. The generalized form of (3) is:

$$\hat{\sigma}^2_0 = \sum_{i=1}^{n} \alpha^2_i\sigma^2_i \tag{3a}$$

where $\hat{\sigma}_0^2$ denotes a predicted variance assuming r for any pair of series equal to zero (or that any positive correlations are exactly cancelled out by negative correlations) and i is a subscript denoting an individual series.

Expression (4) can be generalized to:

$$\hat{\sigma}^2_1 = \sum_{i=1}^{n} \sum_{j=1}^{m} \alpha_i\alpha_j\sigma_i\sigma_j \tag{4a}$$

where $\hat{\sigma}_1^2$ denotes the predicted variance assuming r for any pair of series equal to one, i and j are subscripts denoting the individual series (each series has both an i and a j subscript), and $n=m$.

In its generalized form the technique of calculating predicted aggregate variances makes it possible to determine the extent to which n series of numbers are correlated with one another. The more highly correlated the various series with each other, the closer the actual variance will be to the predicted variance which assumes r's equal to one. A precise measure of the extent to which the series are correlated is given by expression (5):

$$R=\frac{\sigma^2-\hat{\sigma}_0^2}{\hat{\sigma}_1-\hat{\sigma}_0}, \tag{5}$$

where R denotes the extent of correlation and σ^2 denotes the actual variance of the aggregate series. Obviously, if negative correlations predominate between the sectors, $\sigma^2<\hat{\sigma}^2_0$ and R will be negative. A calculation of R for the movements of both sectoral wage rates and sectoral unemployment will provide some measure of the extent to which the labor market carries out its intrafactor allocative function. High positive values of R for both wage-rate and unemployment movements will satisfy the first two necessary conditions for concluding that the labor market is effective in allocating labor on an intrafactor basis, while low (or negative) values of R for one or both of the variables under consideration will indicate that the labor market is not efficiently allocating resources. Obviously, at this point there is a problem in determining just what is meant by the terms high and low as applied to values of R. For the purposes of this discussion a value of R which is significantly different from zero at the .05 level will be considered decisive, i.e., values of R that are in excess of this standard will be treated as "high" and values of R below will be viewed as "low."[9]

As to the sectors to be examined, three approaches appear to be desirable: examination on an industrial, occupational, and geographical

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[9] Expression (5) can be rewritten as a weighted average of all the possible correlation coefficients that exist among pairs of sectors. Thus the R value can be viewed as the mean value of a distribution of correlation coefficients. This distribution can be transformed into a z-distribution with mean

$$\tfrac{1}{2}\log_e\frac{1+R}{1-R} \text{ and variance } \frac{1}{n-3}$$

where n denotes the number of observations through time [3, p. 197 ff.]. Since z is approximately normally distributed, the significance of the R value can be tested by employing the following test statistic in a conventional manner:

$$\tfrac{1}{2}\log_e\frac{1+R}{1-R}\sqrt{\frac{k(k-1)(n-3)}{2}}$$

where k denotes the number of sectors.

basis. Fortunately, sufficient empirical data are available to permit testing on all three counts. Estimates of unemployment rates by industry, occupation, and region are available from the statistics of the Department of Labor. These data are available since 1948 on an industrial and occupational basis and since 1949 on a regional basis. For this study the unemployment data will be utilized through 1960.

Wage data present certain problems. There are data available describing money-wage rates for various sectors. However, the coverage is not complete either temporally or sectorally. Consequently, as a proxy for wage rates, annual income per worker is used in the analysis. This may introduce certain distortions due to differences in the length of the work year in various sectors and the presence of non-wage elements in the income package. Whatever biases are introduced by these distorting factors will weight the empirical results in favor of a finding that the labor market is an inefficient allocator of resources. This must be kept in mind in interpreting the empirical results. Annual income data are available on an industrial, occupational, and regional basis. The industrial data may be derived from the Department of Commerce's statistics detailing compensation of employees and the number of full-time-equivalent employees. These data are available for the full period 1948–60. Occupational income data are contained in the Federal Reserve Board's periodic surveys of consumer finances and are employed for the period 1948–60. Regional income data were obtained

TABLE 1—UNEMPLOYMENT RATES AND COMPENSATION PER FULL-TIME-EQUIVALENT EMPLOYEE, BY INDUSTRY, UNITED STATES, 1948–60

Industry	Weight (α) (1954)	Unemployment Rate (per cent)												
		1948	1949	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960
Mining	.0184	3.0	8.5	6.7	3.8	3.4	5.0	12.3	8.2	6.5	5.5	9.6	8.6	8.5
Construction	.0624	7.6	11.9	10.7	6.0	5.5	6.1	10.5	9.2	8.2	9.1	12.9	11.3	11.4
Durables	.2163	3.5	7.5	5.3	2.6	2.4	2.0	6.6	4.0	4.0	4.3	9.8	5.5	5.7
Nondurables	.1627	3.6	7.0	6.0	4.0	3.2	3.1	5.7	4.4	4.4	4.5	6.7	5.1	5.3
Transport	.0641	3.0	5.2	4.1	1.9	1.9	1.8	4.8	3.5	2.4	2.8	5.2	3.8	4.0
Trade	.2543	4.3	5.8	5.8	3.7	2.1	2.0	5.2	4.3	4.1	4.2	6.4	5.5	5.6
Finance	.0526	1.6	1.8	2.0	1.3	1.5	1.6	2.0	2.1	1.4	1.8	2.9	2.6	2.4
Services	.1692	3.5	5.1	5.0	3.1	2.6	2.4	4.0	3.8	3.2	3.1	4.2	4.0	3.8
Total[a]	1.0000	3.8	6.4	5.6	3.3	2.6	2.5	5.6	4.4	4.0	4.2	7.0	5.3	5.4

Industry	Weight (α) (1954)	Compensation per Full-Time-Equivalent Employee (dollars)												
		1948	1949	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960
Mining	.0194	3593	3421	3755	4231	4415	4756	4806	5189	5529	5750	5755	6137	6329
Construction	.0658	3265	3358	3506	3900	4182	4429	4546	4647	4925	5209	5357	5596	5877
Durables	.2280	3298	3386	3689	4103	4369	4629	4714	5030	5310	5563	5789	6152	6332
Nondurables	.1714	3039	3102	3298	3549	3731	3916	4047	4239	4473	4708	4871	5137	5287
Transport	.0646	3657	3773	3929	4249	4474	4677	4800	5021	5332	5644	5915	6254	6458
Trade	.2430	2916	2990	3146	3291	3406	3567	3695	3851	4019	4191	4320	4516	4689
Finance	.0526	3109	3209	3412	3569	3749	3917	4121	4299	4493	4692	4907	5287	5658
Services	.1552	2158	2219	2275	2437	2618	2755	2878	2963	3106	3262	3383	3566	3761
Total[a]	1.0000	3001	3075	3261	3523	3717	3913	4032	4226	4448	4666	4834	5103	5297

[a] Computed on basis of 1954 weights.

Sources: Unemployment data [11]; income data [13] [15].

from the Commerce Department's estimates of personal income by state for the years 1948–60.

IV. *Empirical Results*

Table 1 presents a summary of the unemployment and income data for eight industries (mining, construction, durable-goods manufacturing, nondurable-goods manufacturing, transport, wholesale and retail trade, finance, and services) for the period 1948–60. The aggregate unemployment and income series in this table are constructed from the sector series by applying the 1954 weights (α values) appropriate to each sector to the non-1954 observations and summing them for each year. This conforms to Solow's use of "fixed" weights [9] and removes any distortion in the empirical results that might result from changes in the relative importance of sectors through time. From these data predicted variances for the aggregate series are calculated and compared with the actual variances and R values determined as follows:

1. Based on predicted variances of 0.7495 (assuming intersectoral correlations of zero) and 3.5074 (assuming perfect correlation between sectors), an R value of .8809 is obtained from the unemployment series (the actual variance is 3.1788).

2. Based on predicted variances of \$810 and \$4,419, an R value of .5891 is obtained from the income series (the actual variance is \$2,936).

In both cases (and in all instances to follow) the variances are calculated on the basis of first differences. This was done to minimize the impact of serial correlation on the R value. For example, the R value for the income data is .9904 if first differences are not employed. (The use of first differences is not crucial for the unemployment data; in the absence of first differences the R value would be .8791.)

Both the R values for the income and unemployment data are statistically significant at the .05 level, thus satisfying the first two necessary conditions for considering the labor market to be an efficient intrafactor allocator of resources.

In order to determine whether the third condition is satisfied, the standard deviation (σ) is employed as a measure of the dispersion of the distribution of wage rates. In 1948 the standard deviation of the distribution of sector wage rates was \$469 as compared to a 1960 value of \$757. Prior to calculating the σ's the 1960 money-wage rates were expressed in terms of 1948 prices (the consumer price index was used for this purpose). The increase in the standard deviation of the distribution of sector real-wage rates between 1948 and 1960 amounts to approximately a 61 per cent change. It is difficult to interpret an increase of this magnitude as being indicative of "no appreciable change in the dispersion of sector wage rates." Therefore, it appears that the

TABLE 2—COMPENSATION PER FULL-TIME-EQUIVALENT EMPLOYEE AND RELATIVE GAINS IN COMPENSATION, BY INDUSTRY, UNITED STATES, 1948 AND 1960

(in 1948 prices)

Industry	Compensation per Full-Time-Equivalent Employee		Gain 1948–1960	
	1948	1960	Absolute	Relative[a]
Mining	$3593	$5104	$1511	+18.88%
Construction	3265	4740	1475	+16.05
Durables	3298	5106	1808	+42.25
Nondurables	3039	4264	1225	− 3.62
Transport	3657	5208	1551	+22.03
Trade	2916	3781	865	−31.94
Finance	3109	4563	1454	+14.40
Services	2158	3033	875	−31.16
Total[b]	3001	4272	1271	—

[a] Computed by comparing the absolute gain in the sector to the over-all absolute gain and expressing the difference as a percentage of the over-all absolute gain.

[b] Computed on basis of 1954 weights.

Source: Table 1.

third of the necessary conditions for demonstrating efficient intrafractor resource allocation is not satisfied in the case of an industrial classification of markets. The sources of the increase in the dispersion of industrial wage rates can be gauged from the data of Table 2. These data show that between 1948 and 1960 five sectors experienced real-wage increases in excess of the average (mining, construction, durable-goods manufacturing, transportation, and finance) while three sectors experienced increases below the average (nondurable-goods manufacturing, trade, and service). Rough estimates of the relative gains and losses incurred by the various sectors are also presented. The largest relative gains were registered by the durable-goods sector (42.25 per cent) and the transport sector (22.03 per cent), while the largest losses were suffered by the trade sector (31.94 per cent) and the service sector (31.16 per cent).[10]

The analysis conducted on an occupational basis (employing the data contained in Table 3) also results in a finding of inefficient intrafactor resource allocation. Eight different occupational groupings are used with the unemployment data (operatives, craftsmen and foremen, farm laborers, nonfarm laborers, sales workers, service workers, professional and technical, and clerical), and four groupings with the income data (professional and semiprofessional, clerical and sales, skilled and semiskilled, and unskilled and service). The predicted variances for the unemployment series are 0.6341 and 2.8337, while the actual variance

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[10] The failure to satisfy the third condition makes it unnecessary to test for the fourth condition.

TABLE 3—UNEMPLOYMENT RATES, 1948–60, AND MEAN INCOME, 1948–60, BY OCCUPATION, UNITED STATES

Occupation	Weight (α) (1954)	Unemployment Rate (per cent)												
		1948	1949	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960
Operatives	.2491	4.1	8.0	6.8	4.3	3.9	3.2	7.6	5.7	5.4	5.7	10.1	7.0	7.2
Craftsmen & Foremen	.1736	2.9	5.9	5.6	2.6	2.4	2.6	4.9	4.0	3.2	3.5	6.4	4.9	4.8
Farm Laborers	.0528	2.3	3.9	5.0	2.1	2.3	2.5	4.2	3.7	3.7	3.4	5.7	4.8	4.5
Nonfarm Laborers	.0767	7.5	12.9	11.7	5.6	5.7	6.1	10.7	10.2	8.2	8.8	13.9	11.8	11.8
Sales Workers	.0881	3.4	3.5	4.0	2.8	2.5	2.1	3.7	2.4	2.7	2.1	3.4	3.5	3.3
Service Workers	.0956	4.8	6.1	6.8	4.3	3.7	3.6	5.2	5.8	4.8	4.7	7.2	6.0	5.6
Professional & Technical	.1094	1.7	1.9	2.2	1.5	1.0	0.8	1.6	1.0	1.0	1.1	1.7	1.5	1.5
Clerical	.1547	2.3	3.8	3.4	2.1	1.8	1.7	3.1	2.6	2.4	2.5	3.8	3.3	3.6
Total[a]	1.0000	3.5	5.9	5.6	3.2	2.9	2.7	5.3	4.4	3.9	4.0	6.8	5.3	5.3

Occupation	Weight (α) (1950)	Mean Income (dollars)												
		1948	1949	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960
Professional & Semiprofessional	.1094	5140	5350	5630	6020	6670	6790	7380	8140	7770	8150	8270	8520	9090
Clerical & Sales	.2428	3350	3260	3910	3920	4180	4480	4420	4980	5190	5340	5600	6280	6240
Skilled & Semiskilled	.4227	3470	3350	3530	3970	4200	4550	4500	4830	5030	5460	5470	5850	5820
Unskilled & Service	.2251	2310	2407	2350	2320	2620	2760	2990	2840	3250	3210	3190	3320	3530
Total[b]	1.0000	3362	3335	3586	3811	4110	4375	4456	4781	4968	5219	5295	5677	5764

[a] Computed on basis of 1954 weights.

[b] Computed on basis of 1950 weights.

Sources Unemployment data [11]; income data [10]. (Prior to 1960 the collection of the data was sponsored by The Board of Governors of The Federal Reserve System and published periodically in the *Fed. Res. Bull.*)

is 2.6718. This produces an R value of .8917. For the income data the predicted variances are \$13,932 and \$48,715 with an actual variance of \$14,782. The resultant R value is .0244. The R value for the unemployment data is statistically significant, while that for the income data obviously is not. Consequently, the second of the necessary conditions for concluding that the labor market is an efficient intrafactor allocator of resources on an occupational basis is not satisfied.[11]

Finally, regional unemployment and income data are analyzed in a similar fashion for the years 1948-60 for income and 1949-60 for unemployment. Eight regions are used in the analysis: New England, Mid-East, Great Lakes, Plains, Southeast, Southwest, Rocky Mountain, and Far West.[12] Table 4 presents the basic data used in the analysis. From these data predicted variances for the unemployment series of 0.5149 and 2.4793 were calculated. Based on an actual variance of 2.2642, the R value is .8905. The predicted variances for the income data are \$754 and \$4,097 with an actual variance of \$3,432. These result in an R value of .8011, the largest thus far obtained with income data. Both R values are statistically significant at the .05 level, satisfying the first and second of the necessary conditions for demonstrating efficient intrafactor resource allocation.

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[11] The failure to satisfy the second condition makes it unnecessary to test for the third and fourth conditions.

[12] The states are grouped as follows: New England: Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont; Mid-East: Delaware, Maryland, New Jersey, New York, and Pennsylvania; Great Lakes: Illinois, Indiana, Michigan, Ohio, and Wisconsin; Plains: Iowa, Kansas, Minnesota, Missouri, Nebraska, North Dakota, and South Dakota; Southeast: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Virginia, and West Virginia; Southwest: Arizona, New Mexico, Oklahoma, and Texas; Rocky Mountain: Colorado, Idaho, Montana, Utah, and Wyoming; and Far West: California, Nevada, Oregon, and Washington.

This necessitates conducting the tests for the third and fourth conditions. With respect to the third condition the respective standard deviations of the distribution of real-wage rates for the years 1948 and 1960 are \$251 and \$291. The change over this period represents an increase of about 16 per cent. Considering that no adjustment in the data has been made to take account of differences in industrial mix between regions, this increase can probably be regarded as insignificant. This is particularly true in that the regions with large increases in income (New England and the Mid-East) also have a greater preponderance of their employment in industrial classifications that show large increases in wages (mining, transportation, durable-goods manufacturing, and construction). For example, the two regions showing the largest increases in per capita income between 1948 and 1960 (New England and the Mid-East) have 25.67 and 21.90 per cent of their employment

TABLE 4.—UNEMPLOYMENT RATES, 1949–60, AND PER CAPITA PERSONAL INCOME, 1948–60, BY REGION, UNITED STATES

Region	Weight (α) (1954)	Unemployment Rate (per cent)											
		1949	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960
New England	.0764	8.2	5.2	3.5	3.5	2.7	4.9	3.4	2.7	3.9	6.3	4.4	4.6
Mid-East	.2776	6.2	4.7	3.1	3.1	3.0	5.3	4.0	3.4	3.9	6.8	5.2	5.1
Great Lakes	.2340	4.9	3.4	2.1	2.2	1.8	4.7	2.4	2.6	2.9	6.5	3.2	3.9
Plains	.0679	3.4	3.2	1.9	1.9	2.1	3.7	2.8	2.4	2.6	4.0	3.0	3.4
Southeast	.1561	6.0	4.7	3.2	3.4	3.4	5.6	3.2	3.2	5.2	5.7	4.1	4.2
Southwest	.0588	2.6	2.4	1.2	1.2	1.6	2.4	1.6	1.5	2.0	3.6	2.8	3.1
Rocky Mountain	.0191	3.3	3.4	1.7	1.7	2.0	3.0	2.0	1.7	2.5	4.3	3.2	3.3
Far West	.1137	8.7	6.4	3.3	3.4	3.5	4.5	2.7	2.5	3.6	5.8	4.0	4.9
Total[a]	1.0000	5.8	4.4	2.7	2.8	2.7	4.8	3.1	2.8	3.6	6.0	4.0	4.4

Region	Weight (α) (1950)	Per Capita Personal Income (dollars)												
		1948	1949	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960
New England	.0616	1511	1474	1628	1831	1917	1968	1957	2087	2214	2298	2302	2388	2471
Mid-East	.2233	1646	1621	1761	1912	1995	2076	2051	2145	2303	2406	2407	2515	2591
Great Lakes	.2020	1592	1514	1661	1869	1939	2050	1973	2078	2207	2260	2200	2316	2373
Plains	.0932	1414	1292	1408	1529	1604	1609	1642	1647	1743	1856	1954	1988	2071
Southeast	.2241	968	943	1009	1128	1194	1232	1217	1291	1402	1446	1485	1566	1607
Southwest	.0754	1173	1246	1286	1415	1497	1528	1541	1581	1702	1772	1819	1883	1912
Rocky Mountain	.0232	1387	1346	1428	1647	1703	1672	1628	1686	1793	1884	1965	2029	2108
Far West	.0972	1711	1687	1795	1984	2078	2126	2102	2189	2326	2397	2430	2570	2642
Total[b]	1.0000	1418	1379	1491	1650	1727	1786	1761	1841	1969	2043	2058	2152	2213

[a] Computed on basis of 1954 weights.
[b] Computed on basis of 1950 weights.
Sources: Unemployment data [16]; income data [5] [14].

in the two highest wage-increase industrial categories (durable-goods manufacturing and construction) as compared to 14.77 and 14.70 in these categories in the two regions with the lowest increases in income (the Plains and Rocky Mountain areas).[13] Similarly, the two regions with the lowest income increases have 40.02 and 42.89 per cent of their nonagricultural, nongovernmental employment in the industrial categories with the lowest increases in wages (trade and service) as compared to 36.78 and 38.05 per cent in the two highest income-increase regions. Therefore, the apparent widening of regional income differentials probably reflects the impact of the widening of industrial differentials.[14] Consequently, the third of the necessary conditions can be considered as satisfied.

As to the fourth condition, the rank order of real-wage rates, the rank-order correlation coefficient between the 1948 and 1960 rankings of regional wage rates is .9524, which is significant at the .05 level. The only changes in ranking which occurred were an exchange of the third and fourth positions by New England and the Great Lakes region and a similar exchange of the fifth and sixth positions by the Plains and Rocky Mountain regions. Consequently, the evidence appears to satisfy the fourth and last of the necessary conditions for demonstrating efficient intrafactor resource allocation between regions.

V. *The Structural-Unemployment Problem*

Some brief reference has previously been made to the upward secular drift in the unemployment rate in the United States in recent years. By way of background, unemployment levels during the course of the past three business cycles (measured from peak to peak) have averaged 4.2 per cent for the 1948-53 cycle, 4.4 per cent for the 1953-57 cycle, and 5.9 per cent during the 1957-60 cycle [11]. The last of these figures offers the unhappy prospect of relatively high levels of unemployment during relatively prosperous business periods; witness the average unemployment rate in excess of 5 per cent which occurred during the expansion phase of the 1957-60 cycle.

There are two alternative explanations of this high rate of unemployment by academicians and policy makers: (1) the structural-transformation hypothesis, which contends that a basic change has occurred in the adjustment mechanism that serves to shift unemployed-labor resources from sector to sector in the economy; and (2) the aggregate-demand hypothesis, which holds that the high rates of unemployment in the 1957-60 cycle merely reflect a lack of an adequate level of aggregate demand in the system.

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[13] These percentages are derived from [12].

[14] A crude attempt at standardizing for differences in industrial mix (consisting of computing a predicted regional distribution of income on the basis of knowledge of the industrial mix of different regions and the behavior of income in each industrial category) revealed that the actual standard deviation of the 1960 distribution (291) was somewhat less than what might have been expected if there were no differences in industrial mix (338).

The first of these explanations argues that the labor-market reactions of the economic system to pressures produced by economic change have been altered in some fundamental respect. As a consequence, higher levels of frictional unemployment can be expected over the course of the business cycle. In a policy sense this suggests that conventional tools of aggregative economic policy, i.e., monetary and fiscal policy, will not be as effective as previously in providing amelioration of these high levels of unemployment. Rather, the application of these tools to this problem might serve only to inflate the general price level with little reduction in the general level of unemployment. On the other hand, the policy implications of the aggregate-demand hypothesis are that the high levels of unemployment in the 1957-60 cycle were the result of a failure to apply adequately the conventional tools of monetary-fiscal policy and that a stronger application of these tools could be expected to reduce the level of unemployment without the risk of severe inflationary consequences.

The rather obvious contradiction in the policy implications of these alternative hypotheses for explaining recent increases in unemployment elevates the consideration of them to something more than mere academic interest. Consequently, it is appropriate at this point to explore the implications of these hypotheses in an effort to determine whether the previous discussion of the efficiency of the labor market as an allocative mechanism can shed any light on their validity.

As has been noted, the structural argument presupposes a change in the adjustment mechanism by which the labor market accommodates economic change. Therefore, assuming some random exogenous change in the economy, such as a demand shift or an alteration in technology, and assuming that the change exerts its impact in an isolated fashion, i.e., on one particular segment of the economy, the effect of that change should be manifested in an increase in the relative amount of unemployment in the affected sector. In short, unemployment in the affected sector should increase relative to the general rate of unemployment. Further, it would be expected that the greater the severity of the random economic change, the greater the adjustment that would be required of the labor-market mechanism. Consequently, more severe economic changes would tend to result in greater amounts of relative unemployment in the affected sector. The effect of this would be to alter the distribution of unemployment among the various sectors of the economy. Therefore, if the structural argument is valid, the distribution of unemployment during the 1957-60 cycle should differ from that during the previous cycles. On the other hand, if the recent unemployment experience has been induced by a deficiency in aggregate demand, the distribution of unemployment throughout the various sectors should be roughly similar to the distribution prior to the 1957-60 cycle. This is based on the presumption that deficiencies in aggregate demand are distributed evenly throughout the system. Consequently, the aggregate-demand hypothesis would be supported by a lack of change in relative unemployment in the various sectors of the economy.

A straightforward method of measuring the degree of change in relative unemployment in a sector has been developed [11]. It consists of regressing sectoral unemployment against total unemployment and time for each of the sectors. The findings can be evaluated statistically and a judgment expressed as to the relative strength of the factors of time and aggregate unemployment upon the level of sectoral unemployment. Obviously, if the time factor is statistically significant, support is lent to the structural-unemployment hypothesis. However, if it is not significant, the aggregate-demand hypothesis is supported. In effect, this technique amounts to an evaluation of time as a factor in explaining a lack of correlation between unemployment series for different sectors. If the unemployment series for all sectors were perfectly correlated with one another, sectoral unemployment would be perfectly correlated with aggregate unemployment, and there would be no unexplained variance which might be accounted for by the time factor. On the other hand, if each and every pair of sectoral-unemployment series had a correlation coefficient of zero, regressing sectoral unemployment against aggregate unemployment would explain none of the variance (leaving a good deal of room for the time factor to work).

It should be fairly obvious at this juncture that this technique is similar to the statistical technique that has been used in this paper to evaluate the efficiency of the labor market as an allocative mechanism with one major exception, viz., it evaluates each sector individually, whereas the aggregative technique used in this paper expresses the intersectoral relationships in the form of a single statistic. Being able to incorporate this complex of relationships in a single statistic has sizable advantages for purposes of evaluating the validity of the structural-unemployment and aggregate-demand hypotheses. The sector-by-sector evaluation requires some judgment as to what constitutes a meaningful number of sectors in which the time factor is statistically significant. This type of judgment can be avoided by employing the R statistic previously described and varying the time period for which it is calculated. In this fashion, the R value for one period of time may be employed as a control for the purpose of evaluating the R statistic for another period. Specifically, the structural-unemployment hypothesis focuses on the period 1957-60. Consequently, it should prove enlightening to calculate an R value for the period prior to 1957 (1948 through 1956) and compare it with an R value for the period 1957 through 1960. If the structural-unemployment hypothesis is borne out, the R value for the 1957-60 period will be lower than that for the pre-1957 period, as unemployment will not be as strongly correlated between various sectors in the post-1957 period. Of course, similar R values for the two periods (or for that matter a larger 1957-60 R value) will substantiate the aggregate-demand hypothesis.

The suggested calculations were made for the three different sectoral classifications employed elsewhere in the paper, i.e., on an industrial, occupational, and a regional basis. The results show that for all three bases of classification the R values for the 1957-60 period are actually somewhat larger than those for the pre-1957 period. For the industrial classification the 1957-60 value is .9620 as compared to .8890 for the pre-1957 period. Similar values for an occupational classification are .9110 and .8964, while those for a regional breakdown are .9130 and .8923. The difference on an industrial basis is significant at the .05 level, while those on an occupational and regional basis are not.[15] The conclusion suggested by these data is that the structural-unemployment hypothesis is not a valid explanation of the increase in unemployment that has marked recent developments in the U.S. economy. This confirms the findings of [11], which were based on an analysis of sectoral unemployment by occupation and industry.

VI. *Conclusions*

At least three major conclusions emerge from the data analyzed in this paper. First, there is the finding that the labor market is a reasonably efficient allocator of labor between regional sectors of that market. This runs contrary to certain widely accepted notions regarding a lack of geographic mobility between labor markets. A word of caution is in

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[15] The test statistic is derived in the same fashion as that for evaluating the significance of the difference between R and zero. It is:

$$\frac{\bar{z}_1 - \bar{z}_2}{\sqrt{\dfrac{2(n_1 + n_2 - 6)}{(n_1 - 3)(n_2 - 3)k(k - 1)}}}$$

where $\bar{z}_i$ denotes the mean of the respective z-distributions

$$\left(\text{equal to } \tfrac{1}{2} \log_e \frac{1 + R_i}{1 - R_i}\right),$$

n_i denotes the number of observations through time, and k denotes the number of sectors involved.

order concerning this conclusion. Mobility may take a variety of forms. Its positive aspect is the direct movement of unemployed workers from one sector to another. However, the same result may be obtained by a decline in the numbers who attempt to enter less desirable sectors of the market. There is some positive evidence that this may be the case on a regional basis. Mobility patterns in certain areas suffering chronic unemployment reveal that the level of in-immigration into these areas is markedly less than that into other areas which are similar except for their level of unemployment. At the same time these chronically depressed areas do not have levels of out-migration which are greater than those of other areas [4]. Thus, the net effect is one of out-migration from these areas despite the fact that there is no apparent unemployment-induced increase in out-migration. Regardless of how the transfer is effected, the impact on the labor market is the same, a redistribution of unemployment.[16]

Secondly, there are apparently barriers to the mobility of workers between occupations and industries that are not strictly the product of the opportunity costs associated with such transfers. Interestingly, the impact of these barriers is felt almost exclusively in the income levels associated with the occupational and industrial groupings. For example, among occupations in the interval 1948-60 annual income for the unskilled and service group increased by only $1,220, while that for the professional and semiprofessional category rose by $3,910. This suggests the presence of barriers to mobility that restrict entry into the latter occupational category. One possible explanation for this phenomenon may simply be that typically the length of time necessary to make an occupational shift of the magnitude implied by the broad categories of markets employed in the empirical analysis is so lengthy that it does not exert an impact in a period of thirteen years(the interval for which empirical data relating to income are presented in this study). One might go so far as to suggest that occupational shifts of this magnitude occur from generation to generation rather than within a single lifetime. If this is the case, it is not surprising that the impact of any adjustment process cannot be detected over a span of a few years.

Even more interesting is the behavior of wage incomes in the industrial sectors. Certain sectors (notably durable-goods manufacturing, transportation, mining, and construction—see Table 3 for specific details) have benefited in terms of income at the expense of other sectors (particularly the trade and service sectors). This state of affairs is remarkably consistent with a rather standard theoretical argument relating to the impact of labor unions in the labor market [8]. To recapitulate, this argument holds that unions practice restriction in the labor market in order to protect their wage gains, and as a result the level of employment is reduced in the union sector. The impact of this is to shift workers from the union to the nonunion sector with a resultant depression of nonunion wage rates. Since the four sectors which have benefited most from the increasing dispersion of industrial wage rates over the period 1948-60 represent the strongest centers of unionism in the United States, while the two sectors which have suffered such sizable relative losses are among the least unionized in the economy, the evidence developed by our testing procedure strongly supports this traditional theoretical argument.[17] In addition, the observed behavior of unemployment rates in various sectors argues that the labor market operates in a fashion that would minimize the impact on the union sector of unemployment induced by excessive wage-rate increases. This means that the cost to unions (in terms of unemployment) of wage-rate increases in excess of those warranted by a competitive market may not be particularly great.[18]

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[16] A clarifying comment is necessary with respect to the conclusion regarding labor mobility. In footnote 3 it was noted that one situation does exist in which immobility of labor is consistent with an equalization of wage rates and an absence of involuntary unemployment. This raises the possibility that the empirical findings of this paper may be consistent with more than one hypothesis with respect to labor mobility. This is not the case, for in the situation cited (similar sector production functions and perfect mobility of capital) complete equalization of wage rates and not a consistent hierarchy of wage rates would be the end result.

The third and final conclusion is that the structural unemployment explanation for the recent increase in unemployment is not valid. This, of course, argues for the policy program that accompanies such a finding, i.e., a stronger use of monetary-fiscal policy as a device to alleviate the higher levels of unemployment. However, a qualifying comment is in order. The evidence by no means clearly indicates that the recent high levels of unemployment are the result of a classic deficiency of aggregate demand. Rather, the findings at the microeconomic level offer the possibility that the impact of union market power may be disrupting the labor-market allocative process at the interfactor level, i.e., that the relative price of labor in general has been increased by the actions of labor unions to a level that generates widespread unemployment. For this to be the case, it is necessary to have some assumption to the effect that there is downward "stickiness" of relative sectoral wage rates in the nonunion areas that limits the extent to which these sectors can make the adjustments required by the impact of changes in the union portions of the economy. If this is true, the post-1957 experience in the United States represents a classic case of wage-push inflation with its attendant unemployment effects. And, of course, alleviation of this unemployment through a deliberate stimulation of aggregate demand (such as the proposed tax reduction) merely alters the situation to one of "qualified" wage-push inflation.

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[17] From [2] and [12] the percentage of union membership by industry in 1960 can be estimated. For the four industries showing the largest relative wage gains these are: mining, 84 per cent; construction, 79 per cent; durable-goods manufacturing, 56 per cent; and transport, 81 per cent. In the two sectors showing the greatest relative losses the percentages are: trade, 7 per cent; and service, 17 per cent. For the other two industrial categories—nondurable-goods manufacturing and finance—the respective percentages are 43 and 3. From these data it can be seen that the only sector which is strongly at variance with the relationship between union activity and relative wage gains is the finance area with a relative gain of 14.40 per cent between 1948–60 despite the low level of union activity. This is difficult to interpret, although a conceivable explanation may be that the finance occupational mix and/or changes in that mix have weighted the increases in wages more heavily with the professional and technical occupations that show large relative gains in income in this period. A simple rank-order comparison of the degree of unionization and relative wage gains produces a correlation coefficient of .67 which is significant at the .10 level. A similar comparison excluding finance gave a correlation of .71 which is significant at the .05 level.

[18] Since several *caveats* have been noted with respect to the possibility of biases in the procedure for testing for efficient resource allocation, a summary of their implications for the

Collectively, these conclusions present a somewhat varied picture. In some respects the labor market appears to perform more effectively than is generally believed, e.g., in terms of its ability to efficiently allocate resources on an interregional basis and its ability to distribute unemployment in a fairly even fashion throughout the system. On the other hand, its over-all performance as an efficient allocator of resources on an industrial and occupational basis is far from distinguished. In particular, the apparent presence of sizable elements of market power in the hands of labor unions is somewhat disturbing. To sum up, we cannot be exceedingly happy with the performance of the labor market as an allocative mechanism, a state of affairs that argues strongly for a rethinking of our policy position with respect to labor-market institutions.

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stated conclusions is in order. Initially, it was pointed out that the assumption of instantaneous adjustment rendered the tests for the first two necessary conditions biased towards showing inefficient allocation. This bias is of concern only with respect to the occupational findings, for on an industrial and regional basis both of the initial two conditions were satisfied. Secondly, it is conceded (n. 8) that by assuming (1) that adjustment lags are not the result of the presence of noneconomic barriers, and (2) certain patterns of demand shifts (of a reinforcing character in the direction in which wage rates are moving), it is possible to argue that a failure to satisfy either the third or fourth conditions does not necessarily imply inefficient allocation. This objection could conceivably apply to the findings on an industrial basis. However, it is rejected on two grounds: (1) that it is not clear that adjustment lags are not the result of noneconomic barriers; and (2), more important, the empirical evidence for the period does not support the possibility of demand shifts accounting for the increased dispersion of industrial wages. If the demand-shift contention were correct, relative increases in the importance of the sectors showing large relative wage gains and relative decreases in the sectors showing relative wage losses would be expected. Using income generated by sector data (Department of Commerce) to estimate relative increases in importance (measured by the ratio of the percentage increase in income originating in a sector to the percentage increase in national income) and comparing the rank order of increases (decreases) in relative importance with the rank order of relative wage gains (losses) produces a rank-order correlation of —.17, which is not significantly different from zero at the .05 level. Consequently, the demand-shift possibility is rejected as an explanation of the change in the dispersion of industrial wage rates.

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COMMUNICATIONS

Optimum Currency Areas

In a recent note [4], Robert A. Mundell has suggested that little in the way of a systematic attempt has been made to define the characteristics of an area over which it is optimal to have a single currency regime, or—what is almost the same thing—a fixed exchange-rate system with guaranteed convertibility of currencies. The extensive literature on the relative merits of fixed versus flexible exchange rates has been rendered somewhat sterile by this omission. Existing national boundaries have been implicitly used to define the single currency area to which flexible external exchange rates would or would not be applied. However, when different possibilities for the grouping of nations in single currency areas exist, as in the EEC, or when resource mobility is low within individual countries, Mundell demonstrates that it is necessary to ask what economic characteristics determine the optimum size of the domain of a single currency. I shall develop the idea of optimality further by discussing the influence of the openness of the economy, i.e., the ratio of tradable to non-tradable goods, on the problem of reconciling external and internal balance, emphasizing the need for internal price-level stability.

"Optimum" is used here to describe a single currency area within which monetary-fiscal policy and flexible external exchange rates can be used to give the best resolution of three (sometimes conflicting) objectives: (1) the maintenance of full employment; (2) the maintenance of balanced international payments; (3) the maintenance of a stable internal average price level. Objective (3) assumes that any capitalist economy requires a stable-valued liquid currency to insure efficient resource allocation. Possible conflicts between (1) and (2) have been well discussed in the literature, especially by J. E. Meade [3], but joint consideration of all three is not usually done. For example, J. L. Stein [6] explicitly assumes internal price-level stability in his discussion of optimal flexibility in the foreign exchange rate. The inclusion of objective (3) makes the problem as much a part of monetary theory as of international trade theory. The idea of optimality, then, is complex and difficult to quantify precisely, so what follows does not presume to be a logically complete model.

"The ratio of tradable to non-tradable goods" is a simplifying concept which assumes all goods can be classified into those that could enter into foreign trade and those that do not because transportation is not feasible for them. A physical description of both tradable and non-tradable goods would correspond to that given by R. F. Harrod [1, pp. 53-56]. This overly sharp distinction between classes of tradable and non-tradable goods is an analytically simple way of taking transportation costs into account. By tradable goods we mean: (1) exportables, which are those goods produced domestically and, in part, exported; (2) importables, which are both produced domestically and imported. The excess of exportables produced over exports will depend directly on the amount of domestic consumption, which is likely to be small when exportable production is heavily specialized in few goods. Similarly, the excess of importables consumed over imports will depend on the specialized nature of imports. Therefore, the value of exportables produced need not be the same as the value of importables consumed, even in the case of balanced trade where the values of imports and exports are equal. However, the total value of tradable goods produced will equal the value of tradable goods consumed under balanced trade. Thus, the expression "the ratio of tradable to non-tradable goods" can apply unambiguously to production or consumption.

I. *A Simple Model*

Ideally, one would like to consider a large group of countries jointly and then decide how they should be divided up into optimum currency regions. The analytical framework for such a task does not exist, so it is necessary to consider a much narrower problem and hope it throws light on the general one—besides being of interest in itself. Consider a well-defined single currency area in which we wish to determine whether or not there should be flexible exchange rates with the outside world. The outside world is itself assumed to be a single currency area which is very large.

If the area under consideration is sufficiently small, we may assume that the money prices of the tradable goods in terms of the outside currency are not influenced by domestic exchange rates or domestic currency prices.[1] In actual practice, the domestic money prices of tradable goods will be more closely tied to foreign prices through existing exchange rates than will the domestic money prices of the non-tradable goods. Under this invariance assumption, i.e., fixed foreign-currency prices, the terms of trade will necessarily be immune to domestic economic policy. Some justification is given for this in R. Hinshaw [2], even for fairly large countries. We now inquire into whether external exchange-rate flexibility or internal fiscal-monetary expansion or contraction is most suitable to maintaining external balance, i.e. shifting production and expenditures between the tradable and non-tradable goods.

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[1] If we apply this assumption to the standard elasticities model, then both the elasticity of foreign demand for home exports η_f and the elasticity of foreign supply of home imports ϵ_f, are assumed infinite. Thus a devaluation, i.e., a rise in the foreign exchange rate k, would always improve the trade balance, B, by an amount proportional to the sum of the home elasticity of demand for imports and the home elasticity of supply of exports, η_h and ϵ_h respectively, i.e.,

$$\frac{dB}{dk} = Z(\epsilon_h + \eta_h)$$

where Z is the value of exports in the case of balanced trade. The trouble with this standard model is that η_h and ϵ_h depend on the amount of domestic absorption permitted in the course of devaluation as well as the openness of the economy; and it is difficult to make explicit what internal price repercussions may occur since the body of non-tradable goods does not enter explicitly in the model. Assuming both η_f and ϵ_f to be infinite is different from the usual simplification that both supply elasticities, ϵ_f and ϵ_h, are infinite, and in my opinion is more appropriate to the consideration of most small areas.

Case 1

Suppose exportables X_1 and importables X_2 together make up a large percentage of the goods consumed domestically. Suppose further a flexible exchange-rate system is used to maintain external balance. The price of the non-tradable good, X_3, is kept constant in terms of the domestic currency. Exchange-rate changes will vary the domestic prices of X_1 and X_2 directly by the amount of the change. Thus, if the domestic currency is devalued 10 per cent, the domestic money prices of X_1 and X_2 will rise by 10 per cent and thus rise 10 per cent relative to X_3. The rationale of such a policy is that the production of X_1 and X_2 should increase, and the consumption of X_1 and X_2 should decline, improving the balance of payments. Direct absorption reduction from the price rise in tradable goods may have to be supplemented by deliberate contractionary monetary-fiscal policy, if unemployment is small. Substantial theoretical justification for considering relative price changes between tradable and non-tradable goods to be more important than changes in the terms of trade for external balance is given by I. F. Pearce [5].

From Case 1, it is clear that external exchange-rate fluctuations, responding to shifts in the demand for imports or exports, are not compatible with internal price-level stability for a highly open economy, objective (3). In addition, such a policy by itself may not succeed in changing relative prices or affecting the trade balance. In a highly open economy operating close to full employment, significant improvement in the trade balance will have to be accomplished via the reduction of domestic absorption, i.e., real expenditures, which is the only possible way of keeping the price of X_3 constant in terms of the domestic currency. Thus, a substantial rise in domestic taxes may be necessary whether or not there is any exchange rate change. In the extreme case where the economy is completely open, i.e., all goods produced and consumed are tradable with prices determined in the outside world, the only way the trade balance can be improved is by lowering domestic expenditures while maintaining output levels. Changes in the exchange rate will necessarily be completely offset by internal price-level repercussions with no improvement in the trade balance.

To restate the core of the argument: if we move across the spectrum from closed to open economies, flexible exchange rates become both less effective as a control device for external balance and more damaging to internal price-level stability. In fact, if one were worried about unwanted speculative movements in a floating exchange rate in Case 1 of an open economy, a policy of completely fixed exchange rates (or common currency ties with the outside world) would be optimal. Blunt monetary and fiscal weapons which evenly reduced expenditures in all sectors could be counted on to improve immediately the trade balance by releasing goods from domestic consumption in the large tradable-goods sector. Exportables previously consumed domestically would be released for export; imports would be directly curtailed, and domestically produced importables made available for substitution with imports. The reduction of expenditures in the relatively small non-tradable-goods sector would initially only cause unemployment which, depending on the degree of interindustry resource mobility and price flexibility, might eventually be translated into more production in the tradable-goods sectors, and possibly improve the trade balance in the longer run. The smaller this non-tradable-goods sector, the smaller will be the immediate impact of reducing expenditures on employment and total production, and thus the more efficient this policy of expenditure reduction will be as a device for improving external balance (the surplus of production over expenditures).

Any region within a common currency area faced with a loss of demand for its products will be forced to cut its expenditures through a loss of bank reserves and regional income, thus eventually correcting the trade balance. A separate currency region with fixed exchange rates may have to carry out the cutback of expenditures more through deliberate policy if bank reserve losses are effectively sterilized. In either case, the immediate reduction in real income cannot be avoided if the trade balance is to be improved.

Case 2

Suppose the production of non-tradable goods is very large compared to importables and exportables in the given area. Here the optimal currency arrangements may be to peg the domestic currency to the body of non-tradable goods, i.e., to fix the domestic currency price of X_3 and change the domestic price of the tradable goods by altering the exchange rate to improve the trade balance. A currency devaluation of 10 per cent would cause the domestic prices of X_1 and X_2 to rise by 10 per cent, but the effect on the general domestic price index is much less than in Case 1.

The desired effect of the relative price increase in the tradable goods is to stimulate the production of tradable compared to non-tradable goods and thus improve the trade balance. On the other hand, if monetary-fiscal policy is primarily relied on to reduce domestic demand to maintain external balance, unemployment will be much higher. Much of the immediate impact of the reduction of expenditures will be in the extensive non-tradable-goods industries. If there are any rigidities in resource mobility, the trade balance will not improve much in the first instance. Through this policy, it may be actually necessary to achieve a fall in the domestic money prices of X_3, the numerous non-tradable goods, before sufficient expansion in the production of X_1 and X_2 can be obtained. Since a major component of X_3 will be labor services, it may be necessary to lower wage costs vis-a-vis the domestic money prices of X_1 and X_2, which are fixed by the inflexible external exchange-rate system. Such a policy would contain all the well-known Keynesian difficulties of getting labor to accept a cut in money wages. In addition, a successful policy of lowering prices of the numerous X_3 goods would have a large impact on the average domestic price level. Effectively, we would have permitted the tail (tradable goods) to wag the dog (non-tradable goods) in pursuing restrictive monetary and fiscal policies, with fixed exchange rates to improve the trade balance, for a small proportion of tradable goods.

Our open economy of Case 1 somewhat resembles what Stein [6] has called a "conflict" economy. In a conflict economy, export production is sufficiently large to dominate the generation of domestic income, and thus fluctuations in both are positively correlated. Therefore, with a fixed exchange rate, periods having low income will also have unfavorable trade balances, and vice versa. For income stabilization, objective (1), Stein concludes that a floating exchange rate will be optimal for a conflict economy in a Keynesian environment. The foreign exchange rate would then rise at the top of the cycle and fall at the bottom. These exchange rate changes will stimulate domestic production and income at the bottom of the cycle and damp them at the top. But it is precisely in this case of a highly open economy that exchange-rate changes will mean great fluctuations in internal price levels—sufficiently great, that any effects of exchange rate changes on domestic production may be small. However, there may still remain a direct policy conflict between objectives (1) and (3) in the use of a floating exchange rate. Certainly, the liquidity value of the domestic currency will depend directly on the short-run fortunes of the export commodity(ies) for a floating exchange rate.

Qualifications

The sharp distinction between tradable and non-tradable goods makes the above model analytically much easier to work with; but in practice there is a continuum of goods between the tradable and non-tradable extremes. The relaxation of this sharp distinction does not invalidate the basic idea of the openness of the economy affecting optimum economic policies; but the empirical measurement of the ratio of tradable to non-tradable goods becomes more difficult. Some kind of weighting system for determining the total production in each category might be possible. Certainly, knowledge of total imports and exports would give one a good lead in determining total production of exportables and importables. In addition, the idea of openness would have to be modified when the area was large enough to affect external prices.

II. *Monetary Implications of the Model*

The above discussion has been concerned with the way by which relative price changes in tradable and non-tradable goods can be brought about, and the conditions under which monetary and fiscal policy can be used efficiently to maintain external balance. Minimizing the real cost of adjustments needed to preserve external balance hinged to a large extent on minimizing necessary fluctuations in the over-all domestic price level. Thus the argument is very much concerned with the liquidity properties of money, and it is worth while to look at some of the more general monetary implications of the model. Suppose X_1, X_2, and X_3 are classes of goods rather than single goods as in the Pearce model. One of the aims of monetary policy is to set up a stable kind of money whose value in terms of a representative bundle of economic goods remains more stable than any single physical good. Indeed, it is the maintenance of this stable value which gives money its liquidity properties. The process of saving and capital accumulation in a capitalist system is greatly hampered unless a suitable *numéraire* and store of value exists. It may be still more difficult if a more desirable money is available from another source, e.g., from a larger currency area. This latter possibility is discussed below.

If the area under consideration is sufficiently large so that the body of non-tradable goods is large, then pegging the value of the domestic currency to this body of non-tradable goods is sufficient to give money liquidity value in the eyes of the inhabitants of the area in question. It may not be sufficient from the viewpoint of potential investors in the outside world. However, if the area is large, what outside investors think need not be an overriding consideration. Efficient internal capital accumulation and full employment are more important than external capital movements. If, under these circumstances, trade patterns are so unstable that substantial relative price changes in tradable and non-tradable goods are required to maintain external balance and full employment, then flexible external exchange rates may well be optimal. Resulting internal price changes will not destroy the value of the domestic currency as money.

If the area under consideration is small so that the ratio of tradable to non-tradable goods is large and the prices of the former are fairly well fixed in the outside currency, then the monetary implications of pegging the domestic currency to the non-tradable goods are less satisfactory. Such a class of non-tradable goods may not constitute a typical bundle of economic goods. The class of importables may be more representative, and a currency pegged to maintain its value in terms of importables into a small area may have a higher liquidity value than one pegged to the domestically produced non-tradable goods. However, pegging a currency of a small area to maintain its value in terms of a representative bundle of imports from a large outside area is virtually the same thing as pegging it to the outside currency. Alternatively, if we have a number of small areas which trade extensively with each other, and each pegs its currency to a representative bundle of imports, then each currency will be pegged to the others. To maintain the liquidity value of individual currencies for small areas, a fixed exchange-rate system is necessary. In addition, capital movements among small areas are more needed to promote efficient economic specialization and growth than free capital movements among large, economically developed areas. Contractual arrangements for such movements are greatly facilitated by a common currency. These arguments give us some insight into why each of the fifty states in the United States could not efficiently issue its own currency, aside from the inconvenience of money changing.

If we have a small area whose currency is not convincingly pegged in terms of the currency of a larger area, and so on this account its liquidity value is less, then domestic nationals will attempt to accumulate foreign bank balances. This will occur even though the marginal efficiency of investment in the small area is greater than that outside. As long as the functions of savings and investment are specialized, savers will attempt to accumulate cash balances in the more liquid currency. The illiquidity of domestic currency may also reflect monetary mismanagement as well as small size. In either case, we would expect small countries with weak currencies to have a tendency to finance the balance-of-payments deficits of larger countries with more desirable currencies. Thus, we have capital outflows from countries where the need for capital may be rather high and which arise from "monetary" rather than "real" considerations. Authorities in such countries are generally forced to maintain rather strict exchange controls unless the currency can be pegged in a convincing fashion to that of the larger area.

The above argument is relevant to the use of uncontrolled floating exchange rates. This device of maintaining external balance will only work well when the currency in question has liquidity value of the same order as that of the outside world—or the world's major currencies. This condition was approximately satisfied in the case of the Canadian dollar up to 1961. However, a floating exchange rate for the Korean yen may lead to less satisfactory results. If the official rate were made equal to the black-market rate and there were no further exchange restrictions, there would still be a capital flight out of Korea into currencies with superior liquidity value, aside from problems of political stability. A floating exchange rate in itself is not a sufficient control device and does not necessarily eliminate the need for exchange controls.

By contrast, short-term capital flows among currencies of approximately equal liquidity value are less likely with a floating exchange rate because of the exchange risk and the liquidity equivalence. The possibility of carrying out different degrees of easy or tight monetary policy in different countries is greater as capital flows would not be so responsive to interest-rate differentials. Once the world is divided into a number of optimal-sized currency areas permitting efficient internal capital accumulation, the desirability of short-term capital flows among areas well developed economically becomes less great, and it becomes desirable to insulate the monetary policies of the areas from each other in order that monetary policy may be used more freely to support full employment. However, it does not make any sense to advocate a floating exchange-rate system without first defining the optimal domains of individual currencies.

Suppose we look at the problem of a depressed subregion of a common currency area. Consider the case of West Virginia where non-tradable goods are largely labor services. We have an illustration of an excess supply of non-tradable goods and an excess demand for the tradable goods because of internal price rigidities. Thus, in this sense West Virginia has an *ex ante* balance-of-payments deficit even though in an *ex post* accounting sense there is a balance-of-cash flow in and out of the state. Would the adjustment of external balance and internal full employment be facilitated if West Virginia were incorporated as a country with its own currency? To the extent that the ratio of tradable to non-tradable goods was high, such a monetary system would have little chance of success. A devaluation would be associated with a large domestic price-level increase and hence money illusion would not be much help in getting labor to accept a cut in real wages [4, p. 663]. Labor unions would still continue to bargain in terms of U.S. dollars. In addition, a West Virginian currency tied to a representative bundle of non-tradable goods would not be an entirely acceptable store of value. There undoubtedly would be attempts by West Virginians to accumulate U.S. bank balances. However, if the depressed area were substantially larger, with a small proportion of production in tradable goods, a separate monetary system might be preferable as a device for maintaining full employment and external balance in the absence of factor mobility.

III. *A Concluding Note on Factor Mobility*

The idea of factor mobility has two distinct senses: (1) geographic factor mobility among regions; (2) factor mobility among industries. I think it is fair to say that Mundell [4] had interpretation (1) primarily in mind. His discussion of optimum currency areas in large measure is aimed toward having high geographic factor mobility within each single currency area and using flexible external exchange rates to make up for the lack of factor mobility among areas. Thus, for a given amount of geographic factor mobility in the world, this method of division into currency areas would maximize the possibility of world income and employment, subject to the constraint of maintaining external balance. Of course, the currency arrangements themselves would affect factor mobility, so the extent of factor mobility has to be considered *ex post.* Once we consider problems of factor immobility among industries, it may not be feasible to consider slicing the world into currency areas along industrial groupings rather than geographical groupings. However, from our above discussion, an optimal geographic size still exists even when we are only concerned with interindustry factor immobility.

Consider the special but perhaps common case of factor immobility between regions, each with its own specialized industries, the case where it is difficult to distinguish geographical and interindustrial immobility. Suppose there is a rise in the demand for the products of region A and a decline in the demand for goods of region B. The value of the marginal products of the potentially mobile factors of production in region B in B-type industries will fall, and rise in region A in A-type industries. Now if the possibility of developing or extending A-type industries in B is feasible, then need for factor movement between A and B is not great. The existing immobility between regions can be accepted through monetary arrangements giving both regions their own currencies, thus permitting more flexibility in enabling each area to pursue monetary and fiscal policies geared to internal stability. But if B cannot easily develop A-type industries, then factor movements to A may be the only thing that will prevent a large fall in the unit incomes of potentially mobile factors of production in B. So a policy aimed directly at overcoming the immobility of factor movements between A and B may be optimal, and perhaps the two should be joined in a common currency area. This argument becomes stronger when one considers small areas trying to develop industries in which economies of scale or indivisibilities are very great instead of efficiently moving factors elsewhere.

In a world where trade patterns are not perfectly stable, there will always be the problem of changing the world pattern of resource use among various industries to preserve external balance, full employment, and efficient resource use. In the simple model given in Section I above, we considered the optimum extent of a currency area in terms of its size and structure, i.e., the ratio of tradable to non-tradable goods, in promoting shifts in resources among various industries. The model accepted the degree of internal resource immobility among industries as an obstacle to be overcome as smoothly as possible. The arguments given there for applying flexible exchange rates to optimal-sized currency areas to efficiently overcome factor immobility hold in the main, whether the degree of internal mobility among industries is large or small. Such factor immobility among industries is a painful fact of economic life which has to be overcome as efficiently as possible. However, this criterion of size and openness of a single-currency economy in facilitating interindustry production shifts certainly has to be balanced with purely geographic factor-mobility considerations in determining the optimum extent of a currency area.

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Inventing and Maximizing

Cross-section studies over the period roughly 1900–50 show that patented inventions that improve capital goods tend to be distributed among U.S. industries in proportion to the industries' value-added [5]. The discoverers of this relation held that, in addition to other factors, this result was consistent with maximizing behavior on the part of inventors, since sales of capital goods tend to vary directly with the value added by the industries using those capital goods, and since the prospective net revenues from capital-goods-improving inventions would tend to vary directly with sales of the class of capital goods involved. However, it was pointed out in [2] that (a) other possible size-correlated determinants might explain the correlation, and (b) the influence, if any, of maximizing behavior on the industrial distribution of inventive activity would be more clearly revealed if the effects of industry size were suppressed. While objection (a) cannot now be met, this note attempts to follow up suggestion (b).

I

There are several ways of eliminating the effect of industry size. One way is to combine all the cross sections reported on in [5] and re-estimate the relationship, allowing each industry to have a constant term of its own.

This assumes that the slope of the relationship between value-added and patents is the same for all industries and for the period as a whole, but that its level may differ between industries. This procedure allows the estimation of the *partial* regression coefficient of patents on value-added, holding the mean level of these variables constant for *each* of the component industries. This procedure is equivalent to correlating the deviations of patents and value-added from their respective industry means (in [5] the correlations were computed for deviations from the *over-all* mean rather than for deviations from industry means, including thereby mean industry size as one of the determinants of the computed relationship). When this procedure is followed using the data presented in Tables 3 and 4 of [5],[1] the resulting regression equation is:

$$(1) \qquad P_{ti} = \text{(a set of 14 constants)} + \underset{(.140)}{1.287} V_{t-3,i}; \qquad R^2 = .84$$

where P is patents, V is value-added, t is time, and i the industry. The number in parentheses is the standard error of the coefficient.[2]

The high coefficient of determination, R^2, could be due, however, not to the influence of value-added on patenting three years later, but either to common trends in both variables or to the influence of general business-cycle developments on both variables. To assess these possibilities, a trend factor, T (the last two digits of the year in question), and total gross investment in producers' durable equipment (in 1954 prices)[3] were introduced into the analysis, yielding:

$$(2) \qquad P_{ti} = \text{(a set of 14 constants)} + \underset{(.182)}{1.393} V_{t-3,i} + \underset{(.005)}{.008} I_{t-3} - \underset{(1.5)}{2.8} T_t; \qquad R^2 = .84$$

Thus the introduction of an economy-wide investment variable and a trend factor fails to change the correlation, the influence of value-added on patenting is undiminished, and the standard errors of the new variables are so high that their coefficients are statistically insignificant.

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[1] The source tables provide data for 18 industries for 15 specific years over the interval of 1902-49. To maximize the number of industries with relatively long series used in the following analysis, flat glass and "all other glass" were assigned to the glass industry, and seamless hosiery and "all other knitting" were assigned to the knitting industry. In consequence the data used below relate to 14 industries. The data for 1949 were not used because of the abrupt apparent decline in the patenting of inventions in the post-World War II era, a development, which would make the use of pre- and post-World War II patent statistics in a single cross section inadvisable without some kind of special adjustment. Since data were not available for all years for some industries, the following analyses cover 164 observations, or about 12 observations per industry.

[2] In principle the constants would have independent interest. But the unknown magnitude of interindustry differences in (a) the proportion of inventions patented and (b) the proportion of an industry's patents actually included in the data presented in Table 4 of [5] makes their presentation here pointless, since they could not be interpreted.

[3] Data for this variable are the same as those that underlie the calculations reported by R. M. Solow in "Technical Progress, Capital Formation, and Economic Growth," *Am. Econ. Rev., Proc.,* May 1962, *52,* 76-86.

However, the use of absolute deviations in the foregoing fails to eliminate the effects of industry size entirely: large industries tend to have larger deviations from their means than do small ones. Accordingly, regressions were run using logarithms of all the variables except T, with results as follows:

$$(3)\quad \ln P_{ti} = (\text{a set of constants}) + \underset{(.090)}{.487} \ln V_{t-3,i}; \qquad R^2 = .88$$

$$(4)\quad \ln P_{ti} = (\text{a set of constants}) + \underset{(.123)}{1.154} \ln V_{t-3,i} - \underset{(.12)}{.05} \ln I_{t-3} - \underset{(.006)}{.040} T_t; \qquad R^2 = .91$$

Thus, in both cases the relative deviations are somewhat more highly correlated than are the absolute ones. There is also an indication in equation (4) of a statistically significant downward trend in patenting of about four per cent per year when value-added is held constant. Since the coefficient of ln V_{t-3} is not significantly different from unity, there is no evidence for a significant "scale" effect, in the sense that a 10 per cent increase in value-added, holding other things constant, would result in a substantially larger or smaller increase in the rate of patenting.

In general, the results of all four regressions indicate that the relationship between value-added and patents is statistically significant, the t ratios exceeding 5, even after we allow each industry to have its own mean level. This relationship holds in both linear and logarithmic form. It is not a product of common business-cycle developments (or at least not with a lag of three years), the coefficients of I (economy-wide investment) never being statistically significant.

II

The preceding analysis, like that in [5], suffers from the deficiency that the presumptive proximate determinant, investment, is represented only by proxy. However, the data on investment expenditures by industry presented by the Censuses of Manufacturers for 1939 and 1947 can be directly compared with patenting. (For these comparisons we use, as in the preceding analysis, successful patents filed in the three years after the year of investment.) Moreover, by reducing all data to a per worker basis, we have still another way of eliminating the effects of industry size. The data, shown in Table 1, cover 21 industries for 1939 and 23 for 1947.[4]

Regressions were run in both linear and logarithmic form for each year and for both years combined. We present only the logarithmic results (the linear ones are very similar):

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[4] While investment data for later years exist, usable patent statistics classified by industry for the post-1950 period have not been compiled. Hence the comparisons which follow are the only ones presently possible.

TABLE 1.—INVESTMENT, EMPLOYMENT, AND INVENTION, SELECTED INDUSTRIES, 1939 and 1947

Industry	Avg. No. of Employees		Estimated Investment in Plant and Equipment		Successful Patent Applications	
	1939*	1947	1939**	1947	1940–42	1948–50
Sugar	28.76	35.42	7.451	23.483	93	67
Tobacco	87.53	111.80	8.475	35.645	136[a]	42[b]
Knitting[d]	172.80	147.70	22.077	55.252	288	343
Narrow fabrics	23.63	27.66	2.193	5.670	29	26
Hats	43.32	49.27	2.406	4.791	20	18
Linoleum	7.03	10.06	1.871	8.708	22	15
Other carpet and rug	28.73	47.15	3.394	12.782	39	63
Cordage twine	12.10	15.95	1.020	3.863	11	17
Yarn & thread[d]	350.40	502.80	34.383	139.542	562	761
All other textiles[d]	460.80	450.70	42.734	138.247	886	1097
Lumber and timber prods.	360.60		47.490		423	
Veneer mills		10.46		3.144		20
Cooperage		21.51		1.376		2
All other lumber & wood prods excl. furniture		652.00		177.817		620
Pulp, paperboard, & pulp goods	138.20	200.10	51.153	291.572	264	308
Envelopes	8.69	13.80	1.491	5.338	17	15
Paper bags	11.08	22.27	2.231	9.189	49	27
Paper boxes	62.53	109.80	13.104	47.274	162	146
All other paper	44.20	126.10	7.383	62.595	113	127
Petrol. refining	72.74	145.80	113.431	316.370	1089	663
Footwear excl. rubber	236.90	262.40	9.367	15.265	598[a]	486[b]
Glass	79.83	116.50	17.023	67.112	441[a]	390[c]
Stone & clay prods.	207.70	345.50	50.740	217.954	392	476
Synthetic fibers	48.33	71.88	18.545	67.953	522	345

Employees in thousands; investment in millions of current dollars; patents—actual numbers.

* Average number of wage earners in 1939.

** Reported investment multiplied by the ratio of total value-added in the industry to the value-added in establishments reporting investment in plant and equipment.

[a] Patents granted in 1942–44.

[b] Six times the number of patents granted, first six months of 1949.

[c] Total granted, 1950–52.

[d] Because of the large amount of yarn and thread produced in integrated mills, the figures given by the census for yarn and thread were increased by a factor of three, an expansion suggested by examination of the data in Appendix C, Vol. I. (For example, only 21 per cent of the 1947 shipments of yarn produced under the cotton system originated in mills of which yarn was the primary product.) The excess of the number of workers and volume of investment shown here for yarn and thread over that derived directly from the census volume was deducted from knitting and all other textiles in proportions given by the ratios of the respective variables to each other in these two industries.

$$\ln p_{1940-42} = -3.11 + \underset{(.148)}{.771} \ln i_{1939}; \qquad R^2 = .59$$

$$\ln p_{1948-50} = -3.85 + \underset{(.158)}{.691} \ln i_{1947}; \qquad R^2 = .59$$

$$\ln p_{\text{combined}} = -2.86 + \underset{(.109)}{.722} \ln i - \underset{(.20)}{1.17} D; \qquad R^2 = .58$$

where p and i are capital-goods patents and total investment in plant and equipment respectively, each in per worker units, and D is a variable which equals zero for all 1939 observations and unity for the 1947 ones.

An attempt to find the effects of plant and equipment investment separately by adding investment in equipment as another variable proved unsuccessful, indicating that the coefficients of both types of investment are roughly equal.

The correlation coefficients are all substantial, a result consistent with the Brownlee-Schmookler hypothesis that a high volume of capital-goods sales in a field is taken as a favorable augury that invention in it will be profitable.

In any event, the evidence discussed here does indicate, as argued in [1] [3] [4] [5], and undoubtedly elsewhere, that, at least for those inventive efforts pursued to the point of securing a patentable result, economic influences operating via the demand side are usually strong and perhaps paramount.

ZVI GRILICHES AND JACOB SCHMOOKLER*

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Economic Forecasting When the Subject of the Forecast Is Influenced by the Forecast: Comment

In a recent paper [4], Kemp discussed the question of self-validation of forecasts when the subject of forecast is influenced by the forecasts themselves. His interpretation of the term "self-validating" is based on the criterion of numerical accuracy; to qualify for this epithet, a forecast must result in a smaller absolute forecasting error when it is announced than when it is kept secret. Suppose an official forecast is that income will rise to $500 billion. Further suppose that, with the forecast kept secret, income will actually be $490 billion, but with the forecast made public, income will reach $520 billion instead. In this example Kemp would conclude that the forecast is "self-destructive," because the error when the forecast is made public ($=520-500$) exceeds the error when the forecast is kept secret ($=500-490$).

However, this term can also be interpreted alternatively in a *directional* sense. An official forecast may be considered "self-validating" as long as its publication causes the variable in question to move in the direction of the forecasted change, regardless of the magnitude of forecasting errors. In the example above, since the forecast of an increase does cause the variable to assume a larger value than it would in the absence of the forecast, we may indeed consider the forecast as having validated itself. To avoid confusion, let us refer to this latter sense of validation as "self-reinforcement."

Short of taking a poll, it would be difficult to say which interpretation is the more widely held. It appears, however, that a stronger case can be made for the directional than for the numerical interpretation, because when forecasts influence economic behavior they may serve as an instrument of control, and in such instances, as Kemp recognized [4, p. 496], the numerical accuracy of forecasts loses its relevance, and the reinforcement effect takes on the major significance. But at any rate, as long as the possibility exists that those who claim the never-failing self-validation of official forecasts may actually have the directional interpretation in mind, Kemp could have missed the whole point, or at best have only dealt with part of the question. This paper seeks to fill the gap by examining the reinforcement aspect of that claim.

I. *The Simple Multiplier-Accelerator Model*

In order to facilitate the comparison between the analyses of self-reinforcement and self-validation, we shall start with Kemp's version of the multiplier-accelerator model.

But first, let us define a self-reinforcement index as:

$$R_t = (y_{ta} - y_{ts})/(y_t^* - y_{t-1}), \tag{1}$$

where y is income, t is time period, the subscripts a and s represent "with forecast announced" and "with forecast kept secret" respectively, and the asterisked symbol signifies the official forecast. Obviously, a positive R_t would indicate that the forecast is self-reinforcing, and a negative R_t, the forecast is self-nullifying.

The accelerator mechanism in the model operates on the discrepancy between the expected income and the current income. Kemp's assumptions are (i) that the business community always accepts the official forecast with complete faith, and (ii) that in the absence of a published official forecast, the business community will passively expect current income to persist in the ensuing period—i.e., income is expected to be static. Upon these assumptions (which we adopt to make possible a comparison of the two interpretations), we have

$$y_{ta} = \alpha + \beta y_{t-1} + \delta(y_t^* - y_{t-1}), \qquad 0 < \beta < 1; \quad \alpha, \delta > 0 \tag{2}$$

and

$$y_{tu} = \alpha + \beta y_{t-1}, \tag{3}$$

where β is the marginal propensity to consume, and δ the accelerator.

Substitution of (2) and (3) into (1) yields

$$R_t = \delta > 0. \tag{4}$$

Thus, although official forecasts may not always be self-validating in the numerical sense (as Kemp demonstrated), they nevertheless do always reinforce themselves in this simple model.[1]

This last conclusion amply demonstrates the need for distinguishing clearly between the concepts of self-reinforcement and self-validation. By itself, however, the result in (4) does not establish conclusively the claim that official forecasts are always self-reinforcing, because the two assumptions upon which it is based are severely restrictive. In reality, the business community may not always accept official forecasts unquestioningly, and, moreover, private predictions regarding the future may not simply be of the static variety. In the interest of greater realism, we must modify these assumptions to permit alternative situations.

II. *Incomplete Acceptance of Official Forecasts and Nonstatic Private Predictions*

The possibility of incomplete public acceptance of official forecasts can be shown in the model in various ways. We shall assume that businessmen will as a rule temper an official forecast with a private prediction of their own. Specifically, they will be assumed to take a weighted average of the official forecast and their private prediction in arriving at their final expectation of future income.[2] In symbols,

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[1] This result is different from Kemp's statement that ". . . if the Bank were content to forecast merely the *direction of change* of income the forecasts could not possibly be self-destructive" [4, pp. 493-94, italics his]. His statement implies that the forecast will only indicate "up" or "down," but our discussion deals with specific forecasts. More fundamentally, he was speaking of validation, but we are concerned with reinforcement.

$$y_{te} = wy_t^* + (1 - w)y_t^{**}, \qquad 0 < w \leq 1 \tag{5}$$

where y_{te} is the final income expectation, w is an "acceptance weight," and the symbols with the single asterisk and the double asterisk denote respectively the official forecast and the private prediction of income.

The assumption of the last section corresponds to the extreme case of $w=1$, but in this section we are primarily interested in the case $0<w<1$. The other extreme of $w=0$ is not included here because it implies the complete ignoring of the official forecast by the public, which would, contrary to the context of the present discussion, deprive official forecasts of their capacity to influence income level.

The possibility of nonstatic private predictions may also be allowed for in the model in different ways. For reasons of simplicity as well as plausibility, it will be assumed here that the business community believes in a sort of inertia in the short-term trend of income, and therefore tends habitually to extrapolate the current change into the future. That is,

$$y_t^{**} = y_{t-1} + p(y_{t-1} - y_{t-2}), \qquad p \geq 0 \tag{6}$$

where p is a "projection factor."[3] The simple model of the preceding section adopts the extreme value of $p=0$.

With the new behavioral patterns, we now write

$$y_{ta} = \alpha + \beta y_{t-1} + \delta(y_{te} - y_{t-1}), \tag{7}$$

and

$$y_{tu} = \alpha + \beta y_{t-1} + \delta(y_t^{**} - y_{t-1}), \tag{8}$$

and by utilizing (5) and (6), the self-reinforcement index is found to be

$$R_t = \delta w\left(1 - \frac{p\Delta y_{t-1}}{y_t^* - y_{t-1}}\right), \tag{9}$$

where $\Delta y_{t-1} = y_{t-1} - y_{t-2}$. This index now consists of three components: the accelerator δ, the acceptance weight w, and a parenthetical expression based on the projection factor p. The first one is economic, but the last two are in large measure psychological.

On the basis of the new model, a variety of possible results can be inferred. When $w=1$ and $p=0$, (9) will reduce to (4) and we then have the model of the last section. If $p=0$ but $w\neq 1$, then $R_t=\delta w$, and the value of the index may range from an infinitesimal positive quantity to δ, depending on the magnitude of w, but can never be zero or negative. This means that as long as private predictions are static, official forecasts are always self-reinforcing.

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[2] The weighted-average idea was also used, in different contexts from ours, by Devletoglou [1] and Grunberg and Modigliani [3]. However, the application by these authors differs from the present paper in that they average the official forecast and the current value of the forecast variable, whereas we average the official and private forecasts. The two averages will be identical only if the private forecast is that of "no change."

[3] The projection factor used here conforms to a type of expectation pattern envisaged by Metzler [5]; but in this paper we have excluded from consideration any negative values for this factor. It is interesting to note that although this type of "extrapolative" expectation has usually been interpreted as the embodiment of a *mechanical* rule of behavior, Ferguson [2, pp. 95-96] has recently interpreted it to be the result of a type of *learning* process.

If p is positive, i.e., if private predictions are nonstatic, however, there will arise the possibility of self-nullifying official forecasts. The latter will occur when Δy_{t-1} and the quantity $(y_t^*-y_{t-1})$ have the same sign, and at the same time $|p\Delta y_{t-1}|>|y_t^*-y_{t-1}|$.

Note, however, that if this last inequality sign is reversed, the index will remain positive, and the effect of the p factor will then be not to nullify the forecast but only to reduce the reinforcing power from the level of δw. This is what will happen when the official forecast points to a greater income change (in the currently prevailing direction) than what is privately predicted. Secondly, if the quantity $(y_t^*-y_{t-1})$ is opposite in sign to Δy_{t-1}, then R_t will actually exceed δw, and consequently produce a greater degree of self-reinforcement. This is what will occur when the official forecast calls for a reversal of the direction of income change.

III. *Conclusion*

Within the framework of our analysis, the claim that official forecasts are always self-reinforcing will prove completely valid only on the assumption of static private predictions. When private predictions are nonstatic, self-nullification may result, although there is ample room even in this case for the self-reinforcement effect to manifest itself. A public forecast of reversal, for instance, will still be always self-reinforcing in that case; and a forecast of continuation of current change will prove self-reinforcing provided the magnitude of the forecasted change is a sufficiently bold one. These latter possibilities of self-reinforcement serve to suggest that it would be feasible, even in the (less favorable) case of nonstatic private predictions, to use official forecasts as a tool of control to influence the level of income. All that would be necessary is the proper adjustment of the direction and the magnitude of the forecast.

But what about the question that reliance on *contrived* forecasts may impair the public faith in them? This of course may very well happen; most tools can be expected to be blunted through use, and forecasts are no exception. In our model, the "blunting" will be reflected in the deterioration of the acceptance weight. But it is obvious that official forecasts will not completely forfeit their power to influence income unless the parameter w is reduced all the way to zero, which, in turn, will be unlikely unless contrived forecasts are employed so indiscriminately as to be beyond all reason.

Still, the above has dealt only with the question of feasibility, and feasibility is by no means a synonym for desirability. One may indeed wish to rule out artificial forecasts altogether on moral or other grounds. To consider such factors, however, would take us beyond the scope of this paper.

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Economic Forecasting When the Subject of the Forecast Is Influenced by the Forecast: Comment

In his recent communication [5], Mr. Kemp has set forth a number of conclusions, several of which require comment.

With his first conclusion that "the announcement of a forecast does not necessarily validate . . . the forecast" one could hardly quarrel unless one is prepared to assert that a public prediction could not conceivably affect behavior. Indeed, it is precisely the consideration that public announcement may *invalidate* an otherwise correct prediction that has long caused serious doubts about the possibility of correct public prediction of social events (cf. Morgenstern [6]) and prompted our earlier essay on this subject [3]. We do, however, have serious objections to his algebraic illustration purporting to show that "accurate [public] forecasting may be impossible." It seems in fact to provide a counterexample to our earlier proof, that, in general, correct public prediction is *not* impossible, at least in so far as correct private prediction is possible. Fortunately Kemp's illustration does not constitute a valid counterexample because the model which he uses violates one of the sufficient conditions we stated, namely that the variable to be predicted, in the present instance national income, y, should have a lower and upper bound.

That his model violates this condition can be verified from his equation (1). This equation, as stated, implies that, at a given point of time, national income can be made as large as we please, positive or negative, by the rather simple device of the Central Bank announcing an appropriately large positive or negative number and labeling this its forecast of national income. We presume that this is not the hypothesis that Kemp intended to formalize and that he instead conceived of his equations as providing a reasonable approximation in some reasonable neighborhood. Unfortunately Kemp's proof of the impossibility of correct forecasting does not rely at all on neighborhood properties. Actually he came close to noticing this point when he realized that his system might yield a negative solution for y. He understandably rebelled against this "absurd" result, and the rebellion is expressed in equation (9). But his unwillingness to put up with a negative value for the equilibrium level of income should have been built explicitly into his system

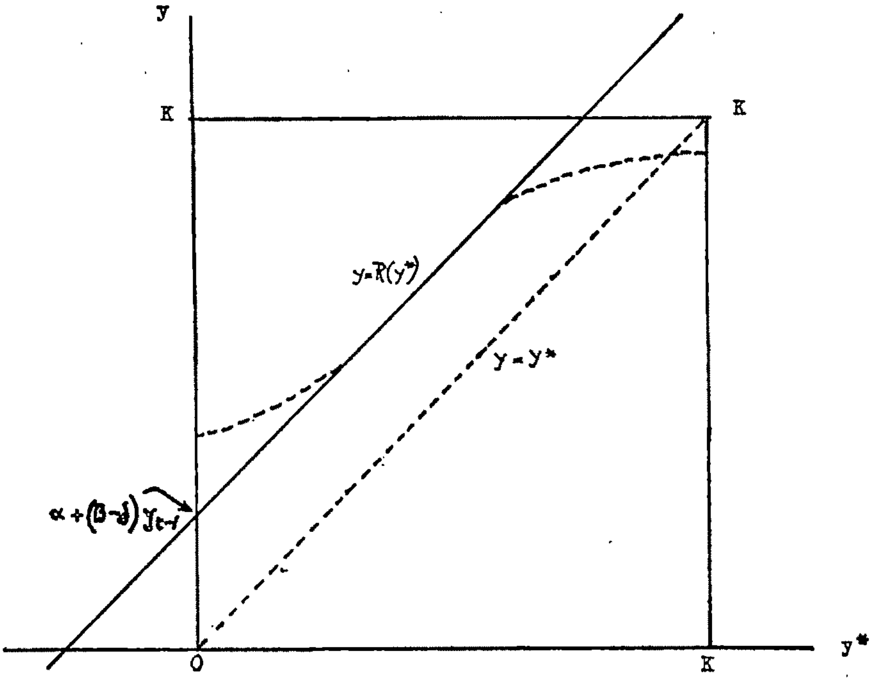

FIGURE 1

and not added as an afterthought. Furthermore, should he not be equally unwilling to tolerate values of y larger than a few trillions? If so, this contraint too should have been made explicit. Furthermore, he should have stated formally what happens to equation (1) for values of y^* implying values of y outside the economically acceptable range. But had he in fact done so, and provided the relation between y and y* implied by the appropriately modified equation (1) remained continuous, he would have found that his impossibility conclusion vanishes too.

It may be helpful to clarify this point in terms of the graphical apparatus we set forth in [3]. Let us measure the variable to be forecasted, y, on the ordinate and the public forecast, y*, on the abscissa and draw first the "requirement function," $y = y^*$, shown as a dotted line in Figure 1. Next, from equation (1), we can derive our "reaction function," $y = R(y^*)$. As shown in [3], the abscissa of the point (or points) where the requirement and the reaction functions intersect or touch yields a correct public prediction.

According to (1), the reaction function implied by Kemp's model can be written as $y_t=R(y^*_t)=[\alpha+(\beta-\delta)y_{t-1}]+\delta y^*_t$. Since at the given point of time the term in brackets can be taken as a constant, the reaction function is seen to reduce to a straight line with slope δ and an intercept positive, zero, or negative depending on whether $[\alpha+(\beta-\delta)y_{t-1}]\gtreqless 0$. It is exemplified in our figure by the solid line, for the case of positive intercept. It is obvious that if δ is unity this line is parallel to the requirement function and therefore can never intersect it. If the constant term happened to be zero—the "singular case" that Kemp decided to "put aside"—then the two lines would coincide and *any* forecast would turn out to be correct (and not merely a forecast of no change, as stated by Kemp). But except for this hair-line case, if equation (1) does in fact hold for all values of y^*—which means, of course, that y is regarded as having no lower or upper bound—then there is indeed no possible correct forecast. But once we recognize that there are limits to which the Central Bank can push income around by mere words, then (1) cannot hold throughout, and the locus of the reaction function must level off, both for sufficiently high and sufficiently low values of y^*. This is shown in our figure by the dotted curves branching off from the solid line. If one is prepared to read some economic content into Kemp's model, which he himself acknowledged as "no doubt . . . grossly unrealistic," one can associate this leveling-off with capacity limitations, bounds to the maximum rate of disinvenstment, monetary constraints, disbelief in "absurd" forecasts, etc. In any event, the leveling-off insures the existence of at least one point at which the requirement and the reaction functions intersect or touch, and hence at least one possible correct public prediction (unless again the new reaction function exhibits important discontinuities).

One other conclusion of Kemp which may be misleading, if not properly qualified, is his assertion that if the announced forecast related to "merely the *direction of change* . . . the forecasts could not possibly be self-destructive" [5, pp. 493–94, author's italics]. This conclusion is valid within his specific model, but it should not be regarded as having general validity. It can be shown—although it would hardly be worth while to take the space to prove this contention here—that the conclusion necessarily holds only under the following conditions: (1) the reaction function is monotonically nondecreasing; (2) the agents behave as though they believed fully the announced forecast; and (3) in the absence of a public forecast the anticipated value of the predictand is precisely the previous value. Kemp's model is seen to satisfy these three conditions, but his conclusion need not hold if one or more of these conditions are violated. In particular it may fail to hold if the reaction function is monotonically decreasing, as in the illustration provided in [3].

The above demonstration that correct public prediction is not impossible even under Kemp's model, once his model is modified to give it slightly more economic content, does not of course imply that the Central Bank would, in fact, have the knowledge required to make an accurate forecast. Nor does it throw any light on whether the level of y resulting from a correct prediction would be more desirable, in some welfare sense, than the level that would prevail in the absence of a public forecast or with an incorrect one. Kemp's analysis in part III throws some light on this question by showing that, for certain not "implausible" values of the parameters, correct public prediction may cause (or increase) instability over time. Similar results had already been reported by Develetoglou [2]. We have been aware of this possibility for quite some time but have hesitated to publish our results because—rightly or wrongly—we regard this result as too baffling to be taken seriously. Indeed, at least in the case of complete belief, this result would imply that perfect foresight may cause or increase oscillatory disturbances. We suspect that the reported mathematical results may arise from the fact that the macroeconomic models from which they are derived fail to take properly into account relevant aspects of economic behavior, in the same way that Kemp's demonstration of the impossibility of correct prediction turns out to reflect inadequate economic specifications of his model. In short, we suggest that before we can reach a sensible conclusion on this complex issue we need to learn to construct a macroeconomic model whose behavior equations are more directly tied to optimizing behavior of the individual units and embodying less preposterous assumptions about the nature of anticipations.[1] Furthermore, perfect foresight may have to be defined as perfect knowledge of the future *time path* of the relevant variables and not merely as knowledge of their standing at an arbitrarily chosen, nearby point of time.

Perhaps, if and when we succeed in carrying out the task outlined above, it will turn out that at least perfect foresight—and hence fully believed correct prediction—cannot increase economic instability, although at present this is no more than sheer speculation.

EMILE GRUNBERG AND FRANCO MODIGLIANI[*]

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[1] For some beginnings in this direction, see Holt and Modigliani [4] and the references cited therein, Muth [7], and Ando and Modigliani [1].

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Economic Forecasting When the Subject of the Forecast Is Influenced by the Forecast: Reply

A bad press is better than no press. I therefore hope that I shall not appear ungrateful if I try to deflect the heavier shafts of Messrs. Chiang, Grunberg and Modigliani.

Chiang argues correctly that "when forecasts influence economic behavior they may serve as an instrument of control, and [that] in such instances, . . . the numerical accuracy of forecasts loses its relevance." However he goes on to suggest that what *is* relevant in such instances is the self-reinforcing power of the forecast, that is, the power of the forecast to make the variable in question move in the direction of the forecasted change. But surely all that really matters, from the point of view of control, is the discrepancy between the desired or "target" value of the forecast variable and its actual or recorded value.[1] A forecast which is successful in terms of the above-mentioned discrepancy may be self-nullifying, self-reinforcing, or neither. Whether the forecast is self-reinforcing or self-nullifying is, in fact, quite irrelevant to the problem of economic control.

The Grunberg-Modigliani bark is terrifying. I am accused of sloppy logic, and my model is dismissed as both lacking in economic content and being too specific to yield conclusions of general validity. But what of the bite?

Consider first the offending logic. In my earlier paper I argued that "if, for whatever reason, income during period (t-1) stands at the 'Keynesian intersection,' that is, if

$$y_{t-1} = \frac{\alpha}{1-\beta}, \tag{1}$$

then, whether or not the forecast is announced, any forecast of no change [in income] will be perfectly accurate" [2, p. 494]. According to Grunberg and Modigliani, however, "*any* forecast would turn out to be correct (and not merely a forecast of no change, as indicated by Kemp)." Let us see. From equation (1) above, and from the first equation of my earlier paper,

$$y_t = \frac{\alpha(1-\delta)}{1-\beta} + \delta y_t^*.$$

It follows that any forecast will turn out to be correct ($y_t = y_t^*$ for all y_t^*) if and *only if* $\delta = 1$. But that is a highly special assumption indeed, one which I simply did not make in my earlier paper.

The second paragraph of my paper contains the following general disclaimer: "The model that I shall work with is of the simplest multiplier-accelerator type and, no doubt, is grossly unrealistic. But that matters little since, for the most part, I am concerned only to produce valid counter-

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[1] A possible measure of success is

$$\left|\frac{y_t^{**}}{y_t} - 1\right|$$

where y^{**} is the target value of y at time t and y_t is the recorded value.

examples to certain generalizations." It is galling then to be accused of putting forward conclusions "which may be misleading, if not properly qualified." Against one outcrop of the accusation of specificity, however, I am prepared to defend myself. Section III of my paper spells out some of the implications of perfectly accurate forecasting. In particular, it is shown that, if the public accepts the Bank's forecasts at face value, the announcement of accurate forecasts might convert an otherwise stable economy into a violently unstable one, with jagged, period-by-period oscillations of increasing amplitude. Grunberg and Modigliani find the possibility "too baffling to be taken seriously" and suggest that I do not "take seriously into account rationality in the formation of expectations." But if the forecaster is in the habit of making perfectly accurate forecasts, is it preposterous to suppose that the public acquires an equally perfect trust in him?

Grunberg and Modigliani have demonstrated that, if upper and lower bounds are imposed on the forecast variable, an accurate forecast is always possible. I have no quarrel with this result. Before writing my paper I had, of course, read their 1954 article [1] on the same subject, and I assumed that a general reference [2, p. 494, n.3] would serve as an acknowledgment of their theorem. Certainly at no point in my paper did I suggest that I had up my sleeve a valid counterexample. All that I wished to suggest in Section III of my paper was the possibility that within the limits of zero and capacity income an accurate announced forecast might be impossible.[3] Since Grunberg and Modigliani do not seek to impugn this conclusion, it is difficult to see what the shooting is all about.

Their footnote broadside against my use of the term "perfect accuracy" is equally puzzling. In particular, it is argued by Grunberg and Modigliani that " 'perfect accuracy' does not have [cannot be given?] a very precise meaning" and that "perfect accuracy does not exist [is never realized?] even in astrophysics." But surely it is one thing to say that "perfect accuracy" cannot be given a precise meaning and quite another to say that perfect accuracy is in practice never realized. One may accept the second judgment without committing oneself to the first.

Naturally, I do not deny the possibility of reworking my Section III in terms of the interval accuracy favored by Grunberg and Modigliani. That possibility had indeed been noted in the first section of my earlier paper.

In that first section I examined the impact of the announcement of a *given* forecast on the accuracy of the forecast. It should be noted, however, that if the Bank is aware that the announcement of the forecast will itself change the observed value of the forecast variable, the Bank will vary its forecast according as it is or is not to be announced. To isolate the conditions under which the announcement of a forecast will be self-validating, one must therefore know what forecasting model the Bank is using. I have examined the special case in which the Bank's model is of the simple multiplier-accelerator type assumed in my earlier paper—but with parameter values which possibly differ from those of the real world. Without reproducing the calculations in detail, I can report that the two main conclusions of the earlier paper emerge unscathed: the announcement of point forecasts may be either self-validating or self-destructive; the announcement of direction-of-change forecasts can never be self-destructive.

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[3] No doubt the possibility of negative incomes should have been recognized!

MURRAY C. KEMP*

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The Differential Effects of Tight Money: Comment

In a study which may well become a classic in the literature of monetary policy, Bach and Huizenga [1] present the results of a major empirical test of the often-alleged discriminatory impact of "tight money" policies. Their conclusion is that: "Widespread criticisms of tight money as unfairly discriminating against small borrowers, both in availability of loans and interest costs, are not supported by the data" (p. 79). The purpose of this note is to suggest an alternative interpretation of the data presented by Bach and Huizenga, an interpretation equally consistent with their data and one very suggestive to those who have wrestled with the knotty problem of "credit rationing" in the admittedly less-than-perfectly competitive money markets.

I. *The Bach-Huizenga Study*

Data presented by Bach and Huizenga (p. 65) show a highly consistent relationship between size of borrower and percentage increase in bank loans between October, 1955, and October, 1957, a period generally agreed to be one of monetary restraint. Uncritically examined, these raw data would seem to imply discriminatory treatment. But, the authors point out, the data equally support the conclusion that loan demand increased during this period much more for large borrowers than for small borrowers. They cite, with apparent approval, a Federal Reserve study [2] which concludes that: "The demand for bank credit rose much less rapidly at small businesses between 1955 and 1957 than at large businesses, and . . . this was the main apparent reason for the differential growth in lending" [1, p. 64].[1] The principal objective of the Bach-Huizenga study may be said to be to test this hypothesis that the differential growth in bank lending over this period was a consequence of changes on the demand side rather than of discrimination on the supply side.

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[1] The words quoted are those of Bach and Huizenga, and not the Federal Reserve's.

Their method is an ingenious one. They set about to distinguish, among individual banks, those most clearly restrained by monetary policy ("tight" banks), those least restrained ("loose" banks), and an intermediate group ("medium" banks). If the relative decline in loans to small borrowers had been just as great, they reasoned, at loose banks as at tight banks, some other factor than monetary restraint had to be the source of the apparent discriminatory impact.

There is not space in this brief note to review in detail the method employed by Bach and Huizenga in distinguishing tight, loose, and medium banks. Suffice it to say that the tight banks were those which were the tightest at the beginning of the period (had the lowest ratio of free reserves plus government bills and certificates to deposits) and which lost the most or gained the least deposits *during* the period, and that the loose banks were those loose at the beginning and which lost the least or gained the most deposits during the period. The medium banks consisted both of initially loose banks which became relatively tighter and initially tight banks which became relatively looser.

The data assembled by Bach and Huizenga show "similar treatment of small and large borrowers by all three groups of banks," leading the authors to "interpret the data as substantially rejecting the hypothesis that tight money led banks to discriminate especially against small-business borrowers" (pp. 66-67).

This brief review does not begin to do justice to the elaborateness of the Bach-Huizenga study, which examines the impact of monetary policy on small and large borrowers among banks of different sizes, and further provides a breakdown by industrial class. But it would be fair to say that these elaborate data all support the one major empirical conclusion mentioned above: that there was no significant and consistent difference between tight and loose banks as regards their treatment of small and large businesses. All three groups of banks increased loans to large businesses relative to small.

Whether this empirical conclusion supports the Back-Huizenga interpretation that tight money policies do not have a discriminatory impact is another matter. The data showing a clear pattern of increase in lending to large firms relative to small are not explained by this study. The pattern is attributed, not by theory nor by statistical test but by process of elimination, to differential changes in demand. It is contended in this note that the Bach-Huizenga data are consistent with a very plausible form of discrimination. All the authors have clearly shown is that if discrimination did exist during the period under review, it existed equally in tight and loose banks. Their finding against discrimination is based on an unstated premise that the pressures on commercial banks which might lead to discriminatory "credit rationing" in a period of tight money must be significantly greater on tight banks than loose banks. In other words, they assume that tight money creates no pressures or inducements to discriminate which influence commercial banks generally, irrespective of degree of "tightness" or "looseness."

II. *An Alternative Interpretation*

Especially interesting in this regard are the Bach-Huizenga findings respecting interest rates. They show similar rate movements for tight, medium, and loose banks. "This finding," they write, "is consistent with the hypothesis that the pattern of interest rates at banks is set by general market forces, and that banks generally follow a policy of price leadership in establishing interest rates, rather than using them as a device to discriminate among borrowers" (p. 79).

Thus the pressures of tight money induce commercial banks, even loose ones, to raise interest rates to borrowers. Here, then, is one effect of tight money whose impact is general among banks.

Moreover, the authors show that interest rates rose more to large borrowers, both absolutely and relatively, than to small borrowers, suggesting the possibility of discrimination-in-reverse with respect to price. "This greater increase in rates to large borrowers," Bach and Huizenga observe, "probably reflected, at least in part, the fact that small borrowers by 1955 were already paying rates near the *customary or legal upper limits* for nonconsumer loans at many banks. These legal limits are *as low as 6 per cent in 11 states, including New York, New Jersey and Pennsylvania,* and range up to 15 per cent in others. Thus as interest rates rose, rates to large borrowers could be increased without violating the customary or legal upper limit, while rates to small borrowers could be raised little or not at all" (p. 77; italics added).

These two important facts—the similar interest-rate behavior at tight and loose banks and the constraints limiting interest-rate increases—must be considered together because they imply discrimination against borrowers whose market-equilibrium interest rates rise enough to exceed the institutionally imposed maxima. If market forces and rational asset management lead to higher absolute interest rates for small than for large borrowers, and if the entire structure of interest rates moves upward because of tight-money policies, the bank manager—whether his bank be loose or tight—has little choice but to cut back on lending to small borrowers. His only alternatives are to lend to small businesses at less-than-equilibrium interest rates, or to violate the legal or traditional ceilings.

It is emphasized that these discriminatory effects would follow quite logically from Bach and Huizenga's own data and their own accompanying description of behavior patterns. If the institutional constraints on interest-rate increases are effective to significant degree, and if they are not offset by non-interest changes in borrowing terms (increases in compensating balances, more and better collateral, etc.) in a way which does not effectively discriminate against small borrowers, a relative increase in loans to large borrowers is precisely the result which should be expected when interest rates rise. The explanation, in this case, is not found in the *deus ex machina* of differential changes in demand, but in a form of discrimination over which bankers themselves have little control.

There is precedent for such an effect. Institutional constraints on interest rates in the residential mortgage-loan market, where FHA and VA rates are set by statute and are often below equilibrium market rates during tight-money periods, are generally agreed to be the factor most important in accounting for the responsiveness of this market to monetary policy. The Bach-Huizenga study at least raises the question of whether there are similar effects in the market for small-borrower commercial and industrial credit.

A. DALE TUSSING*

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2. *Financing Small Business*. Report to the Committees on Banking and Currency and the Select Committees on Small Business, 85th Congress, 2nd sess., by the Federal Reserve System. Washington, April 11, 1958.

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The Differential Effects of Tight Money: Reply

Mr. Tussing's alternative interpretation of our data is ingenious and, at first glance, plausible. But we think the evidence clearly supports our original conclusion.

His interpretation rests on the presumption that loan demand from large and small businesses did not, in fact, differ substantially during the period studied. But there is independent evidence that loan demand by large borrowers did increase more rapidly than that by small borrowers. We did not present this evidence because we did not consider Tussing's hypothesis as a major alternative to our own.

A brief "Reply" does not permit us to present the evidence fully here. The reader is referred, for one careful but thorough summary, to *Financing Small Business* [2], which presents the results of an elaborate Federal Reserve study of small-business financing during the period in question. In their summary on bank lending, the Federal Reserve authors report: "No class of supplier of small business credit reported any appreciable policy or curtailing its small business credit between 1955 and 1957. . . ." Noting the greater rise in loans to large borrowers, they continue: ". . . perhaps the major reason for such a trend was a difference in the demand for funds between small and large firms" (p. 369).

Supporting the Federal Reserve experts' evaluation, the evidence speaks for itself (especially pp. 380-81). During the period, internally generated funds in small firms exceeded all outlays for plant and equipment, inventories, and net receivables; while for large firms internal funds covered only two-thirds of such outlays. Indeed, small corporations were liquid enough to add substantially to their holdings of cash and government securities during the period, while large firms heavily reduced their comparable liquid asset holdings. Moreover, other studies have consistently indicated that small firms typically gain cash and other liquid assets both absolutely and relative to large firms during recoveries when tight money becomes relevant.[1] Such data do strongly confirm our presumption that bank loan demands of small borrowers grew less than those of large firms.

But Tussing might argue that his interpretation is, nevertheless, more attractive than ours. Even if small firms' loan demand didn't increase apace with large firms', the differential credit rationing system he hypothesizes could still have been at work. But in fact the loose banks in the sample were generally so loose that they didn't have to discriminate (ration more stringently) against anybody. As we pointed out in the original article (p. 59), over the period the loose banks gained more deposits than they expanded their total loans *and* investments. Thus, there was no increase whatsoever in pressure to refuse borrowers which might have led to the discrimination against small borrowers presumed by Tussing. On the contrary, the need to refuse loans *lessened* at the loose banks. The loose banks were so loose that they not only could increase their loans by over 30 per cent but could simultaneously substantially *increase* their holdings of securities. To say that a failure to increase loans more to small borrowers under these circumstances was discrimination against them induced by tight money seems to us to be a very strange use indeed of the term "discrimination."

We consider this evidence sufficient to reject the Tussing hypothesis and re-establish our own. But in case any doubt remains, one further aspect of the original data may be cited. Even if Tussing's presumptions were right, that all banks were led to discriminate against small borrowers because the interest rate legally chargeable to them was further below the equilibrium rate than for large borrowers, surely this disequilibrium spread would be narrower at loose than at tight banks. Thus, even though both discriminated against small borrowers, we should expect tight banks to discriminate more.

But this did not occur. In four of the five size classes of banks, the looser banks expanded loans relatively more to large than to small borrowers—that is, discriminated *more* against small borrowers than did the tight banks where surely discrimination should have been greater under the Tussing hypothesis. Only in the $500-$1,000 million bank-size class did tight banks show the greater discrimination against small borrowers which the Tussing hypothesis would lead us to expect.

Lastly, it is widely recognized in banking circles that interest rates are only one way of adjusting the effective cost to borrowers. Changes in compensating balance requirements, length of loan, collateral, and the like provide readily available ways of avoiding legal interest-rate ceilings to a considerable extent.

Interpretation of such complex data is always difficult. Our analysis does not, of course, prove the no-discrimination hypothesis. But the data remain consistent with our hypothesis, and, we believe, not consistent with that advanced by Tussing.

G. L. BACH AND C. J. HUIZENGA*

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[1] Perhaps the most complete and conclusive evidence is presented in [1].

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Errata

In the article by Daniel Orr and W. G. Mellon, "Stochastic Reserve Losses and the Expansion of Bank Credit" (*American Economic Review*, September 1961, *51*, 614–23), a slip in the mathematics has been detected and called to the attention of the authors by Professor S. C. Tsiang.

An unnumbered relation (the fifth on page 623) should read:

$$rk\int_{v}^{\infty} -(L - kD)L\phi(L)dL$$

instead of

$$r\int_{v}^{\infty} -(L - kD)L\phi(L)dL.$$

This changes equation (5), pp. 618 and 623, to

$$0 = i + (M + rv)\left(\frac{dv}{dD} - k\right)\phi(v) - rk[1 - \Phi(v)].$$

Only one change in Table 1 follows upon this correction. The value of D in Case 3 should be 10.7 instead of 10.5.

One further error, unrelated to the above, also has emerged. In Table 1, the D^* of Case 7 should take the value 50 instead of 40.

BOOK REVIEWS

General Economics; Methodology

Fundamentals of Model Construction in Macro-Economics. By ANDREAS G. PAPANDREOU. Training Seminar Series No. 1. Athens: Center of Economic Research, 1962. Pp. x, 172. Paper, $2.50.

The greater part of this book is devoted to the logical and mathematical underpinnings of economic models with, as the author says, ". . . examples referring systematically to macro-economics." Indeed the treatment of macroeconomic models is so restricted in scope that it may be doubted that it can accurately be termed systematic. Essentially the book, which is intended as a pedagogical aid, deals with the logical fundamentals of model construction.

The early portion of the book takes up elementary concepts such as sentences, sets, relations, functions, and structures. The concept of structure, which is drawn from earlier work by J. Marschak, H. Simon, the author, and others, is examined in some detail and forms a logical context for the development of models. Put very briefly, a structure is a set of relations having a non-empty intersection. A structure is called self-contained if the number of variables equals the number of equations; while if the number of variables is greater than the number of equations, the structure is called sectional. Self-contained structures are classified into three groups. If a self-contained structure can be divided into self-contained substructures, it is called unintegrated. A self-contained structure is integrated if it contains no self-contained subsets. A structure is called causal if it contains one or more self-contained subsets but cannot be partitioned into self-contained subsets. Sectional structures are treated as subsets of partially unknown causal structures. Now ". . . a model is a blank whose place may be taken by any member of a well-defined class of structures."

Discussions of static models as descriptive devices and as means of making policy decisions follow. These discussions are phrased in terms of the structure classification outlined above, and it seems to the reviewer that definite gains result from the use of the structure concepts. The policy discussion is reminiscent of Tinbergen's work on the formal theory of policy.

As a preparation for the development of dynamic models, the author discusses the elements of difference equations. This material follows traditional lines, and specific solution methods are developed for first- and second-order linear difference equations with constant coefficients. The material on dynamic models includes a very brief and uninformative section on systems of difference equations. This is followed by a brief but more helpful discussion of the correspondence principle.

Finally ten simple dynamic models taken from macroeconomics are presented and discussed. The discussion is in terms of the mathematical properties of the models rather than their economic characteristics.

Clearly the purpose of the book is to develop and present the elements of the formal logic of models. The author achieves this goal with a high degree of success, and his use of the structure concept as an explanatory and unifying device must be mentioned prominently in connection with this success. The lucid and precise exposition also contributes to the success of the book.

Keeping in mind that the book is to be put into the hands of students, we may inquire as to what group of students can profitably read the book. To determine this it may be pointed out what the student needs to know and does not need to know to read the book with understanding. The student must know something about macroeconomic models because those presented are not explained in a way that would be intelligible to the beginner. The student should know a little of the elements of differential calculus and the theory of determinants because these are not explained although they are used at a few points in the book. On the other hand, the student need have no knowledge of such fundamental concepts as sets, relations, and structures and their significance for economic models.

In the experience of the reviewer an unfortunately large proportion of beginning graduate students are admirably qualified to benefit from the book. Of course, this is simply another way of saying that logical fundamentals are inadequately treated in economics curricula. Perhaps this is due in part to an absence of suitable text material. The book should be helpful in overcoming this difficulty.

The book would be more useful to students if it had an index and a more complete bibliography. Indeed, references are so scanty that at times the author is at fault in not giving credit where credit is due.

A truly prodigious number of typographical errors mar the book. Most of these will cause only passing amusement, but some that occur in the mathematical notation may pull the reader up short. These latter are likely to cause needless frustration in student readers. An errata list is needed.

RALPH W. PFOUTS

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The Ultimate Foundation of Economic Science—An Essay on Method. By LUDWIG VON MISES. Princeton, N.J.: D. Van Nostrand Co., 1962. Pp. xi, 148. $4.50.

Ludwig von Mises' new book is at once an eloquent exposition of his views on the nature of economics as a science and a not so eloquent attack on contemporary thinking in economics.

The philosophical positions of the author are too well known to require detailed presentation here—but a brief and elliptic statement may be useful as a guide to the prospective reader. There are two basic branches of science, the natural sciences and the *sciences of human action.* The former are causality-oriented, while the latter are teleological in nature. Teleology presupposes causality—and expresses the "self-evident truth" that there is such a thing as "consciously aiming at ends." Action, causality, regularity are a priori categories—the category of action being the fundamental category of human knowledge and implying all the categories of logic as well as those of regularity and causality.

The sciences of human action fall into two categories, namely praxeology and history. Economics is the only part of praxeology developed thus far. Its premises—deriving from the category of action—are universal synthetic propositions, and its theorems are necessarily true. Conclusions derived from *ad hoc* (historical) premises may or may not be true depending on whether or not the *ad hoc* premises are true. History provides the experience, the record of human action. Such experience can never falsify a theorem of praxeology (economics). The historian attempts to *understand* (thymological method) the behavior of individuals in the past in view of the basic frame of reference of human action (praxeology). History may provide a guide to future action.

Macroeconomics, mathematical economics, and econometrics are singled out as the outstanding forms of degradation in economic thought. Econometrics, in fact, is considered a "childish play," which at best represents an attempt to deal with history in a quantitative way.

While some economists might be sympathetic to the view that econometrics may be "explanatory" rather than "predictive," very few indeed would be willing to accept this wholesale condemnation of modern work in economics—a condemnation that is only loosely linked (if at all) to the main philosophical argument of the book.

The reader has a difficult time attempting to perceive how logical positivism, institutionalism, macroeconomics, and econometrics are "totalitarian" in character. Indeed the author fails completely in his attempt to establish a connection between his philosophical commitment to the category of human action and his ideological commitment to capitalist society.

The book contains no new ideas—they have all appeared numerous times in his earlier writings.

ANDREAS G. PAPANDREOU

University of California, Berkeley

The New American Political Economy: A Synthesis of Politics and Economics. By MARSHALL E. DIMOCK. New York: Harper & Bros., 1962. Pp. xi, 305. $6.00.

Criticism of the structure and the qualitative performance of the U.S. business system has prompted economists such as Berle, Hamilton, Means, and Galbraith to return to the concepts of political economy with its value judgments, its vigorous policy recommendations, and its subjective evaluation of institutional and political factors. Marshall Dimock's latest book is not only a further step towards the reconstruction of political economics, but it is above all a fervent plea to all economists and political scientists to abandon the pursuit of narrow specialization, to combine managerial, political, and economic analysis and to be ready and eager to make strong value judgments when necessary. "The purist may charge that value judgments are 'unscientific'; the fact remains that they govern the choice of public programs and constitute the standards by which results are appraised. In the real world in which we are interested and in which sentiment is so large a factor, it is impossible to deal with policy . . . without bringing values and sentiment into the picture" (p. 23).

Dimock's book is not concerned primarily with methodology. This is an intensely personal book, and his "new political economy" is essentially a set of policy recommendations that would remake society as thoroughly as the *Wealth of Nations* did nearly two hundred years ago. It is Adam Smith, the author of the *Wealth of Nations* and the author of *The Theory of the Moral Sentiments,* whose views provide the foundation of the "new political economy," although there is very little in this book with which Jefferson or Schumpeter might have disagreed—or Boulding today.

Dimock's "new political economy," a decision-making process which merges economic analysis, political feasibility studies, and sophisticated management practices, relies firmly on price competition as the foundation of the good life. Not only does price competition provide the mechanism whereby the market can play its allocating role, but competition is more than that: it is "a way of life" (p. 133). Thus large-scale organizations are not only a threat because they reduce competition, but also because their bureaucratization is anathema to competition as a way of life, and their management-committees the burial ground of the Schumpeterian entrepreneur. But even if the oligopolistic production and pricing process of the giant corporation were more efficient than competition—as Lilienthal and Galbraith claim—competition would be preferable by far. "A competitive society stresses freedom and incentive to experiment, to innovate, to excel . . ." (p. 135). The purpose of society is not production *qua* production, but the "good life." It is one of the essentials of the "good life" that the human personality be not dwarfed by the size of the organization.

In order to reduce the impact of modern technological and institutional factors that promote bigness, Dimock urges decentralization of the governmental functions by assigning to state, local, or regional agencies as many of the administrative, regulatory, and welfare functions of the federal government as possible; decentralization of giant corporations by creating an antitrust and a tax policy that would promote the decomposition of giant enterprises into their most efficient components. The efficiency of economic operations would be judged by the concept of balance—balance between agriculture and industry, between rural and urban development, between individual work satisfaction and the demands of modern society. Dimock, in Veblenian terms, recognizes the importance of work for the individual as a necessary element of the "good life" and sees in "Madison Avenue" and in the unsophisticated ideology of the businessman a great threat to the survival of the market system. Dimock, very much like Smith, attempts to protect the business system from the businessman. Governmental planning that anticipates and prevents developments that threaten the competitive system is a necessity, and Dimock, for all his enthusiasm for "small government," makes intelligent, effective governmental action the *sine qua non* of his "new political economy." This last point, as well as the general disenchantment with contemporary U.S. business that permeates this book (e.g., p. 97), will make it highly unpopular with the ideologues of the Right, although its basic philosophy is truly conservative.

A few criticisms: (1) The chapter on foreign policy is quite unnecessary. Surely in a book in which the author addresses himself to almost every known domestic economic, political, or sociological problem facing the United States today, he cannot also tackle our foreign policy—such as it is—and especially not in 19 pages. This is by far the weakest chapter in the book.

(2) In advocating the return of governmental functions from Washington to the states and municipalities, Dimock fails to consider the deplorable level of moral standards and intellectual ineffectiveness that is the rule on these levels. The thought of more authority and more money in the hands of the city fathers of this reviewer's "home town," for example, makes one blanch, and it should be added that this particular city has less problems and more Ph.D.'s per capita than almost any other industrial city in the United States.

(3) Dimock occasionally tends to belabor a point, especially in the first few chapters. Thus hardly anyone interested in public policy will contest his point that specialization in U.S. universities has gone too far, that economists should know political science and political scientists should know economics. The point that contemporary business ideology has no relation to reality has recently been made by a practicing political scientist and may not need as much emphasis as Dimock gives it, although the business community is more likely to listen to a management consultant of his repute than to a Democrat who "never met a payroll." These are minor criticisms, however, and should not distract from a vigorous and thought-provoking book that ought to have considerable impact on its readers.

Alfred L. Thimm

Union College

Osnovi političke ekonomije (Principles of Political Economy). By Mirko Perovich. Belgrade: Savremena Administracija, 1962. Pp. 365.

This is a well-written textbook prepared for first-year students of economics—a Yugoslav version of Samuelson. Our main interest in this and similar textbooks published in Yugoslavia and other Communist countries should be in tracing the way in which new generations of young economists are being brought up in Eastern Europe, and also in determining—indirectly—the possible future course of economic analysis in Communist-dominated Europe.

Mirko Perovich's book consists of two distinct parts: (1) economic theory of capitalism, which is purely Marxian and therefore represents a combination of philosophical dogmatism, good historical observations, and economic analysis based on the labor theory of value; (2) economic theory of socialism based on conclusions drawn from the Yugoslav experience with respect to economic planning.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Interdependence, Resource Use and Structural Change in Israel. By MICHAEL BRUNO. Jerusalem: Bank of Israel, 1962. Pp. xix, 324.

This is a book on the application of the input-output technique to the analysis of the production structure of the Israeli economy for the years 1958–1960. The book is divided evenly into text and appendices, with the latter mainly concerned with the construction of tables related to empirical input-output flows at various levels of aggregation (e.g., commodity classification, data, sources), and the text mainly with presenting and interpreting these tables. This emphasis on "practical empiricism" relating to data and application can be explained by the fact that the book is a translation of a research document of the Bank of Israel and is intended as a "practical tool for economic policy making." One should therefore not expect to find significant theoretical extensions of the input-output approach or even statistical implementations of the fancier types of input-output models (e.g., the various regional and dynamic models), which are of dubious relevance to development planning. In short, the book is intended for the "practitioner" rather than the "academician" in the input-output area.

Michael Bruno's model structure is based on the extension of the familiar open input-output model in two directions. On the one hand, the final demand column, Y, is partitioned into several components such as Y_1 (private consumption), Y_2 (government consumption), Y_3 (investment), and Y_4 (exports). On the other hand, several categories of primary factors of production are postulated for each type of factor, e.g., M_1' (labor service coefficient) M_2' (capital service coefficient), M_3' (import coefficient), etc. Thus the model structure, in matrix notation, becomes

$$\text{1a)}\quad X = BY = B(Y_1 + Y_2 + Y_3 + Y_4)$$

$$\text{b)}\quad L_i = M_i'BY = M_i'B(Y_1 + Y_2 + Y_3 + Y_4) \qquad (i = 1, 2, 3)$$

where $B = (I - A)^{-1}$ is the Leontief Inverse Matrix and where L_i is the total employment (or total input) of the i'th primary factor. The five chapters in the text involve mainly the empirical implementation of such a system.

Chapter 1 is devoted to an explanation of the central logic of the simple open model (i.e., $X = BY$, $L = M'BY$). The author motivates the uninitiated reader by working under the self-imposed handicap that mathematical symbols are to be avoided at all cost, though from the point of view of the professional economist the exposition of the entire book probably suffers as a result.

Chapter 2 presents a 25×25 input-output table for Israel, following the previously elaborated conceptual framework. A notable feature of the Israeli input-output table is the detailed treatment given to the import row (M_3'), which is broken down into commodity specifications listed in a separate 25×25 table. This emphasis on foreign trade is carried over to subsequent analyses in the following chapters.

Chapter 3 attempts to introduce "stock" concepts into the "flow" framework of the input-output system. The values of capital stock ($K_1, K_2, \cdots, K_n$) and man-hours of employment ($L_1, L_2, \cdots, L_n$) are independently estimated for the n-industries. Inasmuch as the flow concepts ($\pi_1, \pi_2, \cdots, \pi_n$) (valuation of capital services) and ($w_1, w_2, \cdots, w_n$) (valuation of labor services) in the primary factor rows of the input-output table are factor payments, π_i/K_i and w_i/L_i are thus empirically estimated as the rate of profit and wage rate, or the remuneration of capital and labor in the i'th industry. The measurement of K_i/L_i (capital-labor ratio) and $\pi_i/(\pi_i+w_i)$ (profit share in value added) is also listed for all industries at the 42×42 aggregation level.

Aside from the problem of the quality of the capital stock data (which the author realizes), the grafting of the stock concept into the flow system in an entirely empirical fashion raises serious questions as to the theoretical justification for such an approach. For the author has, in fact, moved toward what may be called "industry-specific distribution-theoretic analysis," which is *not* an area of knowledge in which input-output analysis shows strength. In view of the absence of any reference to relevant distribution-theoretic foundations, we may view Bruno's analysis here as an experiment aimed at gathering inductive evidence for future research.

The longest chapter (Chapter 4) is partly descriptive and partly devoted to analytical matter. In the descriptive part, in addition to presenting the coefficient matrix A and the inverse matrix B for 1958, the author demonstrates the empirical results for the same year due to a decomposition of (1a) into

$$2)\qquad X_1 = BY_1; \qquad X_2 = BY_2; \qquad X_3 = BY_3; \qquad X_4 = BY_4$$

by making use of the linearity of the system. This enables him to attribute the total output of that year to the various final demand components and, through a tabulation of the accounting equality ($X_i = AX_i + Y_i$), to allocate total output (X_i) as intermediary factors (AX_i) or as final demands (Y_i). It should be noted that since this demonstration applies only to one specific final demand bill (Y_1, Y_2, Y_3, Y_4 in 1958), no particular analytical insight can be gained from it—except a sense of the comparative importance of the intermediary factors of production.

For the analytical portion of Chapter 4, the key concept is the *real primary factor* cost c_{ij}, which can be defined for every industry (i) and every primary factor (j) as

$$3)\qquad c_{ij} = M_j' B u_i,$$

where u_i is the i'th column of the identity matrix. (c_{ij} is the total input of the j'th primary factor due to the delivery of one unit of the final demand of the i'th commodity.) The estimated c_{ij} can then be combined with the "stock" estimates of Chapter 3 to obtain concepts such as real unit capital cost, real unit capital-labor ratio, etc. The results of these calculations (for 1958) are tabulated for various industrial aggregates, e.g., agriculture and industry.

The major contribution of this book is the application of this c_{ij} to a "profitability of export" analysis. Basically, the following "cost-benefit ratio" problem is posed: for every exported commodity what is the total domestic real factor input (i.e., the "cost") per dollar of foreign exchange earnings (i.e., the "benefit")? The estimation procedure may be illustrated by assuming that there are two categories of primary factor inputs: labor ($j=1$) and imports ($j=3$). Then the profitability π_i of exports for the i'th industry is:

$$4) \qquad T_i = \frac{c_{i1}}{p_i - c_{i3}r} \doteq \frac{M_1' B u_i}{p_i - M_3' B u_i},$$

where r is the foreign exchange rate and p_i is the price of the exported commodity in foreign currency. The numerator is the real unit labor cost (in domestic currency), the denominator is net foreign exchange earnings per unit export (as it is the difference between the price of export and the unit cost of imported raw materials in terms of foreign currency). Under competitive and linearity assumptions, T_i should be the same for all industries. In fact, the finding for Israel is that the T_i's are quite different for different industries. Although the author did not go into an explanation of why such differences occurred, the empirical findings do have policy implications with respect to the desirable direction of export expansion.

Chapter 5 is devoted to comparative static analysis, which in the input-output tradition usually means a comparison of the changed production structure between two years with changes due either to changes in final demand or to changes in the structural coefficients. For a comparison of final demands, the actual final demand column in 1959 (and 1960) is applied to the structural coefficients of the base year (1958) to obtain an "estimated" bill of imports for 1960, which is compared to the actual bill of import for that year. To the extent that discrepancies occur, conclusions may be drawn with respect to the orientation of the economy toward "import substitution." For a comparison of structural coefficients, the author compares the results of the export profitability analysis for 1959 and 1960 with that of the base year. Comparative static analyses of this type have obvious policy implications with respect to the direction of expansion of exports. It is to be noted that the problem-oriented approach of this book leads to the selection of bottleneck issues most relevant to policy-making issues which, in the case of Israel, center around foreign trade.

The final chapter is devoted to the forecasting problem of comparing actual growth rates (1958–60) and predicted growth rates for the industry sector. The predicted growth rate is derived by a projection of the final demand (a procedure external to the input-output structure) applied to the base-year structural coefficients (a procedure internal to the input-output structure). Since resulting errors can be traced to either the projection of final demand or to changes in the structural coefficients, the author isolates the first type of error by applying the actual final demand for 1960 to the base-year structural coefficients. This enables him to arrive at the conclusion that, for the purpose of short-run forecasts, the input-output approach is adequate for the Israeli economy in the sense that the errors are shown to be due mainly to the final demand projections.

As an instrument of development planning, we might say that the input-output approach is more useful when the country is small (implying only moderate regional complications); when the over-all growth rate is adequate (implying the need for precision in resources calculation due to a tight resources situation); when major resource bottlenecks are identified (implying the possibility of a problem-specific approach such as the foreign trade emphasis in Israel); when qualified research teams are available; and, above all, when sector-sensitive policy instruments are available for plan implementation. In this context Israel may constitute an exceptional case, as all these conditions seem to be satisfied. Nevertheless, this volume may be recommended more generally to practitioners in development planning since it conveys very well the sense of problem anticipation and data consciousness so essential for success in this area.

JOHN C. H. FEI

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Le capital dans l'économie soviétique. By MARIE L. LAVIGNE. Paris: Société d'Edition d'Enseignement Supérieur, 1961. Pp. 351. 25 NF.

This book is a useful compendium of recent Soviet theories and methods of capital valuation and allocation, critically examined in the light of the Lange-Lerner criteria. The Soviet material stems mainly from the years 1957-60, although this limitation is not made explicit. The summary of Western theories is more complete. The author's own opinions are modest. Sometimes Mme. Lavigne seems to concede a point to Soviet arguments; however, on the most controversial topics—such as the interest on nationalized capital—she thinks the judgment is still premature, for Soviet economics is evolving. That evolution is believed to be headed mainly towards the Lange-Lerner model and away from Marx. In her conclusions, Mme. Lavigne sees a possibility of future *rapprochement* between capitalism and socialism, though "as yet vaguely."

The monograph consists of two parts. The first deals with the value of capital and the theories of factor pricing and costs. The chapter on Soviet cost theories is especially welcome, for this subject has been neglected. The second part discusses capital management, the theories and measures of depreciation, and economic objectives of capital allocations. At the end the book has a short list of selected references.

As a contribution to the recent history of Soviet economic doctrines, this monograph is certainly praiseworthy. Its limitations need to be told, therefore. Most of all, its factual backgrounds are noticeably weak. The nullifying treatment it accords to everything called "orthodox" in Soviet theories is too sweeping. The effect of this attitude is noticeable, e.g., in the author's inconclusiveness in the discussion of the notorious "Marxian" law of the faster growth of Dept. I as compared to Dept. II. A background study of the 1953-55 discussion of this law (Prof. E. Kasimenko's writings *et al.*), which is missing, would have helped a lot, while some research into the history of the law would have dissolved most of its mysteries. In Marx's reproduction models there is no such exclusive and invariable law at all. It was first formulated by Lenin, but only in relation to capitalism and so as to prove the inevitability of depressions emanating from such a law. It was M. Tugan-Baronovsky, a revisionist critic of Marx and Lenin, who first suggested that Dept. I could grow faster than Dept. II without producing a crisis. His students, who later became Stalin's followers and planners, made this idea into the initially successful law of the Five-Year Plans, only to realize at the end, by 1953-55, that, essentially as predicted by Lenin, the stringent application of the law resulted in acute disproportions and even a slow-down in Soviet economic growth.[1]

At times Mme. Lavigne's approach is too doctrinaire in its faithful adherence to the Lange-Lerner model: the real intellectual challenge of Soviet issues is lost. For example, in her discussion of the deliberate differential valuation of factors by the Soviets, the author vehemently condemns such practices as inconsistent with her doctrine, but fails to show what is wrong with them from the viewpoint of the Communist Party's objectives. The criticism of the below-cost pricing of agricultural produce is convincing inasmuch as the satisfactory minimum supply of consumer goods is impaired. But why should Dept. I's output be valued equally with that of Dept. II, when the subsidizing of Dept. I at the expense of Dept. II helps expand the country's military and economic potential? Has anyone really proved that direct, progressive income taxation could supply the USSR with more investible capital than it has been able to squeeze out by its present methods?

Despite all its limitations, however, Mme. Lavigne's monograph is worth reading, for it takes stock of the present state of discussions on both sides, and can thus serve as a point of departure for further progress.

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[1] Cf. in addition, V. Holubnychy, "Le ralentissement des rythmes d'accroissement de l'économie soviétique," *Problèmes soviétiques,* no. 2, 1959; and H. H. Höhmann, "Die Produktionsabteilungen I und II bei Karl Marx und in der 'Politischen Ökonomie,' *Osteuropa-Wirtschaft,* no. 2, 1961.

Market Theory and the Price System. By ISRAEL M. KIRZNER. Princeton: D. Van Nostrand Co., 1963. 324 pages. $6.00.

The author of this book, an associate professor of economics at New York University, states in his Preface that it is "designed for an undergraduate course in intermediate price theory." He gives as his "excuse for adding yet another book" to "a number of competently written textbooks" which have been published in this field during the past few years that "his approach, while in no sense original, presents the subject in an entirely different light." What this light is may be inferred from the dedication "To Ludwig von Mises," to whom Professor Kirzner says "he owes his appreciation of the market process," and who also read the entire manuscript and offered many helpful suggestions. By way of further orientation, Professor Kirzner also notes in his Preface that "Whatever the author may have learned from Marshall, Edgeworth, and J. B. Clark, this book probably will reveal that he had learned more from Menger, Böhm-Bawerk, and Wicksteed."

Perhaps the most conspicuous difference between this and the common run of price-theory texts is its moderate size and its conspicuously restrained use of geometrical figures and mathematical equations. If we assume that the function of a textbook in this field is to acquaint the students with the more important developments in twentieth-century price theory, we are driven to the conclusion that this book does not perform that function. Without having undertaken a statistical study of the matter, I would hazard the guess that any one of the ten most widely used elementary texts contains more of the substance of "standard" price theory than this book does.

This might be judged a merit, but I cannot judge it so. Responding to several generations of attacks upon their basic assumptions, price theorists have become conspicuously "iffy." If we assume that buyers behave in such and such a fashion, and that producers do so-and-so, then such and such price patterns result. But Kirzner follows his master in discarding all such timid equivocation. His theory is based on the laws of "human action." Thus, "Each human being is eager to act to improve his position" (p. 1); "That men are able to arrange their preferences in order of importance is inherent in the nature of man himself" and "It is of the essence of action that it aims at encompassing the fulfillment of as many of the actor's desires as possible ***in the order of their urgency***" (p. 46, author's italics); and "A consumer finds himself with given money income only *after* he has made his decisions concerning the quantity of his labor services that he will offer to the market at going wage rates" (p. 63 n.).

Most unusual, perhaps, is Kirzner's identification of the wage worker as a "resource owner." As such:

> He finds himself endowed daily with a given quantity of the factor. He can do one of two things with each unit of his factor supply. He can sell it in the factor market, or he can keep it for himself for direct consumption. For example, a man finds that he can supply twelve hours of labor per day. He can choose between selling all or part of this in the labor market, or enjoying the whole time for himself as leisure (p. 226).

On this theory the four million workers who are now unemployed may be considered to be contributing to the mass consumption on which so many present-day economists set so much store.

Students who make this book their Bible may or may not be adequately prepared for comprehensive examinations on modern price theory, but they will certainly hold the market system in high esteem.

C. E. Ayres

University of Texas

The Nature of Price Theory. By H. H. Liebhafsky. Homewood, Ill., Dorsey Press, 1963. Pp. xxi, 562. $8.00.

With few exceptions recent price theory texts have tended to evade the real difficulties of the subject. Professor Liebhafsky neither evades nor avoids difficulties. He enjoys tangling with them and is never in any hurry to finish them off.

The organization of this book reflects the nature and logical organization of its subject far better than do most of its competitors. The introductory section includes a chapter on the basic quantitative relationships employed in the analysis, an excellent idea which is weakened in this instance by Liebhafsky's decision to illustrate these relationships in terms of cardinal utility. To an extent this chapter has been turned into a brief for the reinstatement of an extreme form of Jevonian utility analysis, an analysis which the author attributes to Marshall. Thorough discussions of consumer equilibrium and of the theory of production and cost precede the sections on product and factor markets. The concluding section includes surveys of static welfare economics and of the relationship of price theory to theories of economic development. A bow to fashion is made by inclusion of the usual labored solution of an elementary linear-programming problem. Had this irrelevancy been replaced by a chapter clarifying the relationships of partial and general analysis, the coverage and organization of the book would have approximated an ideal.

Unfortunately, the excellencies of the text are offset by serious weaknesses. The most important of these are a pseudomathematical method which is used to supplement the textual and graphical analysis and occasional failure on the part of the author to grasp the real meaning of the assumptions with which he is dealing.

The mathematics employed is an algebra of finite differences, which is called upon to do much of the work of the differential calculus. Aside from being more difficult to follow than calculus, the technique is inadequate for many of the tasks assigned to it. Thus the author is led into confusion and occasional error. For example, a seven-step "proof" that vertically parallel indifference curves must imply constant marginal utility of the numeraire as income increases (rather than constant marginal rate of substitution between the numeraire and the good under consideration), really "proves" only that vertically parallel indifference curves cannot be vertically tangent to nonparallel price lines (p. 114).

The most fundamental theoretical difficulties stem from confusion regarding the meaning of partial analysis. To Liebhafsky, *ceteris paribus* implies NOT *mutatis mutandi.* Any consideration of compensating adjustments implies that we are dealing with general analysis (pp. 81-82). Thus Marshall's *ceteris paribus* clause for demand analysis is held to require constancy of all other prices, of money income and of the marginal utility of money (p. 59). *Therefore, all Marshallian demand schedules are of unitary elasticity* (pp. 75-76). Having achieved a successful *reductio ad absurdum,* the author chooses to accept the conclusion rather than to reject the premises. Friedman's interpretation of Marshallian demand is not mentioned, despite a plethora of citations of less significant works.

Another set of confusions arises from the interpretation of the inner cost difference resulting from a price fall as the Hicksian substitution effect (pp. 98-105 and *passim*). Thus the income effect, the remainder of the change, is independent of the level of real income. This interpretation leads to identification of normal goods as those goods with elastic demands, and of inferior goods as those with inelastic demands (p. 486). By implicitly defining complementarity in terms of cardinal utility, the identity is carried a step further. Goods with elastic demands are substitutes against the numeraire, while those with inelastic demands are complementary with it (p. 501). Thus two valuable tools have been subsumed into demand elasticity, and complementarity has become possible in a two-commodity situation.

While Liebhafsky insists on an objectifiable measure of real income changes, he proposes a new family of "ideal" index numbers that are nonobjectifiable. Although it is maintained that ". . . the (basic) index uses all the data and only the data" (p. 540), it actually uses only half of the data, that of the base period. However, it also requires a measure of the equivalent variation in income, which is *not* data. (I interpret Liebhafsky's "*R*" as the equivalent variation on the basis of its identification as *FI* in Figure C-2. Liebhafsky has designated another quantity, which seems to approximate equivalent consumer's surplus, as the equivalent variation.) The proposal for objectifying the equivalent variation reduces the index to a complex and arbitrary measure with no apparent advantages over Fisher's "ideal" index.

Serious confusions also arise in two appendices on consumer's surplus. A great deal of effort is devoted to demonstrating that Marshall's *minimum* estimate of increments in consumer's surplus in terms of the inner cost difference is not a full measure, though it is acknowledged that Marshall had pointed this out (pp. 509-12). A lengthy section demonstrates that Hicks' equivalent variation is not the same thing as his compensating variation, and that neither concept is the same thing as Marshall's consumer's surplus (pp. 522-27). It is not clear whether or not this is expected to come as a surprise to Hicks. Neither is it clear why the comparison was made with the income variations rather than with Hicks' measures of the surplus.

One more general point to note is the extensive use of references to the literature of economics. (The index gives no measure of this, as footnotes are not usually indexed.) Whether detailed citation is worthwhile in a text is debatable, though certainly key references should be made. In any case, if detailed citation is to be included it should be authoritative. It is not always so in this book. The idea of "external diseconomies" is attributed to Viner (p. 184), though it was the central point in the controversy over Pigou's use of increasing supply price. The marginal-productivity theory of distribution is credited to J. B. Clark (pp. 314-16), in complete disregard of Wicksteed and his critics. The marginal-cost proposal for efficiency pricing is dated from Montgomery's and Hotelling's 1938 articles (p. 405), though it stems from Pigou's *Economics of Welfare* and it had been discussed extensively in the early 'thirties. It had been incorporated into J. E. Meade's elementary text by 1936. With over 150 pages devoted to demand theory, the single reference to revealed preference fails to mention Samuelson, citing only Houthakker's article on integrability conditions, an important but highly esoteric point (p. 515).

A few other important weaknesses may be listed: (1) "Infinitely elastic demand" is defined so that elasticity is unitary (p. 68). (2) First-order homogeneity of the production function is held to imply constant costs to the firm. This is a common conclusion, which is true in the pure Walrasian model. It is not true in the Marshallian model employed here, in which the entrepreneurial input is fixed to the firm (p. 120). (3) Positive marginal productivity of all inputs is held to imply convexity of isoquants. It implies negative slope, but nothing more. *Falling* (and positive) marginal products are sufficient, though not necessary, for convexity (p. 135). (4) "Economic capacity" is defined as the output which minimizes short-run average cost. A better definition, if such a concept must be defined, would be the point or set of points on the short-run cost curve which are also on the long-run cost curve (p. 164). (5) Divergence between the supply price of labor and the value of marginal product is assumed to represent profits for the firm. But monopolistic position in product markets and even monopsony in factor markets may easily exist in competitive situations, in which profits tend to be eliminated (pp. 263-64). (6) The illustration of game theory applied to oligopoly is trivial, as three of the alternatives open to one player are dominated by the fourth (p. 278).

It seems unlikely that this book in its present form will suffice as a text for students at any level. Yet the sections on product and factor markets and several other chapters are superior to corresponding sections in most other texts in the field. As much of the erroneous material is really extraneous to the purpose of the book, a revised edition will merit further consideration.

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University of Arizona

National Income—Its Measurement, Determination and Relation to Public Policy. By SAM ROSEN. New York: Holt Rinehart and Winston, Inc., 1963. Pp. xviii, 454. $7.50.

This book represents the latest addition to the rapidly proliferating list of macroeconomic textbooks. Professor Rosen's avowed intent in this book is to construct a functionally balanced presentation of the "trichotomy of macroeconomics . . . measurement, theory, and public policy." The book, accordingly, is divided into three sections, each dealing, though not exclusively, with one facet of this trichotomy.

Part I, entitled "Measurement of National Income," is concerned primarily with the conceptual and quantitative aspects of the income and product accounts as constructed by the N.I.D. of the Commerce Department. The approach used in the presentation is one which is, by now, almost standard. It begins with an income statement of the single business unit and builds, through the requisite series of adjustments and consolidating procedures, to the national income and product statement and the sector accounts. The author rounds out this section by introducing the complementary social-accounting techniques of input-output and flow-of-funds analysis.

This section goes beyond most recent textbook discussions of social accounting. There is, for example, one chapter devoted to the "Genesis of National Income" which, despite the ambiguity of the title, deals with the historical development of social accounting. This chapter draws heavily upon the monumental work of Paul Studenski. As an aside, it may be hoped that this single chapter will not be used as a substitute for student exposure to the Studenski work itself. Professor Rosen's book also marks a return to the commendable practice of providing some discussion of the data collection and compilation process. There is also space devoted to consideration of the validity of the final measures and some of the critical views which have been expressed about the conceptual arrangement of the income and product accounts.

A theoretical statement of "National Income Determination" is presented in Part II. This section also includes a chapter covering post-World War II cyclical experiences. While the notion of such a chapter is in itself laudable, this reviewer felt that it could profitably have been deferred until after the framework of the income-determination system had been presented. If this had been done, the chapter could have been used to exhibit the workings of the macroeconomic model in a real-world setting, as well as indicating the direction of future expansion of the basic model, i.e., toward cyclical theory; and finally it would have served to introduce the policy discussion which followed.

In Part III the author devotes 61 of the book's 454 pages to a consideration of "Public Policy." Here the student is confronted with a discussion of national goals, fiscal and monetary policy—including some recognition of the host of related problems. Included in this part also are policy issues and formulations raised by wage-price inflation, monopoly, economic controls, guaranteed annual wages, wage-employment relationships, and foreign-trade problems.

Writing textbooks is all too frequently a necessary but thankless labor of love, for almost any method of presentation will generate some reaction based upon the "I would have done it another way in my class" response. This must be borne in mind when considering any reviewer's critique of any particular textbook contribution. With due allowance for this manifestation of the pedagogue's militant sense of individuality, it seemed to this reviewer that the deficiencies of this book are essentially concentrated in Parts II and III. Indeed, Part I is a comprehensive and lucid presentation of social-accounting concepts and problems.

In Part II the author presents the basic macroeconomic model and its subsequent extensions in prose fashion with some minimal reliance upon geometric constructions. This, in the eyes of some instructors, may be a praiseworthy achievement which in fact commends the book for their serious consideration as a text. However this reviewer felt that the prime result of this effort was a certain lack of clarity or sharpness of concepts and relationship which is sure to be transmuted into student confusion (see, for example, the presentation of the accelerator principle).

The major weakness of Part III, the public-policy discussion, lies in the frugality of treatment accorded to the welter of complex policy issues catalogued by the author as being significant.

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Die Konsum- und Investitionsfunktion. Untersuchung für die Bundesrepublik Deutschland. By H. Ismar, G. Lange, and H. v. Schweinitz. Edited by Leo Brandt; with an introduction by W. Krelle. Cologne and Opladen: Westdeutscher Verlag, 1962. Pp. 419. DM 76.

This study is a very competent econometric analysis of investment and consumption behavior in the Federal Republic of Germany during the postwar period (1950 to 1958). Under the guidance of Professor Krelle the authors have made a contribution which might well prove path breaking for econometric analysis done in Germany. Even those students of the questions analyzed who are inclined to a somewhat sceptical attitude with respect to the general validity of the results achieved by econometric studies based on time series for the period under consideration cannot help but be impressed by this volume.

In Chapter 1, H. H. Ismar introduces the statistical methods used. There can be little doubt that this section is far too demanding for the average reader with a background in economics. (The author, who is a mathematician by training, demands too much a priori information from the economists formulating hypotheses to be analyzed by the econometrician [pp. 25 and 27].) It is to be hoped that the economist who is unable to follow the author to his level of abstraction and who does not have the prior knowledge required will not be discouraged by this chapter. Even to him the rest of the book makes very worth-while reading. The chapters on the investment function and the consumption function start with an extensive survey of existing literature. Both existing theoretical and econometric analyses of the questions considered are dealt with. Even though no claim to complete coverage of the available literature is made, there can be no doubt that this survey will provide a most valuable source of information for most German readers.

It is to be regretted that some of the theoretical considerations that precede the econometric analysis of investment behavior give rise to serious objections. This is true both for the ideas on the relation between liquidity and depreciation (p. 103) and the relation between the accelerator and the capital-output coefficient (p. 140). The critical analysis of the statistical data used and of the methods employed to gain additional statistical information deserves high credit. The importance of these contributions made by Brandt and Lange will go beyond the use made of the data in this particular volume. The statistical method used all through the analysis is the least-squares single-equation method. The introduction to the volume makes it evident that the authors are well aware of the objections that might be raised to this approach.

Within these limitations the authors employ highly advanced statistical techniques. The extensive use made of the methods of confluence analysis shows that the application of multiple-regression methods is not warranted in any of the cases under consideration. The methods employed in analyzing consumption behavior seem to go beyond the analysis of the investment function in two respects. The indirect method of parameter estimation is used, and through this the systematic positive bias introduced by the definition of national income is eliminated. The question remains open whether it might not have been advisable to use a similar approach in analyzing investment behavior. If, as the author states (p. 173), gross investment during one period can best be explained by the profits of the same period, it might be suspected that a bias similar to that of the direct estimation of the parameters of the consumption function might be partly responsible for the high significance achieved. The results of the analysis of investment behavior in individual industries give additional support to this suspicion. It turns out that the relation established cannot be applied to express the investment behavior in consumption goods industries (p. 207). This is exactly what should have been expected if the assumption is made that investment has a dominating influence on profits in capital-goods industries.

In addition a test of the predictive qualities of the consumption function is made (p. 333). For this reason the author does not use all information available to compute the consumption function.

There can be no doubt that, if this contribution to econometric analysis gets the attention it so amply deserves, it might well result in some discussion among German economists. Objections might be directed against the particular econometric methods employed and against the basic assumptions made. It might be argued that the assumption of constant structural relations during the period considered is highly unrealistic. In this case the methods applied, i.e., the use of statistical data gained from time series, would not give the results desired. Unless this reviewer is very much mistaken, both the editor and the authors of this study would be well satisfied if their contribution gave rise to a competent discussion of these problems. A better and more competent starting point for such a discussion could hardly be asked for.

JENS LÜBBERT

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Inflation—Proceedings of a Conference Held by the International Economic Association. Edited by D. C. HAGUE. New York: St Martin's Press, 1962. Pp. xii, 510. $12.00.

In 1959 the International Economic Association devoted its annual Round Table Conference to the problem(s) of inflation. The 48 economists who participated at Elsinore were from 24 countries. Seven were from the United States (Chamberlin, Duesenberry, Ellis, Gardner, Haberler, Marget, and Triffin), four each from England and France, and three each from Germany, Netherlands, and Sweden.

In a sense this volume is a memorial to Erik Lindahl, who was president of the Association from 1956 to 1959. He was presiding officer and host at the conference, presented one of the papers, and was to have edited this volume. His death soon after the conference placed the responsibility for the editing, including the preparation of the summary of the discussion, upon Douglas C. Hague of the University of Sheffield.

Twenty-three papers were presented and are included in the volume. The conference first considered the causes of inflation and its effects. It then took up fiscal and monetary policies for controlling inflation. Then followed three papers on the part played by wages and trade union power. The next subject was the international aspects of inflation, including the possible contributions of international organizations to price stability. Finally, the conference considered inflation in socialist and in underdeveloped countries.

Professor Hague's summary of the debate fills the last third of the book if one uses a word count, for it is printed in smaller type. Although the "debate" usually did not appear to go beyond a number of comments on each paper and a final rejoinder by the author, the reporter has managed to bring this part of the book to life. While the papers were relatively brief and simple, the discussion added many elaborations and complications, and these are the most interesting part of the book, particularly since they reflected so often the nature of the economy with which the individual economists were most familiar. Thus, for example, the problems connected with the money supply for a less developed country take quite different forms from those in advanced countries because of the requirements of an economy being commercialized and the limited use of demand deposits.

In general, the debate disclosed points of disagreement but there were too many participants and too little time to allow any disagreement to be explored in depth. To be sure, there were a number of cases where apparent disagreements disappeared as soon as it became evident that the difference was primarily one of definition of terms rather than of concept. (Are cyclical price increases which are not offset by subsequent decreases to be regarded as inflation?) And other cases turned out to be instances where the defendant agreed that his model did not allow for the conditions or assumptions put forward by the commentator. (To what extent is velocity to be envisaged as becoming infinite, as secularly stable, or as subject to adaptation to changing situations?) The separate papers and discussions of inflation in a socialist economy (Poland) and in Latin America also made it clear that economic actions and reactions are dependent in large part upon the institutional environment, including much more emphasis on what was called the " 'democratic' or 'political' push."

As one might expect, the book reflects the various explanations of postwar price history which have been the subject of active discussion over the past decade. As to causation, each speaker placed his own emphasis on the various factors, but the total effect of the discussion, summed up very well by Professor Lundberg, was to recognize that an economy has interlocking parts with variables which are largely interdependent, so that any analysis involves both a series of interdependent equations and a sequence analysis. Even velocity was described as both a cause and an effect. Nevertheless, the greater emphasis (except in countries where imports are substantial) seemed to be placed on demand-pull, with the added point that the level of demand can be more easily controlled by policy than can cost factors. Within this framework, there was the usual disagreement as to whether or not a change in the money supply was the main cause of or merely concomitant with a price movement. The differences on this point were strongest in connection with creeping inflation.

As to cost-push, the greatest blame seems to have been laid at the door of labor unions. Unemployment would have to be very high to limit wage-push. Some seemed to agree that the main aim of policy is to achieve and maintain full employment while others emphasized economic growth. At any rate, it was suggested that even after full employment (less than 4 per cent unemployment) had been reached and excess demand had been removed, there might still be a 2 or 3 per cent price and wage annual increase due to trade union pressure or serious frictional problems. The discussion of wage-push led to a few suggestions for direct action. One suggestion was to organize both business and labor into strong national monopolies so that they would be compelled to think in national terms. A second solution, which got little support, was to introduce automatic escalation procedures. A third (from the United States) was to reduce labor union power or to strengthen employer power.

The effort was made, and not fully accepted, to distinguish between labor and business monopolies on two grounds: first that labor monopolies are stronger and more privileged than business monopolies; and second, that a labor monopoly is everlastingly trying to increase its share, while the business monopoly presumably reaches its optimum price, having the elasticity of demand and the threat of entry in mind, and has no incentive to push further. However, the two limitations are not always very meaningful. In the steel industry, for example, the limits seem now to be set by substitutes, but steel product prices rose very rapidly during the 'fifties. In fact, a wise monopolist might not have given so much encouragement to aluminum and plastics. Government interferences likewise might have continuing tendencies to raise prices or be a one-time influence, as in the imposition of excise taxes. While labor power was given great emphasis in the papers, the discussion gave some recognition, regarded by the reviewer as inadequate, to business- and government-administered price rigidities with their ratchet operations.

The discussion of the effects of inflation ran the gamut from its being "evil in so many ways" to its having little influence on the rate of growth or the amplitude or pattern of cycles. At least, there seemed to be the negative conclusion that inflation was not a required condition for economic growth, although it would not necessarily destroy development. The effect of inflation on distribution emphasized its differential asset effects rather than income effects. In creeping inflation, clashes of interest may result in little actual change, but the clashes themselves are disturbing factors.

As to policies to control inflation, the variety of causative forces was paralleled by a similar profusion of possible remedies. Since any one policy instrument soon ran into diminishing returns, a multiple policy seemed better. While monetary and fiscal action received, as always, the most attention, antimonopoly policy, increased resource mobility, free imports, and limiting cyclical fluctuations all were recommended. There was an interesting though brief discussion concerning the possibility of there being an interest cost-push element as a consequence of monetary policy which might weaken the use of high interest rates as a deflationary factor. Perhaps credit-rationing with its direct quantitative effect is the better course. For many countries, the forces requiring governments to act are not rising prices per se but the reaction of the balance of payments to inflation and the necessity to halt the loss of reserves.

A commentary on a summary of a series of discussions is certain to be unsatisfactory, for it necessarily means very incomplete coverage. The above comments are intended to point out the sort of subject discussed rather than to be a substitute for reading the book. The reader who is truly interested will find much that is provocative if not conclusive, with many sidelights on recent history and policy in various countries. There are even some flashes of humor—the fact that the conference was held in Elsinore led to considerable speculation about the identification of Hamlet and, even more, of his ghost.

WILLARD L. THORP

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John R. Commons: His Assault on Laissez-Faire. By LAFAYETTE G. HARTER, JR. Oregon State Monograph, Studies in Economics No. 5. Corvallis: Oregon State University Press, 1962. Pp. xi, 283. $4.95.

Although, as Dr. Harter remarks, ". . . no student of Commons has written a book about him," it could hardly be said that he has suffered neglect. His many able and eminent students have manifested a degree of loyalty to his influence and devotion to his person that is virtually unique in American economics. He is recognized throughout the world as one of the leading figures in American economics of the first third of the century and as one of the three "founding fathers" of the "institutionalist school."

Except for matters of emphasis the present book says nothing new about Commons. However, it is the most complete survey of his career and achievements which has appeared to date, and its emphases are judicious and significant. Thus Harter's characterization of Commons' professional crusade as an attack on *laissez faire* is extremely shrewd. In contrast to Veblen, whose interests were almost wholly theoretical—the preconceptions of classical theory, the meanings implicit in "imbecile institutions," and the "common sense" of the "underlying community"—Commons was chiefly concerned with the way the institutions of private enterprise actually work and with the "institutional supplements," as Morris Copeland has called them, which they imperatively require.

In effect, Harter attributes to Commons three more or less distinct careers. In all three he was a pioneer. The first in point of time was that of student and historian of the labor movement. Although Ely's *Labor Movement in America* antedated him, and although Ely himself was responsible for bringing Commons to Wisconsin for the specific purpose of investigating the history of the labor movement, Commons' achievements in this area were so monumental that he is now generally and correctly regarded as *the* pioneer in this major field, as it is now understood.

Commons was no less a frontiersman in the field of social legislation. The story of his service as a one-man brain trust to the elder LaFollette is well known and is very well retold by Harter. In both of these areas Commons' former students and colleagues carried on his work with such conspicuous success that his importance as a historical figure would have been securely established if either career stood by itself.

In his third career, as an institutional theorist, Commons' position is somewhat anomalous. As Harter emphasizes throughout his treatment, Commons was an empiricist by instinct and temperament. He lacked Veblen's genius for playing with abstractions. Furthermore, as Harter also points out, he was not a luminous writer. In his theoretical works he is always difficult to follow. When his *Legal Foundations of Capitalism* appeared, Professor Sharfman slaughtered it as confused and pretentious. It was and is. But it was so, largely because Commons was struggling with a job no one else had tackled. Along with Veblen, he was reconceiving the meaning and significance of the basic institutions of Western society. If this is old stuff today, it is because of such pioneers.

As a whole generation of students have discovered to their sorrow, *Institutional Economics* also is far from being a luminous exposition of institutionalist theory. That such is the case was due, I think, in part to Commons' constitutional modesty. Whereas Veblen's total rejection of "marginalism" expressed his conviction that as a portrayal of economic actuality it was "pre-Darwinian," false and misleading, Commons' impatience with standard theory resulted rather from his acute sense of its limitations and especially its failure to provide a basis for imperatively needed reforms. Senator Morse, who as a Wisconsin undergraduate came into only indirect contact with Commons through Perlman and Witte, remarks in his introduction to the book that "John R. Commons virtually founded a school of thought of his own. . . ." It would be more accurate to say, as Harter does, that he, more than anyone else, created the field of "labor economics" and the profession of economic brain truster. He contributed his immense prestige to institutionalism, but very little else.

Nevertheless, some of his concepts may contain the germs of meanings on which future generations may set high value. Neil Chamberlain, while recognizing Commons' theoretical weaknesses, remarks that his genius was for the creation of concepts. Some of these, and especially that of the "going concern," have been adopted into the language. Recognition of Western society as a going concern notwithstanding its manifold defects and shortcomings, and realization of the importance of keeping it going, may be the answer to the challenge of total revolution.

Quiet, gentle, and unassuming as he was, John R. Commons was a great man. In assembling this sympathetic and understanding account of Commons' life and work Harter has done the profession a service; and the Press of Oregon State University, where Harter is an assistant professor, has done the world a service in publishing it. The careful reader will find a number of minor lapses. For example, we are told that Selig Perlman ". . . joined Commons at the University of Wisconsin . . . in 1900," although Harter has stated repeatedly and correctly that Commons joined Ely in Madison in 1904. Again, the text reads, "Although Commons spent years writing and rewriting his works on this theories . . ." These are slips which careful editing and proofreading should have eliminated. No text is perfect, and the quest for ceremonial adequacy is conspicuous waste. In substance, *John R. Commons: His Assault on Laissez-Faire* is an excellent study.

C. E. AYRES

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Economic History; Economic Development; National Economies

Economics for Development. By STEPHEN ENKE. Englewood Cliffs, N.J.: Prentice-Hall, 1963. Pp. xxii, 616. $11.35; text, $8.50.

"There must be more *economics* in economic development if there is to be more *development*," the author of this book says. He has done much to chart a path toward this goal. This alone is a valuable contribution to the field.

Part I (Chapters 1-3) is descriptive. It attempts to portray for the many who cannot live and work abroad "how the inhabitants of the underdeveloped countries tend to think and feel, how they earn and survive, and the important ways in which their economic situations differ." Much of the 25 additional chapters and two appendices is an economic analysis of problems occurring in underdeveloped countries.

Innovations are said to be "the most potent source of rising living standards." Hence Part II begins with a study of classical development theory, "for these once-revolutionary ideas still form the core of development theory today." There follows a study of development through science and technology (technology as a factor of production), a welcome chapter on the economics of innovation (should capital-poor countries favor capital-saving innovations?), and a section in which the author presents his thesis that increased agricultural productivity is generally a condition for industrialization and urban growth. The section is concluded with a study of growth models and some remarks on "stages" of economic growth.

Capital, combined with labor and technology, is treated as an important source of increased "productivity," a vague term that can be defined to account for much of the addition to national output in the advanced countries. Part III, therefore, is devoted to the accumulation and use of capital in underdeveloped countries with particular reference to government policies for increasing saving and investment, for establishing tax programs, and for meeting banking requirements.

Population problems that "can only offset government programs to raise per capita incomes" are treated in a number of ways. The rate of growth, size (ratio of labor force to population) and the hours of work (including work incentives), and government measures to reduce births (including the economics of bonus schemes) receive attention. A section on the bonus-vasectomy procedure, which the author has presented elsewhere, is stressed. Labor is given other dimensions in chapters on education and public health.

Growth through world trade—"the flow of inventions, entrepreneurship, capital, and finished goods in exchange for raw materials and unfinished goods"—is treated adequately, which is unusual for a text of this kind. A brief analysis of international capital movements including U.S. aid programs is presented.

Some aspects of India's third plan are in an appendix as a case study. There is a good bibliography classified by chapter subjects.

Theories of economic development are not treated in the style of Higgins or Meier and Baldwin; nor, with the exception of classical theory, is there any special effort to present in brief the theory of particular authors. Particular authors, however, are not overlooked. Schumpeter's contributions are noted in a chapter on science and technology; the Harrod-Domar contributions are noted in the chapter on models. There is no chapter on what Kindleberger calls "scale" or "transformation," but the ideas are there.

In general, the author believes that technical assistance and capital from advanced countries can be used to accelerate growth in the underdeveloped nations. He also believes the main source of economic progress must come from the people of these lands. Government can help by making new ideas and new ways respected, but government can also frustrate its people by holding out too much hope. Satisfactory growth can only be achieved, if at all, over a very long and trying period of cultural change.

Edward G. Nelson

University of Kansas

Economic Development and the Price Level. By Geoffrey Maynard. New York: St. Martin's Press; London: Macmillan, 1962. Pp. viii, 295. $7.50.

The appearance of this book at about the same time as Martin Bailey's *National Income and the Price Level* and the presumable overlap of topics, as indicated by some similarities in the titles, might invite comparison. However no serious comparison is intended, for whereas the latter is an examination of the state of macro-theory as a programmatic science (the separation of economics into positive and normative categories has only very limited value), the book under review is an examination of history—a few choice hypotheses, a few choice case studies, and a strong argument for balanced growth.

Professor Maynard divides his study into two parts. The first, "Some Theoretical Considerations," is an interesting, albeit repetitive, exercise in the economic logic of investment and growth under conditions of the currently backward and advanced economies and (for heaven's sake, why a separate category?) "institutional economies." The last category refers to the advanced economies since World War I, when their markets became increasingly characterized by imperfect competition. The second part, "Some Historical Evidence," consists of a group of summary surveys, admirably brief and relevant, of the United States, Great Britain, Japan, the Soviet Union, and Latin America, especially Argentina and Chile, focused on long-term investment, development, and price patterns.

The study examines the economies aggregated into two sectors: primary (largely agricultural) and industrial. The changing relative rates of growth of these two sectors result in changing terms of trade. The changing terms of trade, given demand, are essentially responsible for the secular price trends, excepting wars, that have affected the international economy, especially the economies of the West, over the last few centuries. The periods of relatively rapid growth with rising prices have occurred during periods when industrial growth has outstripped agricultural growth. Conversely when agricultural growth has outstripped industrial growth, whether by a great margin or not, the price level has tended to fall. The connection relating the changes in relative growth rates with changing price levels lies in agricultural inputs to industry as direct costs and, for given money wages, as the determinant of real wages. The lower the direct costs of agricultural inputs and the less the money wages (these seem to be correlatives), for given demand, the higher the profits, the higher the investment, the higher the rate of growth. The author argues that since farmers tend to utilize increases in income to accumulate hoards and/or decrease indebtedness, when the terms of trade turn against agriculture, the shift in the gains of trade to the antihoarding industrialist stimulates an increase in investment and economic growth.

This book serves as a warning to those who manage the destinies of the underdeveloped economies: do not neglect agricultural growth in the quest of economic development lest the terms of trade, in turning against industry, impede its growth!

One is left to puzzle: (1) Why do changes in the terms of trade stem only from the supply side as implicit in this thesis? Why can't we expect economies to be specialized in agricultural or industrial outputs and grow at satisfactory rates, since income elasticity of demand has not been of concern in this study? (2) Where, if agriculturalists are such notorious hoarders, do their debts arise that require centuries of debt reduction? When the international economy faced periods of inflation, how did such economies as that of Denmark fare? (3) How does a currently underdeveloped economy manage to avoid the perils of unbalance if (a) its factor proportions in fact dictate unbalance; or (b) its trading opportunities are limited by one of the regional trading "clubs" it is associated or unassociated with; or (c) any one of its sectors is unable or unwilling to submit to involuntary funding of the expansion or another sector—that is, if the primary sector does not wish to follow the example of the docile Japanese farmers?

It is apparent that no theory which purports to describe market economies can be considered complete or perhaps meaningful if it eschews even the simplest demand relations. This study suffers from this disability in its almost total preoccupation with supply. If incomplete, however, it is informative and it might be described as one of the better essays in economic history of its type.

Daniel Garnick

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Quiet Crisis in India—Economic Development and American Policy. By JOHN P. LEWIS. Washington: Brookings Institution, 1962. Pp. xiii, 350. $5.75.

This is a good book, warmly recommended to both specialist and general reader. Not only is it well written, insightful, and intelligent, but it also manages to avoid the unhappy choice between starry-eyed wishful thinking and the militantly unconventional wisdom of despair characteristic of so much of the literature on India.

The author takes as his point of departure what he calls the "procedural uniqueness" of the Indian experiment, i.e., her attempt to accelerate growth by constitutional means, and devotes the book to an assessment of the recent record and current prospects of this "political phenomenon requiring economic implementation." Wisely, this is attempted not by trying to be comprehensive about post-independence economic growth but by a highly selective analysis of specific problems and problem areas in the context of an appropriate over-all development strategy. This strategy is never very satisfactorily defined—economizing on foreign exchange (p. 22), for example, is surely a constraint, not a goal—but Lewis believes the strategy to be too savings-centered and aggregative and not focused sufficiently on specific "bottleneck" problems such as decision-making and skill shortages obstructing the efficient allocation of resources. Singled out for specific emphasis in this context is the agricultural sector, but with the refreshing difference that the weight of the argument is placed on basic rural reform and encompasses a well-documented plea for the dispersal of the growing industrial sector in townships larger than the tradition-oriented village and smaller than the congested city.

No one who has broken away from the spell of the articulate Delhi civil servant and ventured into the Indian countryside will fail to endorse heartily Lewis' concern that Indian strategy has thus far failed to ensure participation of the rural masses—failed, as he puts it, to get "millions of cultivators to do a lot of comparatively little things differently" (p. 145). But this correct diagnosis is, curiously enough, accompanied by warm support for the current heavy-industry emphasis, partly because "that is the way Indian authorities regard the matter" (p. 49), but mainly because agricultural reform and heavy-industry growth are viewed as noncompetitive. Admittedly the required changes in agriculture are mainly organizational, while those in heavy industry claim capital and highly skilled manpower, but it is entirely too facile to conclude that they can each be "pressed at maximum feasible rates" (p. 47) without interference with each other. The simple facts are that: (1) a successful agricultural transformation requires a simultaneous expansion of the industrial sector to absorb the released low- or zero-productivity agricultural labor force; and (2) continued emphasis on capital-intensive heavy industry will not provide the employment opportunities—and over-all productivity gains—that would be available if the same investment fund were "spread" over a large number of modernized but labor-intensive production functions. In spite of Lewis' acknowledgement elsewhere of the importance of small-scale industry and indigenous innovation, the redundant resource problem is not, as he claims (p. 50), "conceptually distinguishable" from the long-run expansion of output. The absorption of the surplus manpower cannot be viewed as part of a "supplemental development design" but must be an integral part of the basic strategy, seeking a variety of imaginative choices—extending beyond labor-intensive public works—between inefficient Ghandian handicrafts and inefficient heavy industry. Lewis' assertion (p. 48) that the shortage of foreign exchange plus the rising demand for the products of heavy industry which accompany rising income require domestic production of such commodities would, taken to its logical conclusion, lead us to abolish comparative advantage altogether. Especially since he seems elsewhere (p. 53) to be aware of the coincidence of output and employment objectives in the dualistic labor-surplus economy, this hearty endorsement of the current heavy-industry bias is hard to comprehend; surely it must be clear to him that in this fashion only a higher output per industrial worker, but not a higher per capita income for the economy as a whole, is assured.

The one other point on which we may note some ambivalence between Lewis' always keen diagnosis and the cure proposed is with respect to the role of government. In spite of his apt appraisal of the capabilities and limitations of the civil service he seems to retain a somewhat romantic picture of the government's role in achieving a "designed pace" for the development program via "carefully scrutinized and programmed requirements for net inputs" (p. 96), and a rather puzzling faith that it is government which can somehow ensure mass participation at the local level. No one—possibly not even Bauer—would wish to deny the importance of public action in providing national cohesion and leadership, social overhead facilities, etc.; but from this it doesn't follow that the government's meager bureaucratic resources can be successfully deployed to "run an allocative system that implements the development goals in a coherent, internally consistent manner while, at the same time, stimulating, not impairing, idle-resource activation and private productive energies" (p. 115). Lewis is too astute an observer not to note that the "planning and administrative structure tends very substantially to deaden the upward ferment of development impulses, ideas, and decisions originating at the local level" (p. 133) or that "the government itself . . . has shown comparatively little appetite for original, all-Indian, technical design" (p. 58). But his solution (same page) nevertheless calls on that same government to "foster" exploitation of the indigenous labor-using innovation potential. There seems to be a certain reluctance to recognize that the answer may, in fact, lie in the direction of an increasing role for the market mechanism—on efficiency, not doctrinal, grounds. At a minimum it may be necessary to weigh the "vagaries, delays and the perversities of market allocation" (p. 27) against the efficiency of direct controls administered by a nonspecialist civil service which, even with the best intentions and even if it somehow always knew what "right policy" was, would find it physically impossible to reach out to the millions of individual cultivators and small entrepreneurs.

Lewis' emphasis on planning for policy and structural change rather than for aggregate resources might well have led him to suggest that government concentrate instead on creating the conditions under which individual response mechanisms can be expected to operate more freely. But that is a matter for dispute. Beyond dispute is the fact that *Quiet Crisis in India* is a fresh and provocative book which manages to put its finger on the guts of the development problem.

GUSTAV RANIS

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Voprosy ekonomiki promyshlennosti SSSR na sovremennom etape kommunisticheskogo stroitel'stva (Problems of Economics of Industry of the USSR at the Present Stage of Communist Construction). By B. S. GERASHCHENKO AND V. S. GERASHCHENKO. Moscow: Publishing House of Economic Literature, 1962. Pp. 356.

This book (by two brothers, to judge by their common patronymic) is presented as a critical examination of the principal current problems of Soviet industry. However, it is of interest to Western readers more for a catalog of the problems of industrial planning and management which are identified and discussed by Soviet economists, than either for the acuity of the authors' analysis of these problems or for the imagination or daring of their suggestions for changes to solve them. Much of the book is descriptive rather than analytical in character, reflecting the authors' attempt to address a varied audience, including (according to an editorial preface) planning and managerial personnel in industry, university teachers and students, and propagandists.

The first three chapters of the book (about 100 pages) are entirely descriptive and rather dull (for Soviet as well as Western readers, I believe). They consist, respectively, of familiar figures on Soviet economic development from the statistical handbooks, a recapitulation of targets for the seven- (1959-65) and twenty- (1961-80) year plans, and a superficial discussion of technological progress that leans heavily on citations to the writings of Marx and Engels. The remaining eight chapters are meatier, containing not only a description of current Soviet practice but also some analysis of problems and some modest proposals for change. Individual chapters are devoted to industrial planning, labor productivity and incentives, economic cooperation among the Communist countries (a brief survey that is not pertinent to the rest of the book), enterprise management, the role of profit, cost reduction, effective use of fixed and working capital, and the financing of industry. All of these chapters treat industry as a whole, without considering specific problems of individual branches, such as machine building, chemicals, or mining.

The authors identify a number of shortcomings in the operation of Soviet industry and offer some recommendations for changes in planning and administration. The changes which they propose are, however, moderate rather than drastic and well within the present Soviet framework of centralized control based primarily on physical planning. In fact, the Gerashchenkos show a lack of confidence in the devolution of authority to regional economic administrations or to enterprise managers, calling for more centralized decision-making in various instances. For example, they oppose the redistribution of retained profits among enterprises by regional economic councils, advocating instead that any retained profits in excess of the needs of individual enterprises earning them should be transferred to the central authorities as budget revenues.

Similarly, the Gerashchenkos take a conservative position on the interrelated questions of capital charges, profits, and prices. They favor calculating the "effectiveness" of the use of fixed assets, expressed by the relationship of profits to fixed assets, as one of various "indicators" of enterprise performance. But they do not endorse a capital charge, nor do they support any of the proposals for fundamental reform of the price system advanced in recent years. Their most radical suggestion in regard to investment is to finance the reconstruction and modernization of existing enterprises by "long-term" (five to six years) bank credit, instead of budget grants as at present. Budget grants would, however, still be used to finance investment in new enterprises. Although the five- to six-year recoupment period suggested implies a minimum rate of return on investment, the authors do not pursue the point. Their principal reason for favoring bank credit financing is to increase the supervision of the financial authorities over the execution of the investment plan —in Soviet parlance, to "strengthen control by the ruble." The entire subject of investment criteria is ignored, presumably on the ground that it is an aspect of "planning" rather than the "economics of industry" and therefore outside the scope of the book.

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Accounting in Soviet Planning and Management. By ROBERT W. CAMPBELL. Cambridge: Harvard University Press, 1963. Pp. 316. $5.50.

I believe all students of the Soviet Union will be grateful to Professor Campbell for publishing his study of Soviet industrial accounting and its relation to the management of the Soviet economy. The book is valuable because it throws light on the highly important, though relatively little-known, role played by Soviet accountants and the tools and methods they have developed to provide information to management for the operation and control of the gigantic economic machine developed by the Soviet Union since 1920.

Accounting serves two cardinal functions in a modern economy. It provides information for economic decision-making, and it provides information for checking and controlling labor performance. The Soviet economic system with its centralization of planning and controls depends entirely on accounting information for the smooth and efficient operation of its economy. As a result, as Campbell points out, the activities of Soviet enterprises are covered by a system of accounting probably more comprehensive than that in any other economy of the world.

The salient topics covered by the author are the interrelationships between cost accounting, pricing, and economic decision-making; the value and depreciation of fixed assets; the impact of Soviet accounting methods on economic decisions; and problems of inventory control. The other interesting comments found in this study deal with the place of the accountant in the Soviet industrial world, his duties, the accounting methods and practices used in the Soviet Union, and what is being done with the tremendous flow of accounting data generated by the system.

Ostensibly these data provide basic information to Soviet planners on which to base their decisions and to enable them to have better control over the economy and the bureaucracy that operates it from day to day. However, as Campbell indicates, Soviet accounting has considerable weaknesses in terms of the major functions it is supposed to provide because the cost data supplied by the profession are often inappropriate for economic decision-making. The important features of Soviet accounting, in actual practice, show slow reporting of cost data and occasional disregard of and confusion from the instructions and circulars of the Ministry of Finance regarding specified rules for accounting and reporting.

Among the different functions assigned to Soviet accountancy, plan fulfillment appears among the most important; therefore, accounting practices and rules have been tailored to this objective, but in the process concepts of cost and value often have been distorted. Depreciation is considerably understated, joint costs are arbitrarily arrived at, and a great deal of confusion is found in the treatment of obsolescence. Frequently accounts are padded, falsified, and shifted in order to satisfy the planners and to earn bonuses that depend on plan fulfillment. Most readers of Soviet professional periodicals are well aware of the continuous stream of complaints and criticisms of the Soviet methods and practices regarding accounting and economic data. In addition, Marxian orthodoxy often confuses and interferes with the calculation of economically relevant costs and with effectiveness of controls. As a result Soviet accounting concepts are inappropriate for economic decision-making. This situation is partly accounted for by the pressure of time due to a lack of computing equipment and the relatively low skills and education of Soviet accountants.

Some forty years ago Lenin made the point that: "Accounting and controls—are the principal thing that are necessary to the 'setting up' and correct functioning of the first phase of Communist Society. . . . The accounting and control necessary for this have been so utterly simplified by capitalism that they have become the extraordinarily simple operations of checking, recording and issuing receipts, which anyone who can read and write and who knows the first four rules of arithmetic can perform."[1] Lenin was neither an accountant nor an economist, but he instinctively realized the importance of accounting data in a centrally controlled economy. However, his naive view that the job of an accountant can be performed without adequate training is apparently still shared by his heirs.

The short glossary of Soviet accounting terminology at the end of the book is particularly welcome. One of the difficulties that students of Soviet economics have is in understanding the meaning of Soviet terms, due to a rather careless use of these terms in books on various aspects of Soviet life without adequate explanations of their meaning.

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[1] P. J. D. Wiles, *The Political Economy of Communism*, Oxford, 1962, p. 55.

L'Inde indépendante. BY CHARLES BETTELHEIM. Paris: Libraire Armand Colin, 1962. Pp. 525. NF 30.

This book is a survey of the Indian economic, political, and social scene by a scholar of the Sorbonne who has made several trips to India and participated in work at the Indian Statistical Institute at Calcutta. He is also a Marxist: thus we have the useful experience of a Left view on the growth problems of this major country of Asia and the world.

The book is divided into two parts, "India after Independence," and "Toward the India of Tomorrow." The subjects of the chapters indicate the pattern of coverage and emphasis. In the first part, they include agriculture's production and problems, with its economic and social structure; industrial production and structure; the *bourgeoisie* and capital; the petty *bourgeoisie* and the proletariat; and political structure. In the second part are: the tendency toward state capitalism and the five-year plans, the development of agriculture and agricultural policy, industrial progress, economic relations with foreigners, and the evolution of the standard of living; social struggles and political life. The main text is supplemented by appendices on the third five-year plan, the elections of 1962, biographical notes on notable Indians, and a good index. There is an appreciable amount of statistical data. The book is therefore a survey on the grand model of the India of our time.

What is the vision of the Indian economy and society held by this knowledgeable Marxist professor? Its social structure is that of "a country essentially rural and agricultural, where there remain numerous economic and social relations of feudal or semi-feudal type, and which is still partially dominated by foreign capital" (p. 177). Agricultural development in the 1950's has been modest, an index of crop output rising only 2.5 per cent a year (pp. 230-31). "The feebleness of the agricultural development of India is . . . the more tragic in that the countryside displays vast resources of unemployed or underemployed labor which, in another social order, would be able to contribute to a powerful development of the productive forces if they were systematically applied to the work of land improvement, irrigation, drainage, reforestration, the fight against erosion, etc. The example of China in the course of these last years shows with what rapidity agricultural production is able to grow, thanks to putting to work such an unemployed potential" (p. 60).

"Industry has experienced the soundest growth . . . however. . . . The progress of industry does not constitute all net gains for Indian society. A part of this progress is directed to satisfy the growth of demand coming from the most comfortable social classes, while basic and essential needs continue unsatisfied" (pp. 445-46; also pp. 293 ff.). Further, a development dangerous for the future is that basic and intermediate industry has not been developing in proportion to the last industrial stage. The effect of this disproportion is unfavorable to foreign trade and the balance of payments (p. 446). The disproportions have multiplied the links between Indian and foreign capital, with only a partial offset from the development of state capitalism.

Aid from the Communist-bloc countries, though small compared to that from non-Communist countries, has been important, Bettelheim judges, in that it has helped to build up basic and intermediate industry; and, because of political competition, has stimulated a rapid growth in aid from non-Communist countries.

The Marxian type of crisis is seen to sharpen. The urban small *bourgeoisie* expand in the gaps left by the growth of monopoly capitalism and state capitalism. The artisan and traditional small trading groups suffer a slow and unequal deterioration in their position. The industrial proletariat suffer from increased unemployment and underemployment (pp. 448-49). In the country, the most comfortable rural classes are transformed slowly into rural capitalists, while the agricultural workers, poor peasants, and traditional artisans see their conditions of life stagnate or deteriorate, mainly from underemployment (p. 456). Foreign indebtedness will grow, and one day may permit the principal creditors to impose on India the economic policies of their choice (p. 453). Population pressure is not a problem of strategic concern (p. 10). The prospect is that, under its present organization, India is condemned to experience slow and dependent development (p. 454).

The general international lesson of Indian experience is that "underdeveloped countries are not able to obtain rapid economic growth except on condition of 'profound social transformation'" (which is Bettelheim's phrase for a Leninist revolution). The full utilization of resources must not be limited by private property and profit seeking. The social transformation "must permit, to the country that undertakes it, the total mobilization of the human, technical, and material resources, in order to place them at the service of the whole society" (p. 462). Underdeveloped countries that lack the domestic resources of India can follow "the Indian way only at the risk of much more modest progress, and of arriving more quickly at brutal contradictions" (p. 465).

So much for Bettelheim's general views, which give a more orthodox impression than many of his detailed analyses. To evaluate a general interpretation is a prolonged process beyond our space limit. But we can test briefly several quantitative conclusions of Bettelheim against available data.

(1) He sees private industry as aiming at quick profits and avoiding long-term commitment of capital (p. 215), in that during the Second Plan it devoted only 69 per cent of its industrial investment to basic industry, as compared to 98 per cent of public-sector investment (p. 327). Passing by the validity of the standard Communist priority for heavy industry as compared to light industry,[1] we may note that the government policy statement of

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[1] John Lewis, in his recent perceptive study of Indian growth problems, *Quiet Crisis in India* (Brookings, 1962), pp. 38-45, judges that adequate justification for emphasis on heavy industry does exist in India's need to economize on foreign exchange in the long-run future, coupled with its favorable domestic-resource mix. The growth of basic industry will eventually diminish claims on foreign exchange, which cannot be built up adequately, in prospect, from a pattern of exports subject to sluggish demand. The Indian development scheme, he reasons (p. 41), "calls for very heavy foreign exchange deficits temporarily as a means of forestalling an endless, chronically unmanageable foreign exchange crisis later on."

1956 limits private-sector participation in basic and strategic industry, and gives it freedom of action only in consumer-goods production.[2] No wonder that private industry did not devote as large a share of its investment to basic industry as did government.

(2) The Communist countries are commended for aid supporting basic industry, in contrast with the mixed aid obtained from non-Communist countries. In fact, the data show that the United States, the United Kingdom, West Germany, and Japan have contributed many large grants for capital goods, including support for specific heavy industries; and that one large USSR loan for heavy industry utilized during the second five-year plan, for the Bhilai steel plant, is conspicuous in forming the total picture of Communist-bloc support.[3] Also, one should remember that aid in the form of foodstuffs or of other consumer goods can free foreign exchange and domestic resources for investment in heavy industry.

(3) Bettelheim indicates that upper-income landowners and industrialists have benefited more than other groups since independence. In a brief but careful survey of six different estimates of income shares, Mukherjee comes out with notably cautious conclusions. The estimates agree that wages and salaries are from 28 to 32 per cent of domestic product. Mukherjee judges that "the accuracy of the estimates and the degree of comparability achieved do not warrant any unequivocal conclusion being drawn on the change in factor distribution of national income over the period."[4] Kuznets' data indicate that the share of the top one-twentieth of income units in India fell nearly 10 per cent between 1950 and 1955-56, though the share of the bottom 40 per cent also fell (by 3.5 per cent). All the intermediate groups gained.[5]

These data do not justify a firm conclusion on changes in the distribution of income; their construction involves qualitative judgments as well as uncertainties in the original statistics. But they are the best we have. They do not support the repeated indications (pp. 197, 200, 445-46, 457) that upper-income groups are securing growing importance in the Indian economy.

Bettelheim's image of India is a Leninist rather than a Marxist one. Marx was willing to praise in glowing terms the achievement of capitalism in raising society from its feudal level. A good Marxist, one expects, often would judge that capitalism is the best engine for progress in many an underdeveloped economy for decades to come. But Bettelheim sees for India a slow and probably diminishing rate of growth as alone compatible with social stability (p. 459). He finds China a bright example of how resources can be totally mobilized toward growth.

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[2] *Monthly Statistical Commentary on Indian Economic Conditions*, Nov. 1959, p. 21: Agriculture, foodstuffs, textile, etc. companies as compared with companies in mining, metals, chemicals, electricity, etc. See also *op. cit.*, Jan. 1962, pp. 10-12.

[3] *Monthly Statistical Commentary on Indian Economic Conditions*, May 1962, pp. 16-17.

[4] M. Mukherjee, "On the Available Estimates of the Breakdown of National Income by Distributive Shares in India," in *Papers on National Income and Allied Topics*, Indian Conference on Research in National Income, Bombay 1962, p. 36. See also p. 29.

[5] S. Kuznets, "Quantitative Aspects of the Economic Growth of Nations: VIII—Distribution of Income by Size," in *Economic Development and Cultural Change*, Jan. 1963, p. 13. The original statistics are from studies of M. Mukherjee and A. K. Ghosh, and of H. F. Lydall, based on income tax returns plus survey statistics.

Indian leaders, in contrast, even when economic growth appeared to be progressing rapidly in China, were repelled by the human costs of the totalitarian effort. Nehru urged that India should not "sacrifice democratic institutions on the altar of economic progress."[6] The same emphasis is present in John Lewis' analysis of Indian growth problems and policy: "The unique dignity of the Indian development experiment and the unique importance of its success to the United States stems from India's faithful adherence thus far to a constitutional mode of development."[7] Lewis, however, judges that adherence to constitutional methods will be tested strenuously before the decade is over. He is as pessimistic from his point of view as Bettelheim is from his: Bettelheim does not think that we shall see any deep and major change of political scene in the near future (p. 457).

The faithful Leninist orthodoxy of such a diligent student of the Indian scene as Bettelheim is impressive. It is an example to all of us of the strait-jacket into which our theories can force and shape what we see. At the same time, the analysis does describe patterns of social and political pressure worth consideration in any analysis of Indian development.

Obviously private-ownership economies can grow rapidly or can stagnate, and centrally directed and centrally owned economies can progress rapidly or can enmesh themselves in repeated crises. If it should happen that the present agricultural and industrial stagnation in Communist China continues during the next decade or so, while the uniquely rapid growth of the Japanese economy also continues, many a Communist economist would be hard pressed to become more realistic and more flexible in his interpretations. And this would be to the good.

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[6] Quoted in Wilfred Malenbaum, *East and West in India's Development*, National Planning Association, Washington 1959.

[7] *Op. cit.*, p. 333.

Problems of Growth of an Underdeveloped Economy—India. By NASIR AHMAD KHAN. New York: Asia Publishing House, 1961. Pp. 441. $10.00.

The major objective of the present work is to analyze the characteristics and behavior of different sectors of the Indian economy. It is an extension of the author's Ph.D. dissertation submitted to Lucknow University. The original thesis was revised after further research at the University of Chicago.

Part I deals with the supply conditions in agriculture and industry in the short and long run (1911-51). The chief feature of the Indian economy is its heavy reliance on agriculture, combined with the relatively inelastic food supply and the large increase of population. There is a remarkable constancy in the area and yield of food crops and in agricultural production as a whole. However, even after a considerable increase in industrial output, which forms a very small part of total output, productivity per man has not shown any marked improvement.

The long-run movements in agricultural production are intertwined with random fluctuations of 3 to 4 years and major fluctuations of every 7 or 8 years. In industry the short cyclical movements are roughly 5 to 6 years, and the major cycles are 11 to 12 years. The relative inelasticity of supply of agricultural output, especially during a depression, has been noted by many. The author also observed that (a) rice and wheat production shows smaller deviation than other agricultural or industrial commodities, and jute the largest; (b) cotton and jute manufactures have a smaller range of fluctuations than the cotton and jute crops; and (c) industrial production as a whole indicates larger fluctuations than agricultural production as a whole.

Part II considers occupational distribution of population, net output and relative incomes, wages and consumption patterns. Colin Clark's well-known hypothesis was tested by the Indian data. The percentage of labor engaged in primary industry remained the same between 1911 and 1931 and declined only in 1941. Approximately 70 per cent of the total labor force is still in agriculture. Only 11 per cent of the labor force is employed in service industry. The percentage of national income spent for food and primary products was highest in India (24 per cent), while real income per capita was the lowest among all the countries compared. In the United States only 8 per cent of income is spent on food.

In Part III Khan takes up the problems of industrialization in India and evaluates the experiences of the five-year plans. The task facing India today is that of diverting the surplus labor from primary to secondary and tertiary occupations. This can be done if the rate of expansion of the nonagricultural sector is greater than the rate of increase of the working population. As a result of the development programme of the first five-year plan, agricultural production increased by about 21 per cent during 1950-51 to 1955-56. Industrial production increased by 38 per cent during 1950-56. However, the first five-year plan ended with about 5 million laborers still unemployed. The second five-year plan set forth as its main objective the creation of jobs for 1.8 million new laborers entering the market each year, in addition to the 5 million unemployed. Net capital formation in the second five-year plan was expected to increase from 6.8 per cent to 11 per cent of the national income. But Khan points out that this expectation is too high in the light of past trends. The rate of saving in the economy is not likely to be more than 8 per cent. It is expected that there will still be 7 million unemployed at the end of the second five-year plan.

The concluding chapter discusses certain aspects of coordination and decentralization, capital utilization, reorganization of agriculture, institutional bottlenecks, and requirements of balanced growth. As the author correctly points out, when adjustment between the agriculture and industry sectors is successful, underdeveloped nations are well on the way to economic development. If agriculture is to progress and develop, both fundamental and applied research is necessary. The family farms are too small to engage in organized research and too poor to introduce better methods. Such research must, therefore, be institutionalized, and the government has the responsibility to organize and undertake agricultural and economic research.

Although the reader may sometimes find it very difficult to see the point of emphasis, the presentation throughout is detailed and thorough. The book is not strong in analysis but contains a wealth of data concerning the Indian economy. It reflects the joining of careful statistical survey with the testing of the implications of basic relationships among economic variables. No person concerned with economic development, planning, and policy can afford to miss this work.

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Economic Development in Burma 1951-1960. By LOUIS J. WALINSKY. New York: The Twentieth Century Fund, 1962. Pp. xxvii, 680. $10.00.

In 1951, the firm, Robert R. Nathan Associates, was engaged under a U.S. foreign aid grant to formulate a Burmese national development program and subsequently retained by Burma to provide economic advisory services during the plan's implementation. Enjoying the confidence of the Prime Minister, U Nu, the firm of economists actively participated in the country's economic decision-making process until the premature termination of its contract by General Ne Win's caretaker government in 1959.

As general manager and chief economist of the Nathan field staff, the author of this book therefore had a unique vantage point for viewing the Burmese experience. Although at times sounding like a personal apologia, this volume is probably the most comprehensive, reliable, and intimate account yet written by an American of an emerging nation's political-economic development.

After sketching the economic background of Burma and describing the consultants' development program in the first two parts, Part III is used to present a chronological account of the efforts to implement the plan in a changing economic setting, dimly perceived at first because of statistical deficiencies, with emphasis upon the institutional and personal background of economic decisions. The naive optimism which equated independence with abundance (U Nu in 1952 predicted that every family would have a car in the not too distant future; present rate—500 people per car) was quickly shattered as the economy, balanced on a tightrope with inflation on one side and foreign exchange losses on the other, moved from crisis to crisis.

The implementation of the development program by major sectors and projects is discussed in Part IV. Burma's annual growth rate of 3.5 per cent in per capita output was not unsuccessful except in relation to expectations and to potentiality, for this was largely reconstruction of war-damaged social overhead. Per capita output was still about a fifth below the prewar level and the plan's target at the end of the decade. However, the author fails to concede that the Burmese could legitimately measure progress by alternative criteria—Burmanization, nationalizaton, industrialization—by which standards considerable progress was realized. In the consultants' frustrated attempts to raise real income levels, these distinctly Burmese goals are viewed as obstructions rather than desiderata, a perspective that imparts a Western cultural bias to the volume and must have impaired the value of the consultants' services. Nevertheless, toward the end of the period U Nu did grant concessions to private enterprise in recognition of the incompatibility of the public corporations' inefficiency and goals of increasing real income, although the effect of these tentative moves has been submerged in the socialistic program of the military, which assumed first temporary and now indefinite control.

In Part V the problem areas in the program's implementation are treated, chief among which, in the author's view, was the subversion of the civil service by the party machine for personal and political objectives, coupled with a hoarding of authority which crippled the decision-making process. Defects in the plan itself are also considered, particularly the grossly erroneous estimate of rice export prospects, both price level and production; 1960 export earnings from rice were half of the consultants' more "pessimistic" forecast. He concedes that the price decline undermined the government's confidence and absorbed the major efforts of those concerned with economic development, but he is on tenuous ground in denying that the price fall was predictable or that the psychology of easy financing stimulated an unrealistically ambitious development program. If the need for foreign assistance had been delineated at the outset of the program rather than sought under the compulsion of a dwindling reserve position, its use could have been more clearly evaluated as one of the costs of development.

The length of the volume is due to the repetitive presentation of materials in each of its four major parts. Continuous reading consequently is tedious, but selections relevant to one's interest can be read independently because of their self-contained nature.

Like Burmese development during this period, the opportunity cost of this study is great, for the author's unique position and field-staff support permitted a more searching analysis of economic issues. He evidences no theoretical strategy for Burmese development. Certain rough judgments were made, such as a higher marginal productivity of capital in agriculture than in industry and the essentiality of regaining Burma's prewar rice-export position, which, "influenced by ideological considerations, by flair, by 'feel,' by political sensitivity, by questions of practical readiness" (p. 596), constituted the decision-making framework. His conclusion in the final part that "evidence can be culled from the Burmese experience to support almost any theory" (p. 593) may indicate the disparity between the theorist and practitioner of economic development, but it is disappointing to those who look to empirical studies of this nature to bring the two into meaningful contact. However, the author does believe that Hirschman's concept of imbalanced development best describes the painful educaton in economics which the Burmese political leaders received too late to put into practice.

Laurence D. Stifel

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Statistical Methods; Econometrics; Social Accounting

Simulation of Market Processes. By FREDERICK E. BALDERSTON AND AUSTIN C. HOGGATT. Berkeley: Institute of Business and Economic Research, University of California, 1962. Pp. 281. $4.00.

In this study an elaborate model of a market is constructed in computer language. The implications of the model are then developed by allowing the model to run on a computer under certain experimental conditions.

The model is based on observations made on the firms in the Pacific Coast lumber industry. The market is composed of manufacturers, wholesalers, and retailers. The number of each of these units in the simulation varies because there is entry and exit. At the maximum there are 30 manufacturers, 15 wholesalers, and 60 retailers. The wholesalers function primarily as communication channels between manufacturers and retailers. The actual commodity is shipped from manufacturer to retailer to consumer. The firms at each level make the same decisions (though not necessarily in the same way) as firms in the real world. Each manufacturer must set a price and determine an output. In addition, the firm must determine which wholesalers it prefers for trading purposes. The wholesalers' major decisions are the setting of preferences on manufacturers and retailers. In addition the wholesalers drive the model by sending search messages and attempting to effect transactions between retailers and manufacturers as a result of the information received. Retailers in turn determine prices, quantities to be sold, and set preferences on wholesalers.

There are two parameters that are modified experimentally—the message cost and the method of setting preference orderings. The former has four values ranging from $0 to $192. The latter has two variations, one based on past transactions and one based on random choice. The authors proceed, then, to have two runs for each of the four message costs—one run with preferences based on experience and one run with preferences based on random choice.

With the eight runs of the model the authors create a world which they analyze by the classical procedures of constructing hypotheses and testing these statistically with the data from the runs. The elementary hypotheses are tested first. By elementary the authors mean essentially two variable relationships that ignore the interactions in the total system. These hypotheses relate to the stabilization of the market, the effects of increased message cost, and the effects of the changing preference conditions. These hypotheses (some 33 in total) are used in large part to relate the model to classical theory. In addition the authors examine the model's output in relation to a number of important questions such as over-all market efficiency, plant size, and the distribution of firm size.

The work throughout the book is of high technical quality and imaginative in its conception. Certain notions of market segmentation are of interest and may have potential beyond the use of the model. The attempts to isolate the causal mechanisms of the model are also an important step in the use of simulation models.

Basically we have here an experiment that is in many ways analogous to a laboratory experiment. The authors have constructed the firms using the West Coast lumber industry as a rough model. For example, the initial assets of the firms in the model were representative of the empirical situation. The basic problem in this case is the same as that faced by the lab experimenter or, for that matter, the mathematical model builder. How does one go from the results of the model or experiment to the real world? The authors do not discuss this problem directly. They seem to feel that the study is theoretically oriented and one cannot do everything in one study. Nevertheless, if the work is to have an impact, there must be empirical testing with data from the real world.

One of the virtues of the computer language is that models can be generated that result in testable propositions, perhaps more easily than with other languages which the economist uses. However, the testing phase cannot be eliminated merely by capturing some of the real world in the units of the model. Further, in the Balderston-Hoggatt model the decision rules are not necessarily based on empirical observation. It therefore becomes even more important to move to the real world.

As a theoretical study of market processes, however, the book adds to the economist's knowledge of market behavior. Both because of the theoretical contributions and because of the imaginative use of a new methodology, the book is important for economists interested in microbehavior.

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Economic Systems; Planning and Reform; Cooperation

Ekonomicheskoye sorevnovanye sotsialisma s kapitalismom (Economic Competition Between Socialism and Capitalism). Edited by V. A. JAMIN. Moscow: Publishing House of Social-Economic Literature, 1962. Pp. 502.

This book consists of 31 articles organized into 10 chapters dealing with various aspects of the subject. It is a fairly comprehensive compilation of Soviet views on the theory and practice of competition between the two social systems.

The theory can be paraphrased briefly as follows: competition between capitalism and socialism is viewed as a contest between a system that has outlived its usefulness and its historic successor. The eventual victory of socialism is deemed inevitable. This is supposed to occur mainly as a consequence of the growing internal stresses within the capitalist society. In addition, the rising economic power of the USSR and its allies will tend to draw the world as a whole, and the less developed countries in particular, into the Soviet orbit. As a result, the hegemony of the United States will be replaced by that of the Soviet Union. At the same time, the example of the coming higher standard of living and higher cultural level in the socialist part of the world will influence more and more countries to adopt socialism. Unable to resort to war due to the increasing military strength of the socialist bloc and to the fear of atomic suicide, capitalism will be forced to abandon its dominant position.

Some of the alleged principal causes of the expected triumph of the socialist bloc are ample natural resources, planned economy that makes uninterrupted development and a high rate of reinvestment possible, a high level of international division of labor within the bloc, and a faster technological progress inherent in socialist economy.

The stated immediate goal of the socialist bloc is to surpass the rest of the world in total and per capita output, productivity of labor, and standard of living. It is pointed out, however, that the composition of the product mix in socialist countries is to be determined by scientifically worked out norms rather than through a blind imitation of the advanced capitalist countries.

The difficulties of comparing the two economies are examined, but judged to be surmountable. It is admitted that some of these difficulties arise from the failure of Soviet statistics to supply sufficiently detailed data. However, Western economists are accused of exaggerating this whole problem.

The work contains much statistical information on the relative levels and rates of development of the USSR as compared with the United States and Western Europe. The quality of the accompanying analysis is very uneven —some articles are on a fairly high plane, while some others are not above resorting to arbitrary selection of statistical material.

The average level of both Soviet industrial production and national income was placed at 60 per cent of that of the United States in 1958-59, productivity of labor at 42.4-49.7 per cent, but between 10 to 31 per cent higher than in the principal Western European countries. The per capita personal income was stated to be about 40 per cent of the United States' level. In this reviewer's judgment, but contrary to the opinion of many Western students, these ratios, with the probable exception of the one referring to per capita personal income, are fairly realistic.

On the basis of the planned 8.6 per cent yearly increase of industrial output for the USSR and an estimated 2½ per cent for the United States, the former country is expected to surpass the latter by about 1968 and to attain a level more than double that of the United States by 1980. A low rate of growth for both the United States and the rest of the capitalist world is contemplated, due to a gradual exhaustion of factors of growth as exemplified by completion of postwar restorations and a gradual saturation of demand for consumer goods. It is also claimed that productivity of capitalist economies will, in the long run, suffer from their increasing militarization, which will tend to use up the resources that otherwise could be directed into production of capital goods. At the same time it is claimed, in another chapter, that socialist countries have the means to overcome this dilemma.

As a matter of fact, the above prognosis now appears somewhat outdated, due mostly to the better actual performance by the U.S. economy between 1958 and 1962. As a result, Soviet industrial output, in spite of its somewhat faster-than-planned growth, is unlikely to catch up with that of the United States before the 1970's.

The authors of the book foresee that the socialist bloc as a whole will produce 50 per cent of world industrial output by 1965. This sounds illogical because Soviet output in 1965 is still expected to be smaller than that of the United States, which implies that the rest of the socialist bloc is expected to outproduce the entire capitalist world without the United States by 1965. Apparently the chief cause of this gross error is a totally unrealistic plan for Red China.

We will end this brief synopsis with a conclusion that, if viewed through a critical prism, this book offers interesting and informative reading.

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Geld und Währung—Gesammelte Abhandlungen. By FRIEDRICH A. LUTZ. Tübingen: J. C. B. Mohr (Paul Siebeck), 1962. Pp. 267. DM 29.50.

The Walter Eucken Institut could hardly have chosen a more fitting first volume of a new series than the present *Festschrift* for the occasion of Professor Lutz's sixtieth birthday. Eucken has long been recognized as one of the main figures of German liberal economic thought and the present volume proves that Lutz—a student of Eucken—has carried on the tradition to the present generation. There is a variety of contributions in the present collection: the first essay was originally published in a German journal in 1935; the last is a translation from the English of a lecture given by Lutz in Stockholm in the spring of 1961; the longest piece, a reprint of a pamphlet long out of print, takes up almost a third of the volume; the shortest, but a few pages long, reproduces an essay published in a Swiss newspaper in 1960. Such variation should not be surprising to the *habitué* of the German *Festschrift;* striking, however, is the consistency of Lutz's economic philosophy, which he applies with equal candor and skill to the domestic monetary problems of Hitler's Germany in 1935 and to the international monetary problems of the United States in 1961.

Many of the postwar contributions reprinted are well known to Anglo-Saxon readers; the earlier interwar essays are not; among these the longest selection, "Das Grundproblem der Geldverfassung," should prove the most interesting. Actually, there is not just one but three problems of monetary management which, according to Lutz, must be solved: (1) Does the government, or any other institution responsible to it, effectively control the quantity of money in the economy? (2) Is the safety of money guaranteed? (3) Can commercial banks at the same time remain reasonably free from the government and monopoly, or, to put it differently, can they operate in a free and competitive manner? By applying these criteria to various countries and different periods, Lutz shows clearly and elegantly that the very effort to solve one or two of these problems may *eo ipso* conflict with the successful solution of another.

The English money market is really the only one among those examined that gets a high grade, if not the only passing grade. This is true for 1844 as well as for the 1930's. After giving a succinct account of the positions of the Banking School and the Currency School, the author praises the legislation of 1844:

> Our basic problem appears to be fully solved: the creation of money was taken away from the private sector and entrusted to a central authority, removed from the realm of free competition. Bank notes are covered 100%. Thus they are safe. On the other hand the business of banking is left to private institutions who really compete with each other. All three partial problems which add up to our basic problem, appear to be on the way to final and satisfactory solution.

Similarly Lutz thinks that in the 1930's England was equally fortunate in being able to solve the basic monetary problem. The Bank of England, through the skillful use of monetary weapons, including the "scissors action," remained clearly in the saddle and kept control over the quantity of money. Avoidance of excessive risks and maintenance of a high proportion of capital to other commercial bank liabilities guaranteed the high quality of deposit money. Only as to the third area, i.e., competition among banks, is the author less certain that England has succeeded.

The United States does not come off as well.The Federal Reserve System in the early 1930's, according to Lutz, "danced to the tune of the member banks" rather than the other way around. This unfortunate state of affairs was due to the fact that the Federal Reserve rediscount rate was lower than the customer rate; add to this the author's lack of faith in the often professed tradition against indebtedness at the Federal Reserve and the case for weak central banking is made.

Imprudent risk-taking by commercial banks and the shrinkage of capital as a percentage of commercial bank liabilities (according to Lutz the percentage dropped from 23.7 in 1873 to but 11.6 in 1929) contributed to the inability of the U.S. banking system to weather the storm of depression, as the British were able to do.

While Lutz grants that a certain competitiveness among U.S. banks existed prior to the great depression, he feels that the banking and monetary legislation of the 1930's—of which he approves from the point of view of the other basic monetary objectives—has reduced the freedom of action for commercial banks and thus the degree of competition in the money market.

The lowest rating, in the opinion of this reviewer, is given by Lutz to the German banking system. The Reichsbank had essentially only one monetary weapon at its disposal, rediscount policy, and even this was not as powerful as it could have been. The quality of German deposit money is also found wanting for reasons similar to those given in the case of the United States. Most of all, Lutz feels that German banks have eliminated competition through cartel arrangements, that they have regulated and bureaucratized "the fun out of banking." Economic laws have been violated because the cartel has prevented the individual bank from increasing or decreasing the rate of interest in a sufficiently wide range in relation to the degree of risk assumed.

The monopolistic rigidity of the interest rate also kept it from playing the role assigned to it by Böhm-Bawerk and Wicksell in the allocation of capital.

F. A. Breier

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Financing American Enterprise—The Story of Commercial Banking. By Paul B. Trescott. New York: Harper and Row, 1963. Pp. xiii, 304. $4.50.

Recently economists have become interested in the process of capital mobilization and in the effects of barriers to mobilization on the shape and direction of development. Students of U.S. growth have, however, found it difficult to assess the role of commercial banks in the capital-mobilization process. Despite a vast literature on the political aspects of banking, on cyclic responses to changes in bank behavior, and on the efficacy of central bank regulation, there have been few attempts to examine bank investment patterns and the impact of these patterns on U.S. development. It is to these questions that Professor Trescott addresses himself.

During the past three decades the "new" school of entrepreneurial history has produced a number of business and commercial biographies. Thus far, few economic historians with interests wider than a single firm have availed themselves of the growing body of materials. Trescott, however, has drawn heavily on these histories in his attempt to assess the contribution of the commercial banks to U.S. development.

In time his book spans the period from the first merchantile loans of the Bank of Pennsylvania in the 1780's to the local-development loans of the Central Penn National in the 1960's. It is, however, to an understanding of the period between the end of the Civil War and the depression of the 1930's that the volume contributes most heavily. Trescott suggests that the slow development and lack of industrial orientation of southern banks can, in large measure, be attributed to that region's failure to adopt free banking laws before the Civil War and from discriminatory allocation of National Bank note quotas during the war. He shows that the commercial banks were extremely important in financing post-bellum industrial development in the West, and that they made much greater contributions to meeting the long-term demands of agriculture than is generally realized. Finally he proves that the recent resurgence of entrepreneurial banking (including the innovation of consumer, home mortgage, and development loans) dates not from the post-World War II era but at least from the 1920's. In each case his evidence is difficult to deny, and economic historians will have to modify their work to take account of his findings.

Within the framework Trescott has set for himself, few criticisms can be leveled against his work. Coverage of the ante-bellum period is thin and encompasses little ground not already well covered by Bray Hammond *et al.* This weakness most likely reflects the scarcity of good business histories on the era, but it is a real weakness none the less. Again, in the next to the last chapter, discussion of the efficacy of monetary and fiscal policy since World War II, although technically correct and informative to the nonspecialists, appears a little out of place. These are, however, minor criticisms.

Despite the contribution it is, perhaps, too bad that the author did not alter the focus of his volume slightly. As written, it lacks the strong statistical base that would permit the student to evaluate with greater certainty the size and directions of the contributions made by the banking system. The data on which such a study could be made are not readily available but in all probability they do exist. Trescott has worked under the sponsorship of the American Bankers Association, and some information almost certainly reposes in the vaults of those member banks whose histories go back into the nineteenth century. Few banks ever destroy their records, and the author himself has shown that 22 banks organized before 1804 are still operating and that almost half of the first 50 national banks still survive. Until recently it would have taken a truly Herculean effort to have extracted and analyzed the significant bits of information from even one of these banks, but with the recent revolution in data-processing techniques, the problem has become quite manageable.

Despite these minor complaints, Trescott has made a major contribution to our understanding of U.S. development. He has begun research on a question that has long been ignored, and his conclusions, though perhaps not final, will have to be considered in any future study of capital mobilization. In addition, the author has shown that business histories and commercial biographies can be used in studies broader in scope than a single firm. His is certainly an important piece of work.

Lance E. Davis

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Financial Institutions: Their Role in the American Economy. By Gilbert W. Cooke, Charles L. Prather, Frederick E. Case, and Douglas W. Bellemore. New York: Simmons-Boardman, 1962. Pp. x, 501. $10.50; text, $7.75.

This textbook is designed for comprehensive courses in finance that cut across a variety of related disciplines, including money and banking, monetary and debt management policies, savings institutions, federal regulation of financial institutions and markets, federal lending programs, business finance, securities markets, and real estate and consumer credit. Its 28 chapters are spread quite evenly over all of these areas. Public finance and international finance are not covered.

The book derives its strength from the fact that it presents, within the span of just over 500 pages, a substantially larger amount of factual information on financial markets and institutions than can be found in any other single source. It contains over 100 tables and charts. References to standard works dealing with the subject under discussion in greater breadth and depth are spread liberally through the footnotes. Each chapter discusses in ample detail the financial instruments, markets, and institutions with which it deals. Chapter 11 on accounts receivable and inventory financing, for example, begins by contrasting factoring with the non-notification plan of receivables financing, provides a short historical sketch of both, reviews legal aspects of receivables financing, discusses costs of funds, and indicates sources of funds to commercial finance companies before turning to inventory financing.

The authors' efforts to fulfill the promise contained in the book's subtitle are, however, singularly disappointing. Given to painstaking description, the book lacks an over-all organizational theme and fails to display a compelling concern for the relation of financial markets and institutions to the structure and performance of the economy. The amount of analysis and interpretation is limited, often cryptic, and at times badly confused. Facts are recited portraying the historical trend toward the institutionalization of savings (pp. 464-71), the expansion of institutional holdings of common stock (p. 425), and the rapid growth of corporate pension funds (pp. 479-81), without an observable demonstration of interest in the economic implications of these developments. It is asserted without supporting argument that legislation in the 1930's created a dangerously inflationary money and credit system (p. 58). A rate of personal savings amounting to 7 per cent of disposable personal income is said to be unusually high (p. 466). The reader is told that little future growth can be expected in the size of the Federal Investment Accounts (p. 465), implying a basic change in congressional philosophy with respect to the financing of social security. He is encouraged to take the view that $3 billion constitutes a "large" federal deficit (p. 124). It is implied that investment, as the term is used in the GNP accounts, refers to the use of cash by business firms (p. 12). An aggregate excess of savings over investment, it is argued, may lead to an accumulation of funds in the form of cash (p. 35). Demand balances are excluded as a form of individual savings held at financial institutions (pp. 32-33), and subsequently (p. 276) what appears to be an estimate of individual ownership of savings and loan shares, deposits in credit unions, and time and savings accounts at commercial and mutual savings banks is identified as "total net savings."

The book will prove serviceable for courses that take a purely descriptive approach to the study of financial markets and institutions. Anyone looking for a text with analytic depth and substance would be well advised to continue searching.

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International Economics

International Trade and Development. By Gerald M. Meier. New York: Harper and Row, 1963. Pp. xiii, 208. $4.25.

The fundamental postulates and conclusions of classical and neoclassical trade theory have come under critical scrutiny in the voluminous literature on economic development. Against these basic attacks Professor Meier defends traditional theory. Admittedly, its assumptions and formal analysis do not deal adequately with the problems of development; yet it "has shown a remarkable capacity to absorb modifications," and the major task should be to provide these modifications rather than to destroy the sound and resilient structure.

Meier's study divides rather easily into two sections. The first, consisting of five chapters, presents the traditional theories of comparative costs, terms of trade, balance of payments, and capital movements, together with recent modifications related particularly to economic growth, such as the Harrod-Domar model and the Hicks-Johnson discussion of neutral, export-based, and import-based expansion of production. Much of the exposition—e.g., the first 10 sections of the chapter on comparative costs—is of very high quality indeed; nevertheless, the author's normally lucid style occasionally breaks down. (See the second complete sentence on p. 35.) More fundamentally, Meier at times involves himself in major conceptual difficulties, particularly with respect to inflation, a problem which will be considered below.

More deserving of unqualified praise are the two final chapters dealing extensively with commercial policy and the place of trade in the development process. While Meier acknowledges the logical validity of such arguments for protection as terms-of-trade and external economies, he believes that in practice the benefits to be gained from such protection are easily exaggerated, and that in general the dangers of protection outweigh potential gains. External economies in themselves do not establish a case for tariffs; what is important is a calculation of the *net* external economies—the economies less the diseconomies. Similarly, much of the attack against the neoclassical position on trade as a stimulus to development is countered by Meier. It is true that expanding trade has not always been accompanied by growth; yet it is difficult to attribute the lack of growth to the trade itself. Furthermore, "international trade did have a propulsive role in the development of a number of countries [Switzerland, Australia, Japan, etc.] that are now among the richest in the world" (p. 159).

Given Meier's classical leanings, it is rather surprising that he handles monetary and inflationary problems so poorly. The first detailed reference to "inflation" occurs in Chapter 4 (pp. 76-78), where the term is taken to mean an excess of domestic demand over domestic supply, i.e., an external current deficit. "Inflation" and external imbalance are then allowed to chase one another around in a circle, with results which would be surprising if they were not tautologically true. For example (p. 76): "As long as investment expenditure is not increased by inflationary means, it will not cause any balance of payments deficit . . . even though the investment itself may entail some increase in imports. For, regardless of its character, the output created by the investment allows aggregate supply to keep pace with aggregate demand, thereby removing pressure on the balance of payments." The use of the term "inflation" is highly questionable where it has no necessary relationship with price levels, nor with the money supply, nor with bank reserves, nor even with financial assets.

Nor is this abuse of the inflation issue a monetary lapse. Inflation again enters the picture (p. 100), apparently this time meaning a rise in the price level. Yet even here his analysis raises questions: "Capital borrowings stimulate employment and output and thereby induce imports through the increase in real income. If the borrowing leads to monetary expansion, the ensuing inflation will also stimulate imports." But why should the spending arising directly from the borrowings themselves lead to a rise in employment (and not to a rise in prices), while the spending arising from monetary expansion leads (only?) to a rise in prices (and not to a rise in employment)? A similar puzzling distinction is made (p. 102) between the effects of the volume of investment and those of the character of investment.

In spite of these criticisms, parts of Meier's book—the chapters on commercial policy and development through trade, the first 10 sections of Chapter 2, and the first eight sections of Chapter 5—provide an excellent introduction to problems of economic growth for undergraduates at the junior-senior level, as well as for graduate students. It is a pity that the study is not uniformly of the high quality established in these sections.

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Foreign Trade and the National Economy. Studies in Comparative Economics No. 2. By Charles P. Kindleberger. New Haven and London: Yale University Press, 1962. Pp. xv, 265. $6.00; paper, $1.45.

Professor Kindleberger's monograph is the second volume in a series of "Studies in Comparative Economics" planned a few years ago by The Inter-University Committee on Comparative Economics under the chairmanship of Professor Lloyd Reynolds. This entire series had originally been mapped out with the hope that a systematic rethinking of particular branches of economics in the worldwide perspective afforded by the postwar years would improve the content of economics and make it more universally relevant. A primary stimulus for the inquiry would be to determine the extent to which principles of Western economics might, or might not, have general applicability throughout the world with all its varieties of social and economic institutions and different historical stages of development.

The volume under review is a relatively brief study of foreign trade in which the author restricts the discussion to two big questions, namely (1) what determines the nature and the amounts of the goods a country buys and sells in international trade, and (2) what is the impact of foreign trade on national economic life?

Focusing the analysis on these two questions, many topics customarily discussed at length in modern textbooks on international economics, such as the balance of payments and international monetary organization, find little or no place in this book. What is offered instead is a lively and persistent probing into the nature of the national interest in foreign trade and a search for valid generalizations about the role of foreign trade in different national economies. The book is at its best in its attempts to use the varied strands of modern research, in raising questions about such research, and is perhaps particularly suggestive in emphasizing that the importance of foreign trade to particular areas is strongly conditioned by the stage of development of the area.

Kindleberger's method is to analyze in separate chapters the influence of transport costs, natural resources, labor, capital, technology, monopoly and state interference, and also random factors such as wars and revolutions, strikes, and weather-induced crop variations, as these determine what kind of goods a country trades; how and why this trade changes over time; and what this trade does for the life of the country concerned. There are also valuable chapters on the variable capacities of national economies to transform and adapt their foreign trade to changing circumstances and on foreign trade under Soviet institutions. The final portion of the book introduces the reader to the effect of foreign trade on growth, stability, and social and political institutions.

Occasionally Kindleberger's receptive disposition towards new hypotheses may leave readers wondering how a coherent basis for policy-making is to be fashioned out of the variety of theoretical propositions he passes in review. His casual attitude towards the older wisdom of economic liberalism (the classic doctrine of international trade, he says [p. 26], "will occupy us for only a few minutes") may leave the uninitiated reader thinking that the law of comparative costs is to be understood only on the questionable basis of a strictly quantitative and oversimplified Heckscher-Ohlin type of theorizing. Yet, as Kindleberger himself points out, this version of comparative costs can be a great source of confusion and misunderstanding, as Myrdal, Leontief, and Stolper and Samuelson have each in their own way had occasion to demonstrate.

In fact, the upshot of Kindleberger's discussion of the Heckscher-Ohlin theorem in different sections of his book would seem to be negative. On page 67 he summarizes his chapter on labor by saying that the Heckscher-Ohlin theorem "retains value as a rough guide to economic possibilities," but later on in the book—after grappling with the so-called "Leontief paradox"—he concedes that "the basic assumption of relatively fixed proportions of factor inputs in different commodities, fundamental to the Heckscher-Ohlin theorem, is invalid," because there is in modern times an increasing range over which factors may be substituted for one another. The most that can be said, therefore, is that the Heckscher-Ohlin theorem "remains a useful guide to an understanding of *past* commercial policy" (my italics).

Though, generally speaking, Kindleberger is adept at finding factual support for theoretical propositions, he occasionally overreaches himself. So, for instance, he states without qualification that Stolper and Samuelson "proved" what Australian economists had observed intuitively, namely that imports of labor-intensive products reduced the return to labor in importing countries such as Australia and the United States. It seems to this reviewer that the highly restrictive assumptions necessary to prove the Stolper-Samuelson proposition rules out the applicability of this proof to the diverse circumstances of the Australian and U.S. cases, whatever the historical facts about wages might have been.

All in all, however, this is a highly stimulating book and it deserves a wide audience. It reflects an intimate knowledge of the scholarly literature in the field, and the author carries the burden of scholarship with grace and ease. It remains an entirely readable book, in spite of its theoretical orientation.

It will be read with profit even by those who will have their disagreements with particular conclusions.

MAX GIDEONSE

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Case Studies in European Economic Union: The Mechanics of Integration. By J. E. MEADE, H. H. LIESNER, AND S. J. WELLS. London: Oxford University Press for Royal Institute of International Affairs, 1962. Pp. viii, 424. $6.10.

The European Coal and Steel Community, Benelux, and the economic union of Belgium and Luxembourg (BLEU) are the cases dealt with by Professor Meade and his colleagues. Chosen "for the simple reason that they are the only instances of effective economic union in Europe in recent years," they are examined not "to consider whether each union is a desirable or an undesirable institution from an economic point of view" but to answer the question, "What economic arrangements must be made in an economic union in order to make it work efficiently?" Since the cases differ substantially, there are a number of answers to that question. The authors are properly cautious in pointing out that cases from "a very particular kind of highly developed, industralized, private-enterprise, European economy" may have only limited application in the rest of the world, and that even in their own setting three instances can "prove" little in general terms but may throw light on similar problems arising in other circumstances. Nevertheless this book, with its close analysis of experience, helps substantially to build up our understanding of international economic integration, one of the great contemporary themes.

The BLEU study is a reprint of Meade's 1956 pamphlet for the Princeton International Finance Section. Among its main conclusions is the interesting demonstration of how the discrepancy in size of the two partners contributed to the success of the union; Luxembourg tended to go along with Belgian arrangements, while the exceptions made for the smaller county were easily bearable by the larger. The union was also helped by the great solvent of a single currency as the main medium of circulation, with its implications for the decisive role of Belgian financial policy. Even so, the two countries have failed to agree on unifying their turnover taxes (largely for budgetary reasons), and the union continues to provide protection for Luxembourg agriculture.

The study of Benelux, by Meade and Wells, is probably the best in English. About 130 pages give an extraordinarily compact account of such major aspects of the formation of the economic union as tariffs, indirect taxation, direct controls, agriculture, financial policies, the balance of payments, the movement of capital and labor, and public purchasing and investment. The core of the whole analysis—and the part that probably has most relevance to other economic unions because it deals with a problem they must all face—concerns the difficulties for the union that stemmed from the different balance-of-payments situation of the two members (treating BLEU as one) between 1945 and 1949 and the divergent domestic economic policies of the two governments. Stage by stage the authors trace developments until the policies were brought closer together, the Dutch balance of payments had improved, and the way was cleared not only for greater liberalization of trade within the union but also for the adoption of a common commercial policy toward the rest of the world. This is a fine piece of economic narrative and analysis. I wish, though, that there was more discussion of the extent to which the difficulties created for the union and the desire to complete it influenced—or did not influence—the evolution of Dutch policy. Another missing ingredient is any comment on the effect of the larger measures of European integration, and especially the steps toward the Common Market, on the development of Benelux.

The section on the European Coal and Steel Community, by Liesner, is a book in itself. About 220 pages deal with pricing, competition, taxes, subsidies, commercial policy and—at considerable length—transportation. This study's principal additions to the existing literature are in the examination of the ways the Community dealt with the coal surplus from 1958 onward, in the analysis of the subsidies for Belgian coal mining, and in some parts of the discussions of pricing and transportation measures. There is much good material in these chapters, but some lines of inquiry come to rather abrupt ends when the author is unwilling to go beyond published documents for an explanation of measures taken by the Community.

Given the disparity of the cases, one can hardly speak of conclusions to the book as a whole, but the authors have managed to extract some dozen lessons from their work. These are stated so concisely and circumspectly that a reviewer must despair of summarizing them except in so crude a manner as to do injustice to the authors' case. Put very crudely, the studies seem to show that, in the formation of economic unions, tariffs have been relatively easy to deal with, quantitative restrictions much harder, and taxes harder still. Broadly speaking, "the success of a Western European type of economic union is dependent upon the widespread use of the market-price mechanism" but at the same time there are important instances, for example coal and transportation, in which positive intervention by some kind of public authority is essential to the formation of a common market. Transitional arrangements and "shock absorbers" are of crucial importance but do not always disappear when they are supposed to, as the case of Belgian coal shows. The free movement of capital and labor has proved less troublesome than might be expected—partly because it has not moved very freely. "Any economic union would indeed itself be an exception if it did not include an exception for agriculture." The central problem of modern economic union revolves around the difficulties of reconciling free movement of goods and services, national independence in monetary, budgetary and wage policies, and fixed exchange rates.

I wish the authors had gone further in explaining why national policies changed at certain times and why some problems were so much more recalcitrant than others, even though this would have involved some analysis of matters conventionally called political. The particular value of doing so in these cases would have been to show something more of the dynamics of partial integration. Much is said these days of the process by which the integration of one activity is supposed to make for the integration of others, either as a requisite for its own effectiveness or because it creates problems that make for additional steps in the same direction. Ernst Haas and others have made political analyses of these matters, but relatively little attention has been given to the economic processes that have actually taken place. Case studies like these, and especially the one of Benelux, provide opportunities that have yet to be exploited.

Perhaps it is a compliment to a good book that we ask more of it.

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Factors Affecting the United States Balance of Payments. Compilation of Studies Prepared for the Subcommittee on International Exchange and Payments of the Joint Economic Committee, 87th Cong., 2nd Sess. Washington: Supt. Docs., 1962. Pp. vii, 561. $1.75.

About three-quarters of this volume is devoted to new papers, some of which provide fresh material.

Among the most substantial of the new papers is an extremely detailed study of private capital movements by Philip Bell. He finds little evidence in the data over the last decade to support the view that interest rates play an important role in determining capital flows. Bell is very cautious about his findings and underlines, in the hearings on the compendium, his reservations by suggesting that the gold points might be widened if his conclusions are wrong. The conclusions are reached with such painstaking care, their implications for policy are so great, and his discussion reveals so much about private capital movements and their measurement that Bell's study demands attention. His search, involving over 300 regressions, for explanations for shifts in errors and omissions is especially noteworthy.

Jaroslav Vanek provides a masterful review of remedies for balance-of-payments difficulties; he concludes that floating rates are preferable. He estimates that one-half of U.S. unemployment is attributable to the overvaluation of the dollar. He was challenged on this in the hearings and it was suggested that depreciation would constitute a beggar-thy-neighbor policy. But Vanek seems to be on sound ground when he writes, "The net effect of U.S. overvaluation then is a total decline in income and output of the free world, because the income of the United States is free to decline, while that of the surplus countries is constrained by the full capacity operations of these countries." His paper is based partly on his relatively inaccessible Geneva lectures and one wishes that the committee had reprinted these as well. George Halm also supports flexible rates in a lucid review of alternatives but, incomprehensibly, wants them to be as stable as possible. As a practical measure, Halm urges the widening of the gold points, as does Seymour Harris.

The fixed-rate position is defended by James Ingram as a part of a system of complete international financial integration. He calls for fixed-forever rates on the hypothesis that movements of long-term funds will provide equilibrating flows. This is a new emphasis in the defense of fixed rates. But

Ingram's proposal faces at least two problems: Bell's tentative findings and the difficulty of achieving fixed-forever rates and international financial integration. Houthakker supports adjustable fixed rates along conventional lines. The most interesting part of his essay is his ingenious calculation of the degree of overvaluation of the dollar which resurrects in a modified way the absolute form of the purchasing-power parity doctrine.

Lawrence Krause in an important paper analyzes the prospective impact of the Common Market on U.S. agricultural exports, reaching an essentially pessimistic conclusion. Irving Kravis discusses a variety of factors which have shifted the U.S. competitive position, especially vis-à-vis the Common Market. Robert Baldwin reviews the changes in the U.S. share of manufactured exports since 1900, disaggregating them after 1928. He finds the period 1938-52 to be an aberration and concludes with a withering indictment of our "no adjustment" policies. Charles Kindleberger criticizes Buy-American policies, arguing from British and French experience that sales in sheltered markets retard economic growth.

Halm, in a second essay, argues that key-currency countries cannot adjust their exchange rates and therefore suffer inequitable burdens when they face balance-of-payments difficulties. Houthakker disagrees, for he proposes devaluation of the dollar to a new fixed rate. He probably places unwarranted reliance on IMF strictures about competitive devaluation. Here lies a key difference between adjustment through shifts in fixed and fluctuating rates; the latter provide more freedom from competitive adjustments by other countries.

Altman's unique work on the Euro-dollar is reprinted, although, unfortunately for classroom use, his initial article is not reproduced. The first Coombs report on Federal Reserve and Treasury foreign exchange operations is also reprinted, including its inadequate discussion of the Treasury's forward lira operations which failed. There is an extended version of Lederer's important explanation of the problems of measurement in the balance of payments and his defense of the Commerce concept of the deficit. There is also a cut-down version of Machlup's lucid analysis of plans for international monetary reform. Meade's "The Future of International Payments" is republished. In the hearings he labels this work utopian and urges that countries be allowed to change their rates by no more than 2 per cent per year. If Houthakker and Vanek are right in their estimates of the degree of overvaluation, such authority would have been inadequate to meet our problem in view of the slow official recognition of its existence. Three of the more important papers by Undersecretary Roosa are reprinted; these, along with Harris' lead paper, provide a picture of U.S. policy.

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Dimension du marché et optimum de production. By JACQUES DE BANDT. Louvain and Paris: Nauwelaerts, 1962. Pp. 299. Bfr. 275.

This is the third study in a series of research analyses on the European Common Market published by the Institute of Economic, Social, and Political Research at the University of Louvain. All these studies have one thing in common: they attempt to determine what the probable economic effects of the Treaty of Rome are likely to be and what changes are to be expected in the various economic segments of the economies forming the customs union. This study is specifically devoted to the question of the relation between the size of the market and economic efficiency. The underlying idea being tested is whether the greater dimension of the Common Market will permit a larger scale of operation and thereby a larger size of enterprise.

This is, of course, not the first attempt to study the link between size and efficiency (*vide* the comprehensive bibliography of 17 pages which the author furnishes), and, in a sense, Jacques De Bandt may be said to have more or less paralleled the effort made a few years ago by Scitovsky. The author seems, however, intent to avoid the shortcomings of Scitovsky's book, especially those pointed out in Harry Johnson's review (this *Review*, December 1959). Following the example of Scitovsky and of other writers on this subject, De Bandt turns first of all to the U.S. experience in order to find certain clues. He emerges with two considerations: (a) that inductive studies can neither prove nor disprove the argument relating economies of scale to economic integration, and (b) that the lack of detailed statistics in Europe does not permit extension of the findings from the U.S. to the European scene. This is why he turns to economic theory. His admitted search is for those theoretical principles which would permit him to elaborate on the advantages of economic integration. In doing so, he deals in great length with selected topics of the theory of production and thereby covers a great deal of the material found in higher level microeconomic textbooks. He turns primarily to Walras, but is forced to go beyond the Walrasian general equilibrium system to explain such things as the effect of monopolies.

In a very interesting short addendum, which he places close to the start (pp. 5-6), the author cites such recognized authorities as Adam Smith, Marshall, Taussig, Rostas, Fabricant, Scitovsky, and others to indicate that the question of economic size and its effect on economic efficiency has been and is a highly controversial one and is far from being settled. De Bandt lays great stress on the fact that the producer cannot be expected to make optimal choices regarding the extent of his activity, primarily because he cannot estimate with much accuracy the elasticity of demand for his product. It is with this consideration in mind that the author eventually turns to the problem of the type of the product. Here again he stresses the U.S.-European differences, especially the greater simplicity of U.S. goods. This point, we may recall, was emphasized by Professor E. A. G. Robinson at the I.E.A. meeting in Lisbon a few years ago (see A. Robinson, ed.: *Economic Consequences of the Size of Nations*, 1960, p. 347) when he stated that "It was the fact that American goods were cheap, and therefore suited to a large market, which permitted them to be produced on a large scale."

De Bandt's main conclusion is that the enlargement of the market due to economic integration expresses itself basically through the suppression of the artificial limitations imposed on the natural market of each "unit of production." The very insertion of a "unit of production" into a larger, "global" market allows it to benefit from the efficiency obtained in the production of intermediary goods, i.e., from economies of scale. In his view, it is impossible to determine the extent of the economies of scale that will be generated, but he thinks that there are indications that they will be considerable in Western Europe (p. 271). He concludes with a cautious statement that the enlargement of the market resulting from the suppression of the trade barriers between the member countries is a necessary, but not a sufficient, condition for production on a greater scale.

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Zahlungsbilanzstörungen und wirtschaftliches Wachstum. By ANTON KONRAD. Berlin: Duncker & Humblot, 1962. Pp. 172. DM 19.80.

This short analysis constitutes an expanded version of a doctoral dissertation written under the sponsorship of Professor Pfister at Munich University. The purpose is to make contributions to the balance-of-payments theory, with emphasis on the various factors of disequilibrium.

According to the author, a positive influence on the balance is exerted by the following factors: (1) reductions in the rate of investments, e.g., through a business recession; (2) rising productivity of capital, e.g., through technical progress that shortens production methods and sets capital free; (3) importation of capital goods. Factors that exert a negative influence on the balance of payments are said to be (1) a rise of income that exceeds the rate of increase in other countries; (2) productive capacity that rises more slowly than abroad; (3) discrepancies between demand and supply structures as evidenced by the high elasticity of import demand; (4) certain constellations of price elasticity; and (5) inflationary increases of domestic prices.

These various factors of imbalance can produce a number of possible combinations. In the opinion of the author the U.S. surplus of exports during the postwar period was due to such a combination of factors. Income rose more slowly in the United States than in the rest of the world, especially in Western Europe. At the same time, the rate of investment and thus the increase in productive capacity of the United States did not trail those of Western Europe throughout most of this period. In addition, the author maintains, there was the more favorable structure of foreign trade and the lower rate of inflation in the United States. An accumulation of so many factors of disequilibrium could only result from extraordinary circumstances, especially from the "vigorous expansion which the American economy experienced during World War II" (p. 156).

It is, to say the least, surprising to read so many dubious conclusions from what at first glance appears to be a rather scholarly theoretical work on the balance-of-payments theory. Konrad seems to be unaware, or for some mysterious reason chooses not to mention, the dollar crisis that is threatening the free world. In the opinion of this reviewer, a theory that fails to explain economic reality either goes astray in its reasoning or is built on dubious premises. This book, like so many doctoral dissertations, evidences an occasional insensitivity to economic reality.

A book that proposes to deal with balance-of-payments disturbances must be expected to explain the embarrassing loss of gold which the United States has suffered since 1952. And it must endeavor to explain the rapid growth of U.S. short-term liabilities to foreigners, which at the end of 1962 amounted to nearly $25 billion. It must, above all, cope with the precarious situation in which the free world finds itself, due to the U.S. deficits in international payments. It cannot blithely overlook the repeated warnings of the eminent French economist, Jacques Rueff, who envisions a world-wide monetary crisis because of the overextension of the world's reserve currency, the U.S. dollar. And it cannot ignore the numerous plans for a world currency reform, e.g., Robert Triffin's and E. M. Bernstein's proposals.

If the thesis would not claim universal validity for its theoretical principles but modestly offer an analysis of the historical circumstances of the payment imbalances between 1945 and 1952, our critique would, of course, be different. But even in this case we would have to question the author's observations that "income rose more slowly in the U.S. than in the rest of the world, especially in Western Europe" (p. 155). Actually, U.S. income was rising rapidly while Europe was still stagnating under the ravages of the war, under shortages and rationing, trade restrictions and others. And regarding Asia, Africa, and Latin America, i.e., "the rest of the world," few economists, if any, would maintain that U.S. income rose more slowly than income in other parts of the world.

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Business Finance; Investment and Security Markets; Insurance

Financial Management: An Analytical Approach. By ROBERT LINDSAY AND ARNOLD W. SAMETZ. Homewood, Ill.: Richard D. Irwin, Inc., 1963. Pp. xv, 506. $7.95.

Financial Management is intended as a text in a first course in corporation finance. The book is admirably self-contained and should prove usable, perhaps with some supplementary material, even to students who have not had introductory economics or accounting. Part I provides the accounting requisites and other background for the reader; Part II deals with the return to funds invested in plant, equipment, and inventories; Part III discusses the cost of these funds; Part IV treats short-term sources and uses of funds; and Parts V and VI deal with capital structure of the firm and varieties of long-term financing.

Lest the subtitle of the book—"An Analytical Approach"—be confusing, the book makes surprisingly little use of symbols or algebra; instead there is a liberal dose of numerical examples, graphs and charts, and generally well-written arguments. Thus the student is told that the square-root rule for inventories is derived by "a sort of abracadabra," and he must refer to another source for its derivation. While the authors confine themselves to simple techniques of analysis and exposition, this does not usually detract from the depth of their discussion. For example, expected return-variance of return analysis, internal rate of return versus present value, the Modigliani-Miller approach to cost of capital, and similar topics are all covered at a very respectable level with only the aid of simple numerical examples. At other points, however, this reviewer would have preferred the use of more advanced techniques. For example, the authors never introduce correlation explicitly, nor do they employ scatter diagrams, both of which might have aided in presentation.

The authors' "Analytic Approach" is evident in the types of descriptive material they have chosen to delete. In many texts on corporation finance, material on mergers, reorganization, and bankruptcy might receive 10 to 15 per cent of the text, while in Lindsay and Sametz, none. Financial ratios, bond ratings, the Dupont system, and similar materials are given cursory, if any, attention. The authors also cover the essential differences between the various debt and equity instruments in relatively short compass, cutting through the welter of detail on nominal and legalistic distinctions that clogs many other texts. Lindsay and Sametz leave the student with a clear grasp of the basic similarities and differences between instruments rather than their number and nominal diversity.

Before turning to their treatment of empirical material, two other points may be made. *Financial Management* reflects the increasing interest in and literature on capital budgeting. Over two-thirds of the text is devoted to this problem and to long-term finance, while short-term finance is given relatively short shrift. Perhaps this reflects the still unsettled state of capital budget theory, as compared to the well-worn financial ratios and rules of thumb used in the analysis of current assets and liabilities.

Cost-of-capital theory in particular is in an unsettled state, and although the authors have done an excellent sorting-out job, part of their treatment of the subject might confuse the student. The basic cost-of-finance diagram (Figures 8-2, 8-3, and 8-4) shows an S-shaped relation between the cost of and supply of investment funds. In the text this relation is derived as though it reflected a sequence of financing decisions over time, and yet it is to be related to a marginal efficiency of capital schedule pertaining to a point in time. If I understand the authors, their cost-of-finance curve is a composite derived from three separate cost curves, namely the cost and availability of internal funds for various levels of debt and equity, cost of debt for various levels of equity, and cost of equity for various levels of debt. This concept is not a simple one, but some amplification would help clarify what, to this reviewer, was an exception to the authors' general ability to merge successfully dynamics and statics.

I would also quibble with the authors' assertion that businesses are not concerned with dispersion of return from investments above their expected value because businesses can always realize these higher returns in the future by expanding their plants at that time. This assumption allows Lindsay and Sametz to view all businesses as risk averters, which seems questionable. If the assumption is made for pedagogical purposes, this should be stated; otherwise it deserves documentation.

Lindsay and Sametz are particularly strong in their presentation of empirical material. It is difficult to find a table in the book that is not well integrated with the text. As mentioned, I think their presentation would have benefited if they had employed scatter diagrams and regression and correlation analysis, but their discussion is nevertheless at a consistently high level. Chapter 13 on business savings does an excellent job of spelling out the implications of business behavior for the rest of the economy. Chapter 17 presents a good analysis of the way in which businesses shift between securities and bank loans in their finance of short-term fluctuations in trade credit and inventories. Chapter 16 on trade credit is also good, though the authors do not discuss the relationship between interest rates and trade credit (a relation has been found by Meltzer and others for the postwar period). Because their treatment of data is current and of high quality, several chapters might also make useful outside reading in related courses. In addition to the above, Chapter 25 on investment banking, Chapter 26 on the purchasers of corporate securities, and Part VI on capital structure should be mentioned.

The contrast between *Financial Management* and texts of the past few years is not terribly striking except, perhaps, in the emphasis of the former on capital budgeting. However, in comparing this book with corporation finance texts of ten or so years ago, one is impressed with major changes in emphasis. Graphs have replaced photographs; but, more importantly, in place of large bodies of descriptive material there is substantial analysis. Certainly many of these changes are laudable and probably inevitable, since they reflect the stimulating literature in the field in the past decade. Lindsay and Sametz have given us a good example of a new emphasis in corporation finance texts. Their book will rightly receive the attention of teachers of finance and economics, and it deserves the attention of others interested in an introduction to the subject.

ALAN HESTON

University of Pennsylvania

Competition, Regulation and the Public Interest in Nonlife Insurance. By ROY J. HENSLEY. Institute of Business and Economic Research, University of California Publication. Berkeley: University of California Press, 1962. Pp. xii, 259. $6.00.

The purpose of this study is stated at its outset to be (1) to examine some of the important aspects of market structure, market conduct, and economic performance of nonlife insurance, and (2) to suggest changes in market conditions in nonlife insurance that seem to offer opportunities for improving economic performance in the industry.

In pursuing this purpose, the author reviews in successive chapters the types and relative importance of insurance organizations, their channels of distribution, conditions of entry and exit (including some considerations of economies of scale), and a relatively extended consideration of price policy and price-making in the nonlife industry. Whereupon, the nonlife industry is evaluated in terms of certain criteria: (1) progressiveness, in terms of product and product innovation and in terms of changes which reduce costs; (2) variety of forms of insurance protection, sufficient but not excessive to meet the needs of purchasers; (3) appropriateness of facilities and capital funds to meet legitimate demands for protection; (4) adequate but not excessive profits; and (5) necessary but not excessive selling costs. Considerably more space and attention are given to the first criterion than to all the others combined, progressiveness being tested by accomplishments in multiple-line insurance, in automobile insurance, and in accident and health insurance. In these respects, as with the remaining criteria, performance is found to have fallen short of reasonable expectations. The full potential of multiple-line insurance has not been exploited because of rating, legal, financial and personnel problems. Automobile insurance does not compensate justly for traffic injuries. Private accident and health insurance has not moved to embrace group-practice, comprehensive, direct-service medical care plans nor to accept the role of being merely a partner of public prepayment programs. Although the industry has a responsibility to do more to expand product variety to fit policyholders' needs, yet in certain fields, notably accident and health, less variety would seem desirable. The industry, while not guilty of a bad performance, could respond more quickly to increased demand by more adequate capacity to assume risks. Profits have been above what is necessary to sustain the industry; and selling costs, on the average, seem for capital stock agency insurance companies to have been higher than necessary.

This summary and necessarily incomplete reflection of the major judgments reached by the author does injustice to the wealth of sources of information brought to bear on each point. Nevertheless, it also serves to illustrate the problem incurred by the author in attempting to treat as an entity all nonlife insurance, comprehending 27 listed categories of such diverse forms as fire insurance, workmen's compensation, health insurance, and automobile insurance—not to mention marine, fidelity and surety, burglary and theft.

Consequently, the reviewer frequently finds himself questioning the validity of certain generalizations, e.g., that "price competition is not typical in the nonlife insurance industry" (p. 100), that "capital stock companies are likely to be non-competitive in price and contract forms" (p. 199), and that "the amount of price competition provided by independent or deviating companies, mutuals and reciprocal exchanges has generally been insufficient to put much pressure on uniform price patterns" (p. 202). He thinks of the vast differences between the price determination, regulatory, and competitive patterns in fire insurance, in automobile insurance, and in health insurance; in the last of which price competition is more rampant (and more free of industry or regulatory structuring) than perhaps any other form of insurance. Insistently, the reader is tempted to challenge conclusions by reference to a field of nonlife insurance which is an exception, and often a major one, to that which was evidently in the author's mind. In application to the whole nonlife field, an unwarranted emphasis is given to the impact of bureau and government regulation, with a presumption of price uniformity free from the dangers of widespread price competition (p. 75). However valid this presumption might be for certain forms of nonlife insurance, it is certainly not true for other forms which outrank them in premium volume. The force of certain conclusions is consequently weakened by not limiting their application to the relevant categories.

The effort to treat all nonlife insurance as an entity also experiences difficulties, recognized by the author, when a major coverage—health insurance—is also (and principally) written by life insurance companies.

In certain respects, too, the author's expectations may be somewhat naive, as when, for example, in comparing certain aspects of automobile liability insurance with workmen's compensation, he fails to recognize how far the latter falls short of the swift administration of justice. His criticism of the inadequacies of private health insurance, generally well supported, nevertheless curiously fails to mention the development since 1950 of major medical coverages which has gone far to meet some of his criticisms.

On the other hand, the author may be congratulated for being either very influential or a prophet regarding certain implied recommendations, e.g., that rate bureaus should be barred from appearing as aggrieved parties in deviation application proceedings, and that deviations should continue to be valid for more than one year, without refiling—recommendations which are apparently now being accepted by the National Association of Insurance Commissioners.

The author's analysis leads to his suggestion that better industry results could be obtained if there were (1) industry-wide independent action in the expense and profit portions of insurance premiums; (2) collection and processing of loss statistics by a federal agency in which industry members are represented along with public representatives; and (3) federal regulation of the interstate aspects of the business, focusing on the establishment and enforcement of financial standards. The arguments for these suggestions are well developed but again, in the opinion of the reviewer, fail to recognize major inherent differences between different categories of nonlife insurance for which these proposals would have differing meaning. The comparison of the competitive freedom of life insurance with that of nonlife insurance (p. 221) is seriously misleading in failing to recognize the ability of the State, in lieu of rate regulation, to require adequate reserves for life insurance liabilities, a power which it is unable to exercise in nonlife insurance because there is as yet no basis for setting comparable reserve requirements. The recommendations, to many, will be accepted as long-run objectives toward which many intermediate steps will be necessary. Others will reject them even for the long run. To all students of insurance, however, the analyses and challenging conclusions will be an intellectually stirring and worthwhile experience.

Whatever one's aversion to extended footnotes, an exception must be made for this work. Its documentation, including an excellent bibliography, adds depth and provides to all scholars a new plateau from which fresh advances can be made.

William H. Wandel

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Business Organization; Managerial Economics; Marketing; Accounting

Economics and Industrial Efficiency. By H. SPEIGHT. New York: St. Martin's Press, 1962. Pp. x, 262. $5.75.

Lectures to technology students at the University of Leeds on what we would call industrial engineering or engineering economics provided the basis for this unpretentious text. In a limited space Speight manages to come to grips with a remarkable number of important issues. He states the principle of opportunity cost clearly and forthrightly at the outset, and then proceeds to apply it rigorously in considering, successively, optimum size and product-mix, and efficiency in the use of capital and labor, in selection of accounting procedures, and in pricing. In his discussion of capital budgeting, he follows closely the procedures and point of view expressed in Solomon's *The Management of Corporate Capital.* The case for use of a replacement-cost basis for depreciation accounting is presented at some length, and the Bergfield, Earley and Knoblock Profit-Volume ratio is advocated as the best way to approximate incremental cost pricing. A final chapter demolishes the case for cartels and is soberly critical of most industry concentration. Although Speight makes no attempt to duplicate the comprehensiveness of U.S. texts in the field of managerial economics, some teachers of introductory undergraduate courses in this area might find his conciseness a virtue.

JOEL B. DIRLAM

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Entrepreneurs of Lebanon—The Role of the Business Leader in a Developing Economy. By YUSIF A. SAYIGH. Cambridge: Harvard University Press, 1962. Pp. xiv, 181. $4.25.

This book is the result of a research project carried out in Lebanon in 1959 by the Economic Research Institute of the American University of Beirut. It is the first serious attempt to put empirical content into the discussion of entrepreneurship in the Arab countries, and it is a well-written, scholarly, and highly readable document. For conceiving the project, planning and carrying out the field work, and producing such a high-quality product, Professor Sayigh should indeed be included among the entrepreneurs he has attempted to survey.

The book is divided into five chapters. The main findings of the study are given in Chapter 4, entitled "Entrepreneurs of Lebanon: a Self-Portrait." In the remaining chapters, the methodology of the study is described, and certain conclusions are derived from the analysis of the statistical data. An appendix contains 472 statistical tables and the questionnaire from which they were derived.

The survey includes 207 business leaders of whom 130 are in industry, 16 in agriculture, 14 in finance, and 47 in all the other services sectors. The preponderance of industrialists is by no means due to a plethora of entrepreneurship in this field; rather it is due to the availability of an industrial census and to the arbitrary decision to exclude from the survey certain traditional activities such as trade.

The survey reveals that the entrepreneurs of Lebanon are a fairly well-travelled and well-educated group; only five have had no formal education. They are predominantly Christian (166 Christians, 34 Moslems, and seven minorities) and predominantly Lebanese by birth (149), although those of non-Lebanese origin constitute a sizeable minority. More than half the total number studied are engaged in trade and industry, and profit is the main motivation of only 26 per cent of 207 leaders studied. In their attitude toward government, 81 (a large minority) favor more planning and control; only six are strongly opposed to a planned economy. True, the majority is opposed to the State playing a larger role in economic affairs. A large majority is also opposed to active participation in politics, and not many of the total hold employment in the civil service in high esteem.

The above is but a sample of the main findings of the study, which throws light on practically all aspects of entrepreneurship in Lebanon. Since Sayigh is sufficiently encouraged by the results to advocate that similar studies be carried out in neighboring countries in the region, it may be worth while to discuss a number of shortcomings of this type of analysis.

The first question that occurs to the reader is whether businessmen are a reliable source of information on their own attitudes and motivations. There is serious doubt that they are, and the book supplies considerable evidence to confirm this doubt. Although Sayigh is aware that here he is treading on dangerous ground and that his respondents are likely to rationalize their actions, he does not make matters simpler for them. The respondents are made aware from the start that they are "a select number of the most prominent businessmen in Lebanon" (p. 139). In the questionnaire, the alternatives listed in many questions are not independent and a great degree of rationalization on the part of a respondent is required in order that he be able to rank them. For instance, such motives as "pecuniary profit, power, prestige and status, sense of achievement, satisfaction in expansion in one's business and philanthropy and social service in one's money" (p. 106) could all be reduced to pecuniary profit. Is it then surprising that only 26 per cent of the respondents gave the profit motive first priority? Similarly, when a respondent is asked to list the sources of his ideas, would it be reasonable to expect him to distinguish between "own interest or initiative or inventiveness" and "training and experience" and "contacts with other businessmen" without engaging in much rationalizing?

The book contains a commodity bias which is difficult to explain, for it is neither consistent with Sayigh's own definition of economic development as growth in real income nor with his definition of entrepreneurship as involving the introduction of "new combinations." Numerous references suggest that trade activity and a number of other services were "of dubious direct value for development" (p. 79). For instance, "where certain innovational manifestations could be discerned—as in the successful introduction and promotion of a new perfume or shirt or of a new way of rendering some type of service—they seemed too distantly and tenuously related to development" (p. 27).

This commodity bias has greatly damaged the usefulness of the study. It not only results in a sample heavily weighted by industrialists—although industry constitutes not more than 10 per cent of the national income—but, more importantly, it excludes from the study so-called traditional activities where "new combinations" are all the more significant for development.

To characterize the 207 respondents as a sample is, of course, not strictly accurate. Given Sayigh's definitions, they are, in fact, the entire population of entrepreneurs. But because certain activities were excluded by definition, it amounts to circular reasoning to conclude, for instance, that "trade, the most prevalent activity in the country is also the least innovational" (p. 53). Similarly, to conclude that "what is more arresting is the relative popularity of the corporation" (pp. 54-55) involves circular reasoning since "new organization" was one of the criteria of selecting entrepreneurs.

These methodological strictures are perhaps unavoidable in a pioneering study of this nature, and Sayigh is not to be criticized unduly on this count. Of greater importance is the question whether empirical studies in this field are likely to throw much light on the problem of economic development in underdeveloped countries. Numerous lines of investigation are suggested by the study. How significant is the fact that Moslems constitute a relatively small proportion of the total number of entrepreneurs? Is it a question of education and exposure to Western influences, or does religion have much to do with this pattern? Where entrepreneurship fails, can the State effectively step in, or would the State be similarly handicapped by lack of the entrepreneurial spirit? What roles do population movements and minority traits play in the development process? That Sayigh does not attempt to deal with such issues is due to the fact that his study has a more limited objective; more importantly, these issues cannot be discussed intelligently except on a comparative basis.

Sayigh presents two important conclusions. The first is the paramount importance of organization: "If I were asked to emphasize one point upon which I have emerged from the study more convinced than before, I would single out the importance of rational and efficient organization in establishment as a necessary adjunct to innovation" (p. 126). Having already deviated from Schumpeter's definition of the entrepreneur, especially by identifying him with a social class, doesn't this bring Sayigh nearer to a Marshallian definition? The second main conclusion is that the entrepreneur "is not the initiator of development" (p. 137). The initial spark has to come from elsewhere. Once this happens and the setting is ready, the entrepreneur becomes both the beneficiary of this initial development and a strategic factor in a sustained growth. Sayigh's entrepreneur turns out, after all, to be a parameter in his model; he cannot spark growth but he sustains it. Is this a meaningful distinction, or is it perhaps what Arthur Lovejoy referred to as "metaphysical pathos"?

ADNAN MAHHOUK

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Industrial Organization; Government and Business; Industry Studies

Civilian Nuclear Power: Economic Issues and Policy Formation. By PHILIP MULLENBACH. New York: The Twentieth Century Fund, 1963. Pp. xiv, 406. $8.50.

"In spite of previous disappointments, the United States now has an unequaled opportunity to exert vigorous world leadership in developing peacetime uses of atomic energy and should press forward with a broad 'public and private' program that will cost the government $200 million a year for the next decade." So reads the first paragraph of the Twentieth Century Fund news release on this book dated February 11, 1963.

In the same month, David Lilienthal, first chairman of the AEC, in a series of lectures at Princeton, called for the government to abandon its support of atomic power development and to reduce substantially its support of basic atomic research. Lilienthal based his argument on the following points. (1) Energy from the atom is not now needed for civilian purposes. (2) When it is needed, it will be forthcoming without government prodding. (3) There is no urgent fuel or power crisis and no prospect of one in the foreseeable future. (Lilienthal's position was reported in the *Washington Post,* dated March 21, 1963.)

Given these two widely divergent policy positions, one should expect to find in Philip Mullenbach's book a reasoned defense of the first positon and the effective counterarguments to the Lilienthal position. Interestingly enough, however, this is not the case. With the material presented in the book, the Lilienthal position can be as readily advanced as the opposing one. This is not to criticize the book on this score. My implication is only that the news release describing the book, and based wholly on its last few pages, is seriously misleading. In an attempt to make the book "newsworthy," in this era of the image, Twentieth Century Fund presents it as a plea for expanded federal spending programs.

Civilian Nuclear Power is a good book. Mullenbach organizes the facts well, and he presents the experience and the issues relevant to this frustrating emergent industry critically and dispassionately. Gone is the early naive hope for quasi-free energy, which economists shared both with the physical scientists and the general public. Mullenbach notes the various ambivalences that have plagued policy makers from the outset: military versus civilian requirements, economic versus political realities, public versus private power interests, domestic versus foreign policy considerations, short-run versus long-run purposes.

The record is difficult to assess, and acceptable norms to apply either in looking backward or in looking forward simply do not exist. There is no familiar reference system that may be invoked to help us to impose a value pattern on policies or events. An energy source emerged as a by-product of weapons technology, and there is no immediate prospect that this essential relationship will be modified. For the foreseeable future, the federal government must continue to be the monopolist producer-supplier of nuclear fuel.

Even were cost data freely available, which they are not, fundamental issues of imputation arise in any evaluation of nuclear-fuels pricing. Hence, estimates of the costs of generating electric power from nuclear reactors remain subject to an acceptance of an administered price structure. Given this structure, nuclear power is not now competitive in cost with conventional power, either in the United States or elsewhere in the world. The data seem convincing, however, that genuine cost competitiveness will be attained within the next decade in high-cost areas where demand factors allow the full economies of scale to be exploited. The joint General Electric-Pacific Gas and Electric reactor at Bodega Bay, California, scheduled for completion in 1965, deserves special attention in this respect, as does the SENN project in Southern Italy.

Mullenbach does not build a case for accelerated nuclear power development in the United States on cost saving, as such. Suggestively, he argues that nuclear power's main contribution may lie in forestalling unwarranted increases in conventional fuel prices over the next few decades, increases that will not be due to any using up of resources, but due instead to increasingly restrictive monopolistic devices, publicly imposed. Political reality gives this argument considerable weight. Rational economic policy for the domestic oil industry seems out of the question. One means of accomplishing some of the same results might be some investment in atomic power development.

Were the issues solely presented in terms of domestic power supplies, the facts suggest that Lilienthal, on balance, has the stronger position. However, the case for continued federal support for the development of nuclear power becomes stronger when foreign policy considerations are introduced. There is a demonstration value to be exploited from the *successful* development of nuclear power. Mullenbach criticizes President Eisenhower for over-optimism in his atoms-for-peace program announcement in 1953, but he acknowledges that the program, properly conceived, contains the basis for the useful employment of nuclear power policy as an adjunct to U.S. foreign policy generally. Comparable investment in alternative energy sources may well be equally productive, measured in simple efficiency units. However, the demonstration effects to be gained by the exploitation of atomic technology, so long awaited and the hopes for which have been so long frustrated, warrant some special treatment. Considering the least-productive investment in the foreign-aid program as the relevant alternative, investment in the installation of nuclear power reactors in suitably situated areas of the world becomes reasonable national policy.

These remarks touch on only a few of the issues that emerge from this book. Here is, in one place, an accurate and complete discussion of an important policy area about which most of us are singularly ill-informed.

JAMES M. BUCHANAN

University of Virginia

Militarism and Industry—Arms Profiteering in the Missile Age. By VICTOR PERLO. New York: International Publishers, 1963. Pp. 208. $3.75; paper, $1.65.

A book published by International Publishers, with a preface by Professor J. D. Bernal, bears a certain *imprimatur,* and those who still carry intellectual scar tissue from the 1930's will approach it warily. Marxist publications have an extraordinary way of failing to surprise the experienced reader. I must confess, however, to experiencing a little surprise, and that not unpleasantly, in reading this book. The preface has the usual air of secular pontification. The text, as one might expect, is sprinkled heavily with percentages, which seem to be the besetting sin of the Marxist rhetoric. There is a certain amount of speculation, where information is not available, in directions which are not wholly unrelated to the value presuppositions of the author.

Nevertheless Mr. Perlo must be given his due. With the somewhat unsophisticated statistical apparatus at his disposal, he has nevertheless collected some interesting information and raised some important questions. Furthermore the book is written in a moderate and conciliatory spirit which, compared with similar publicatons of a generation ago, reflects a remarkable movement towards objectivity. Much of the material that Perlo has collected regarding the war industry and the American economy is fairly familiar. Nevertheless he does raise questions (such as the impact of the war industry on the distribution of income both as between labor income and nonlabor income and also as between different regions of the country in different industrial groups) which, as far as I know, have not been tackled before. The answers which he gives to these questions may not be above the very first level of approximation and they may not be free from bias, but the problem which he raises is an interesting one that needs further study.

It is not surprising to find that one of the main purposes of this book is to demonstrate a connection between the impact of the war industry on the distribution of income and the attitude of various groups, occupational, regional, industrial or class, towards the war industry itself. Perlo is not wholly averse to the method of nonrandom quotation, nevertheless he disclaims any rigid economic determinism and admits one or two puzzling exceptions to the rule that economic interests determine political opinion. The rule is still a pretty good one, however, and it is not surprising that Perlo finds many cases which confirm it.

The most important conclusion to be drawn from Perlo's analysis, at least in the mind of this reviewer, is that the distributional impact of the war industry as between classes is extraordinarily uniform, whereas the distribution as between regions and industries is not. Perlo himself calculates (p. 119) that real labor income is lowered by about 10 per cent in the United States as a result of the war industry. As the war industry itself is almost exactly 10 per cent of GNP, and a somewhat larger percentage of the National Income, one would expect the diminution of the real value of nonlabor income also to be about 10 per cent or a little more. From the point of view of the traditional Marxist class struggle, therefore, the war industry is almost neutral, but from the point of view of the regional and industrial pressure groups it is not. This fact alone has great political consequences. It is no doubt only a first approximation to say that the enemies of disarmament are likely to be those who will suffer from it, but it is at least a first approximation that is worth making.

A great many of us in middle life today learned in our youth that dialogue with the Marxists was impossible. Times have changed, however, and what I think I have learned from Mr. Perlo's book is that this proposition may have been overlearned and should be re-examined.

K. E. BOULDING

University of Michigan

Domestic Airline Efficiency—An Application of Linear Programming. By RONALD E. MILLER. Cambridge: Massachusetts Institute of Technology Press, 1963. Pp. xiv, 174. $6.00.

In this book, the fifth in a series of regional science studies edited by Walter Isard, the technique of linear programming is applied to the problem of scheduling aircraft flights so as to achieve the greatest possible "efficiency." Here "efficiency" is defined as the minimization of total direct operating costs, subject to three sets of constraints (plus the requirement that the solution values of the variables be nonnegative). The three sets of constraints are as follows: (1) for each type of aircraft, it is required that the utilization implied in the optimum solution not exceed the physically available number of aircraft-hours, given the stock of aircraft and normal adjustments for maintenance and spares; (2) for each "node," or point of arrival and departure, it is required that the number of arriving planes of any type not fall short of the departing number of that type; and (3) for each ordered pair of nodes, it is required that the total number of passengers accommodated be at least equal to "demand."

The analysis is carried out for two time periods, one in 1957 and one in 1963. For the former, "demand" is defined as the actual traffic indicated by Civil Aeronautics Board sample studies; for the latter, "demand" is defined as the traffic level estimated by allocating a CAB national forecast among routes through projecting 1954-58 trends. The "demand" for each route is in passengers per week; the aircraft availabilities are in aircraft-hours per week; and the solution values are in flights per week of each aircraft type between each ordered pair of nodes. Direct operating costs per flight are estimated for each route, and are multiplied by the number of flights to arrive at weekly cost figures under optimal scheduling.

For each year, there are four sets of results, representing combinations of two assumed load-factors (95 per cent and 60 per cent) and two sets of nodes. One set of nodes (the "intercity" model) comprises nine major traffic centers: New York, Chicago, Miami, Los Angeles, Washington, San Francisco, Detroit, Boston, and Cleveland. The other ("interregional") model lumps together the main centers in three geographic sectors to arrive at a seven-node system: (1) New York, Washington, Boston, Philadelphia; (2) Chicago, Detroit, St. Louis, Cleveland; (3) Miami; (4) Los Angeles, San Francisco; (5) Seattle; (6) Dallas; (7) Atlanta.

As Miller points out, there are several reasons why his formulation of the problem of "efficiency" does not coincide with the scheduling problem confronting airline managements or, indeed, with the corresponding problem which would face a national super-manager of air transportation. For any airline, supplies of aircraft are limited to its own stocks and those available through lease or interchange; in the Miller formulation, planes are assumed freely transferable among airlines. But this limitation, presumably not relevant to a super-management, is of relatively minor significance and may be reasonably ignored in a model designed to highlight differences between "ideal" and actual performance. Of greater importance is the fact that the model takes no account of the necessity for and desirability of scheduling flights in response to maintenance needs and inflexible time-patterns of demand. In Miller's words (pp. 92-93), the model's omissions:

> . . . may be of such overwhelming importance that, even if efficiency were the desire of both the CAB and all concerned airlines, it would still be inadequately handled by a model of the sort presented here. Internal airline equipment scheduling and routing problems come to mind here, as does the whole question of the uneven distribution of demand.

It is also recognized (pp. 70-71) that inclusion of additional nodes might well substantially alter the apparent discrepancy between actual and "ideal" results.

This apparent discrepancy is of impressive proportions. For example, Miller's 60 per cent load-factor intercity model for 1957 indicates an optimal total direct operating cost of \$4.2 million, as compared with his estimated actual cost figure of \$9.1 million (p. 97). Among other results of the study, one may note that the "overscheduling" responsible for the excess of actual over optimal costs on a given route is found to be positively associated with the total level of demand and with the number of directly competitive airlines.

Although the model's limitations are carefully indicated, there remains perhaps some tendency to overestimate its normative usefulness. For example, in the discussion of the "shadow prices" for passenger transportation derived from the optimal solution, it is implied that these prices, which represent increments to direct operating cost resulting from unitary additions to "demands," might serve as "ideals" for the guidance of price regulators. Space limitations forbid extensive treatment of this view. Let it be noted, however, that even with Miller's "continuous" formulation of the problem, several zero prices would be recommended as "ideal"; with a more realistic, "integer-valued" formulation, many more such prices might very well result.

All who are concerned with air transport economics should find this study valuable because of its lucid analysis and exemplification of some of the problems of using linear programming in this field.

The second full paragraph quoted on p. 143 should be attributed to Gill and Bates and *not* to Keyes.

LUCILE SHEPPARD KEYES

Washington, D.C.

Price Formation in Natural Gas Fields—A Study of Competition, Monopsony, and Regulation. By PAUL W. MACAVOY. Yale Studies in Economics, No. 14. New Haven and London: Yale University Press, 1962. Pp. xix, 281. $6.75.

In the long and spirited debate over federal regulation of the field price of natural gas, attention has been focused on the market position of the sellers. In an interesting reversal, Professor MacAvoy asks whether monopsonistic power by pipelines may not have been a serious cause of price distortion in these markets.

To help answer this question, he subjects to statistical analysis the gas purchase contracts reported to the Federal Power Commission for a group of geographical markets and time periods. From each contract he takes or estimates the following: price (P); volume of gas (V); distance of the supply from the pipeline (D); the term-length of the contract (T); the year it was signed (Y); the presence or absence of the favored-nation clause (C_1); and the presence or absence of a renegotiation clause (C_2). Using the least-squares method, he fits the data from the contracts negotiated in a two- or three-year period in one market to an equation of the following form:

$$P = \alpha + \beta_1 V + \beta_2 D + \beta_3 T + \beta_4 Y + \beta_5 C_1 + \beta_6 C_2 + \gamma$$

computing the equation for each of thirty markets.

MacAvoy argues that competitive markets can be distinguished from monopsonistic markets by the signs and values of the β coefficients. Under competitive conditions, he suggests, β_1 and β_3 are likely to be positive, β_2 negative, and β_5 and β_6 either negative or zero. Under monopsonistic conditions he believes the likely values of β_1, and β_2 are zero, β_3 positive or zero, and β_5 and β_6 negative or zero. (β_4 is not relevant to market structure.)

He suggests, for example, that under competitive conditions the prices for supplies distant from the pipeline will be less than for those nearby, to offset the costs to the buyer of moving the gas to the transmission line, and that β_2 will therefore be negative. Under monopsony, however, MacAvoy believes the price differential for distance is not likely to be significant "since such would involve payments of unnecessary rental returns to producers" (p. 82).

But this implies very strong monopsony power. More likely, as MacAvoy recognizes elsewhere (e.g., p. 90), the presence of monopsony would mean that price differentials (with respect to distance, for example) would exist but would be less marked and less regular than under competitive conditions. Recognition of the fact that the distinction between competitive and monopsonistic values of the β coefficients is a matter of degree suggests that his test of the sign of the coefficients is inadequate to differentiate between competitive and monopsonistic conditions. The values of the coefficients in the equations bear this out. By the reviewer's count, of 150 values relevant to the argument, 36 imply competitive conditions, 28 monopsony, and 61 fit either. (All of these are also consistent with MacAvoy's test for monopoly, on which he understandably places little weight.) The remaining 25 are not consistent with competition, monopsony, or monopoly.

In his discussion of the extent of competition, monopoly, and monopsony in each of the 30 markets, MacAvoy supplements his statistical results with a description of the recent history of the market and the structure of buyers and sellers. This supplementary material is rather brief and, combined with the statistical evidence, not really adequate to support satisfactory conclusions on the degree of competition in the markets.

As part of his analysis of demand, MacAvoy provides informative statistical estimates of the city-wide home demand for natural gas, using cross-section data, and of the demand for gas in six industries. These studies are useful and would be worth extending.

With two exceptions, monopoly plays no role in his statistical analysis of field markets because the author does not believe monopoly conditions can be distinguished from competitive conditions by the signs or values of the coefficients in his equations. But he gives for each market the percentage of the total volume of contract gas sold in the period by each of the largest suppliers. From this and some manipulation of estimates of elasticity of demand, MacAvoy concludes that there is no significant monopoly influence, thus deciding a question over which many experts have spent many hours without agreement.

As a critique of federal regulation of field prices this book makes little contribution. But the analysis of demand in field markets is interesting and informative. The author's statistical approach is imaginative, and the results are challenging. This is a useful demonstration of the potentialities and the limitations of regression analysis, and of the difficulties of translating broad economic concepts like competition and monopsony into variables that can be measured.

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Competition and Price Making in Food Retailing—The Anatomy of Supermarket Operations. By RALPH CASSADY, JR. New York: The Ronald Press Co., 1962. Pp. ix, 333. $8.00.

This is a useful addition to the still regrettably limited literature on distribution economics. After considering the economics of food demand and supply at the retail level, the structure of food markets and competition within them, the author turns to a detailed examination of supermarket operations. There follow three chapters on legal restrictions on price-making, including antitrust (and Robinson-Patman), state price-fixing, and unfair competition.

The reader must not, however, expect a systematic analysis of the trade. Essentially the book is a set of notes by somebody blessed both with long experience and academic background, so that on whatever aspect of the subject he lights, he has something valuable to say. In that sense, the book might be considered a set of footnotes to *Distribution in Great Britain and North America,* by Hall, Knapp, and Winsten (1961), both when it agrees and when it does not. Thus Ralph Cassady accepts Harrod's casual empiricism —that more affluent individuals are less sensitive to price differences (p. 32)—despite the fact that in the affluent U.S. suburbs price competition is keenest and prices lowest. On the other hand, his diagramming of overlapping trade-area circles (pp. 60 ff.), although inconclusive, is at the very least a vivid picture of the market and offers a start toward the puzzling problems of defining economies of scale and market share. But then there is a rather pointless conventional discussion of price versus non-price competition, when the real point is that the various features and services offered all have a cost and a market price, so that the two concepts are complementary rather than competing.

The discussion of supermarket operations is well worth studying because of its experiences and insights. For example, "the author knows of one southern California independent who purposely chose a location in the neighborhood of four or five successful supermarkets," and he gives the reasons why this odd choice was a good one. But we have no general principle illustrated or deduced, and perhaps there is none. Both the virtues and the limitations of this first-hand contact are repeatedly borne out in the chapters on price-making in supermarkets. One has both information and theorizing, e.g., on the advantages of private-label manufacturing, but no attempt is made to mobilize either principles or statistics to see why some firms at some times and places find it advantageous and some do not. The discussion on the effect of size on operating costs (pp. 263 ff.) starts by stating "that the advantages of size are exaggerated"; yet, on the other hand, they may exist, except where they are not as important as others. "Supermarket expense percentages in the early days of this type of operation . . . were reportedly around 10 per cent of sales, but now they are closer to 17.5 per cent with greatly increased sales [having] increased substantially from 1954 to 1959 on the basis of some confidential figures made available to the author" (p. 264). A few pages later, he mentions briefly the Federal Trade Commission's food-marketing inquiry; but profits as a per cent of sales are a doubtful indicator of anything, and there is no attempt to analyze the figures further (p. 267). Therefore it is not clear whether the apparent fact that average profits are smallest in the largest size group is a statistical illusion or reflects different bundles of services, or greater efficiency, or something else. And the discussion of mergers notes that they may at most have involved stores selling as much as $2.8 billion over the 1955-58 period. But since the increase in food expenditures must have been greater than that (cf. pp. 16-18), it is difficult to see a "consolidation threat" here. Nor does one need the pseudo-profundities advanced by special pleaders whom the author cites without agreement—chance alone, the good and bad luck of a widely varied trade, and the desire to sell out for lack of heirs or successors would suffice to explain a rather microscopic phenomenon. It is both the virtue and weakness of Cassady's book that he notes so much that is important and interesting, while leaving it to others to systematize it further. It is as much as one has the right to ask but less than one might wish.

M. A. ADELMAN

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The Economics of Loyalty-Incentive Rates in the Railroad Industry of the United States. By ROBERT F. LUNDY. Pullman: Washington State University Press for the Bureau of Economic and Business Research, College of Economics and Business, 1963. Pp. 144. $6.00.

This little monograph will be useful to those economists with an interest in the institutional complexities of railroad and general transport regulation. It is not an exercise in applied economic theory, though some evaluation is provided of the possible economic effects of loyalty-incentive rates.

Loyalty-incentive rates may take various specific forms, but all of them represent rate systems designed to attract continuous shipper patronage over relatively long periods. Such rates are in common use in Canada and Europe. In Europe these rates have taken the form of secret contracts between shippers and carriers for the carriage of a minimum volume of specified traffic, between specified points, at a rate lower than the published rate. The loyalty rates used in Canada are similar except that publication is required.

In the United States, approval of loyalty-incentive rates has been sought by the railroads as a means of reversing the continued diversion of traffic from the rails to other media, principally trucks and waterway transportation. Such a reversal could of course be encouraged by seeking approval for the reduction of ordinary commodity rates. This has been done successfully in some areas, but where a competitively threatened commodity is now moving in substantial volume by rail, the railroads are reluctant to jeopardize revenue on the present volume without some assurance that the reduction will regain or hold enough traffic to strengthen or at least protect their profit position. As a consequence the railroads in a series of cases have attempted to establish two forms of loyalty-incentive rates. The annual volume rate is an agreement between railroad and shipper under which the rate per ton is reduced in return for the shipment of a specified minimum annual volume of certain commodities. Four rates of this type have been approved. The second form of loyalty-incentive rates has been contract rates similar to the Canadian agreed charges. So far, rail attempts to obtain such contract rates have been disallowed by the ICC.

The book provides an introductory discussion of the competitive setting of the American railroads, which has given rise in important measure to the loyalty-rate issue. Chapter 2 provides an historical and descriptive discussion of the loyalty-rate schemes in Europe and Canada. Chapters 3 and 4 consider in detail the important controlling cases through which the railroads have established or attempted to establish "annual volume rates" and "contract rates." Chapter 5 attempts to analyze the economic effects of loyalty-incentive rates. The analysis suffers from the fact that there is so little experience on which to base any comparisons. The need here might be filled by a more extensive analysis of European and Canadian experience than is provided in Chapter 2.

The last chapter concludes in favor of loyalty rates as a means of permitting fuller utilization of railroad fixed facilities and reducing the disadvantage of regulated common carriers in competing with the contract and unregulated carriers. I realize that Lundy's recommendations have been constrained by the practical consideration that the present system of regulation is likely to continue. For my part, and an increasing number of economists seem to agree, I would much prefer to see the inequities between regulated and unregulated carriers removed by eliminating the economic regulation of transportation by the ICC. As for the loyalty rates, I would favor permitting any such rates which pass the test of ordinary antitrust criteria. In this regard, Lundy expresses doubt (p. 91) that the lawfulness of such rates would be clear under the antitrust criteria, and doubt (p. 139) that their unlawfulness would be clear.

VERNON L. SMITH

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Transportation—Economics and Public Policy. By DUDLEY F. PEGRUM. Homewood, Ill.: Richard D. Irwin, Inc., 1963. Pp. xviii, 625. $8.50.

Professor Pegrum's book goes a long way toward meeting the widely felt need for a transport-economics text that is focused on the central problems of today; that utilizes economic theory more fully and historical and institutional material less; and that is more manageable in terms of textual readings for a one-semester course on this long-neglected field.

The author recognizes that transport has so many dimensions that a text must be highly selective in terms of the particular discipline for which it is written. As the economist is concerned with what Pegrum calls "the problem of the economical allocation and utilization of resources as this relates to transportation," the focus of his entire volume is on analysis of the requirements of allocative efficiency as to transport, particularly with respect to public promotional and regulatory policy. Its specific design is "to develop the economic principles of transportation." Pegrum believes that major current transport policies have been constructed on "unsound economic principles or from a lack of principles altogether." He urges a return to "meaningful discussion of the issues" and holds that this can be accomplished only if transport issues are approached through "the accepted corpus of theory" or, in the alternative, through "new theory that is relevant." In view of fiery controversies over the appropriate application of economic regulation, modernization of transport pricing in terms of competitive conditions, and user-fee and investment policies in the public sectors of transport, this reviewer cannot quarrel with Pegrum's general emphasis. While it may not be wholly sufficient, it is obviously much needed.

The text is divided into five parts. Part I treats transport as an economic activity and discusses its place in the economy. It freshly presents transport's relation to the economy; usefully describes the system's facilities and the role of each mode; greatly compresses history, focusing on technological developments and effects; helpfully maps transport routes and commodity flows; and briefly describes the types of carriers and services. After giving a useful exposition of the economies of scale and other considerations in determining the economic structure for transport industries, Part II concentrates on pricing, first on the general theory of pricing and then on the theory of pricing for transport. Inclusion of the former and an explicit discussion of economic principles for organizing transport industries efficiently are welcome innovations, although Pegrum's treatment of market structure is too largely in terms of the relative applicability of natural monopoly and too little in terms of the number and size distribution of firms in particular markets, leadership, and the dimensions of intramodal and intermodal competition other than price. The text's brevity and the exposition of relevant theory limit Pegrum's description and analysis of rates, rate structures, and their bases to such extent that supplementary readings in other sources will have to be assigned. This is perhaps the most significant opportunity cost of the book's emphasis on the allocative efficiency theory.

Part III gives a concise account of the development of transport regulation as a process, dealing broadly, critically, and fully with legal and economic foundations but less completely and specifically with administrative policies and their economic effects. Part IV treats the major problems and issues of national transport policy, beginning with a somewhat repetitious survey of the issues as a whole for the sake of perspective and to draw attention to areas where allocative efficiency reasoning is regarded essential to progress in policy adjustment to modern conditions. One might argue with Pegrum's view that intramodal competition cannot be workable within railroading as dealing only with one dimension of competition—price competition—and as neglecting traffic-capacity relationship changes since the rate wars of the last century, and with his preference for regional railroad monopolies. But, with some repetition, he has succeeded in pointing up the real issues over the role of regulation, organization of transport industries, capital formation and investment, and administration and coordination of governmental policies. Part V is another much-needed innovation in transport texts—it treats the complex urban transport problem in three discerning concluding chapters. Finally, President Kennedy's Transportation Message of April 1962 is helpfully reprinted in Appendix I, and Appendix II gives selected references for further reading.

There is much to commend in Pegrum's text—its unifying emphasis on allocative efficiency, its inclusion of relevant organization and pricing theory, its modern features, its avoidance of excessive descriptive matter, its readable type and brevity, and its forcefully stated and logically derived conclusions with respect to public policy. In this reviewer's judgment, these things outweigh such defects as its omission of references to the originators of critical thinking on some aspects of public policy and its relative neglect of locational theory, rates and rate structures, ICC policies as revealed in decisions, and of the economic effects of various regulatory policies. Perhaps, the book's greatest defect, though, is its failure to present at least a summary comparison of the relevant unit costs of the competing modes of transport for various types of traffic and demands. Finally, Pegrum's treatment of discrimination theory and measures may draw some dissent.

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Labor Economics

The Economics of Labor. By E. H. PHELPS BROWN. New Haven and London: Yale University Press, 1962. Pp. xi, 278. $6.00; paper, $1.45.

The path of industrial development is marked by shimmering monuments to man's enterprise, but strewn along the way is the wreckage of untold resources. From one turning point we view the fearful image of Silas Marner, by-passed by changes in the methods of doing the world's work; at a straight and narrow stretch we meet the faceless organization man who greets us in rigorous FORTRAN; yet over a more random course we are favored by a glimpse of Lady Chatterley and her lover who have rejected—and have been rejected by—the altered modes of social exchange.

I am prompted to such allusions by the spacious essay of E. H. Phelps Brown in which I travel from pre-Christian Greek and Roman worlds to post-Christian Soviet Russia, from medieval Britain and France to contemporary Portuguese East Africa, from the rational method of customary societies to the methodical rationalism of modern, advanced economies. Only an outstanding economist and essayist could have come so close to carrying off in seven chapters and 250 pages a work of so great compass. And Phelps Brown is this economist and essayist.

The first chapter sketches briefly the interdisciplinary foundation on which the study of labor economics is built. Next we read of the rise of the modern wage earner, trade unionism, and governmental concern for the problems of labor. Then there is an examination of those forces affecting the quality of labor. A fourth chapter treats the deployment of labor; a fifth, the determination of wages by conventional and market forces. Then, the origins and effects of collective bargaining and state regulation are dealt with. The last chapter takes up the general level of real wages as influenced by the growth of population, capital, output, and technology.

The brief outline given above cannot convey the historical insight and cultural vision of the author. About the quality of the labor force we learn that "Swahili, for instance, has no words for engine parts and cannot express fractions." And the workers "may be herded into occupations according not to their aptitudes but to caste, village, or tribe they come from." Obviously, the training of such labor entails different tactics from those adopted in advanced societies, even if the economic objectives are nearly the same. We are thus reminded of constraints imposed by mores upon economic affairs.

Two major themes emerge from the book—custom and economic law. Phelps Brown wisely ignores the sterile debate between those schools we call institutionalist and theoretical and, instead, gives credit to the valid arguments of each. As an example of the force of custom in wage relationships, he cites the persisting relative wages of English masons, carpenters, and laborers. From 1412 to 1914, the rates of the first two remained relatively equal and each was about 50 per cent more than that of the laborers. No strange balancing of supply and demand is likely to have produced this equilibrium over five centuries. Custom appears dominant.

In two ways the author reveals the workings of seemingly inexorable economic law. First, the log-normal distribution of earnings is discussed, and reasons are advanced for its congruence with diverse categories of economic data; second, and at greater length, the stability of labor's share in national income. (Here I become persuaded that the stability is more remarkable than Solow has suggested.) The author begins with international comparisons of the wage-income ratio, develops simple definitions of the distribution of output between labor and capital, and then proceeds to reconcile the aggregate Cobb-Douglas production function and its distributive characteristics with the dynamics of technological change. All these tasks are skillfully executed.

There is but one fault I find in the book, and it arises from the essay form. The paucity of footnotes, the absence of graphs and tables (there is one table: the income account of the Soviet Union in 1955), and the temporal and spatial sweep of the writing—these leave the reader somewhat uneasy. The distinguished Committee for the series in comparative economics—of which *The Economics of Labor* is Volume I—expresses the hope that the work "may help teachers to give their courses a broader and more comparative orientation." While I am sure the hope is justified, I am equally sure that much effort will be required by the teacher to give detail to the contours of the essay.

The bibliography consists of selected books and a host of articles from 39 journals of various countries. It seems odd to me that the journal citations omit the titles of the articles, the only guide to subject matter being the headings of the categories into which the articles are grouped.

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Aspects of Labor Economics—A Conference of the Universities-National Bureau Committee for Economic Research. National Bureau of Economic Research Special Conference Series No. 14. Princeton: Princeton University Press, 1962. Pp. xii, 349. $7.50.

This volume contains eight papers as samples of current research on problems in labor economics. Five of the eight papers deal in some manner with the operaton of labor markets. They include another excellent summary article by Melvin W. Reder, "Wage Differentials: Theory and Measurement," in which he gives a clear evaluation of recent studies of occupational and industrial wage structure and adds his own analysis. Reder concludes that relative wages over long periods of time, both by skill and by industry, have behaved more or less as though they were market prices predominantly determined by competitive economic forces. However, he adds that over the short run the competition hypothesis seems less reliable.

In a related paper, "The Effects of Unions on Industrial Wage Differential," H. Gregg Lewis provides a more detailed summary of some 17 studies which have attempted to assay the factors influencing industrial wage differentials. Using the data of these studies, he attempts a new analysis of the degree to which unionism has affected the relative wage of various industries and the dispersion of wages among industries. He concludes that unions had a small impact on the latter but, particularly following World War II, had a significant effect on the former.

George P. Schultz's "A Non-Union Market for White Collar Labor" adds one more to the list of studies of wage rates for similar jobs in local labor markets. At the time it was made, this one had the new twist of dealing with white-collar workers, viz. female clerical workers in banking and insurance. Shultz finds a much narrower range (10 to 12 per cent) of wage rates among firms than has been true of previous studies. However, this is readily accounted for by the important role of market intermediaries (high school guidance counselors, employment agencies, *et al.*) and by the fact that two closely related industries rather than all users of clerical workers are studied. As one of the critics, Richard A. Lester, points out, for a wider group of industries there were ranges of 20 and 30 per cent in wages for similar female clerical jobs even in industry averages.

In their "Competition, Monopoly, and the Pursuit of Pecuniary Gain," Armen A. Alchian and Reuben A. Kessel suggest that actual or potential government limitations on profits of monopoloidal firms result in maximizing a combination of profits and nonpecuniary gains, such as furnishing offices lavishly and hiring people for reasons other than ability. The authors argue that this should not be considered inefficiency since it is not possible for the firm to obtain more pecuniary return without losing some nonpecuniary benefits, or vice versa. However, while such action is not inefficient for the firm, it is inefficient for the economy in that unnecessary inputs are being utilized in the production of a good. In applying this analysis to unions, the authors suggest that in setting a monopoly wage, the union agent and members may benefit by being able to ration jobs to particular ethnic or religious groups and by providing large benefit programs which redound to the benefit of union agents as well as members.

Simon Rottenberg's "The Economics of Occupational Licensing" reports what one might expect: that the demand for such licensing comes from current practitioners, whose incomes are raised because of higher entry costs into the occupation, rather than from consumers seeking quality safeguards, and that the people to be regulated control the licensing agency. While Rottenberg clearly establishes the economic ills of licensing, he fails to ask the crucial question: are these ills worse for society than the possible dangers to health and safety of not licensing such occupations as barbers, airline pilots, and embalmers?

While these studies add to our understanding of labor markets, they by no means resolve the continuing question of the effectiveness of competitive forces in establishing equal wages for similar jobs or of the impact of various interferences with market forces, particularly unions. Clearly, market competition plays an important role in determining wages. But the continuing evidence of industrial wage differentials, which appears again in Shultz's, Reder's, and Lewis' chapters, strongly suggests that more than short-run disequilibria or quality differences are involved in interfirm wage differentials. The Alchian-Kessel paper formally and in detail suggests one possible explanation frequently suggested by students of labor markets in the past.

In his chapter, "Labor Force Participation of Married Women," Jacob Mincer uses family income and earnings of female workers as the chief "explanatory" variables for variations in wives' propensity to work. He separates income into permanent and transitory components *à la* Friedman. His estimates of the elasticities of wives' propensity to work with regard to each of the three variables reveals that female earnings and transitory income are about twice as strong as permanent income in influencing labor supply.

These findings suggest a possible explanation of the conflict between cross-sectional data, which indicate that wives' propensity to work falls with rising incomes of husbands, and the rapidly rising trend in female labor despite increasing incomes. The earnings of wives do not vary as closely with earnings of husbands in cross-sectional data as do increases in male and female earnings over the years. Thus, the positive impact of rising female earnings on the supply of women workers is much greater over time. In addition, transitory income is more important to variations in income of cross-sectional studies than to income changes over time. Since the negative elasticity of the female labor supply is greater with respect to transitory income than to permanent income, the tendency to show a decline in wives' participation as family incomes rise is stronger in cross-sectional data.

The paper provides fresh insight into an important problem in labor economics. The question might be raised as to whether the estimation of the transitory income elasticity is biased upwards because the measures are derived from situations involving only declines from permanent levels. The reaction here may be stronger than to increases. Given the increase of labor-saving devices in the home over the years, it might also be interesting to bring into the analysis some concept of relative marginal productivity of work in the home and work in the market.

In his "Method and Substance in Theorizing about Worker Protest," Abraham J. Siegel sets forth the elements of a theory of worker protest which covers the sources of protest, i.e., a gap between expectations and what exists, the nature of protest, and its impact, i.e., its effectiveness in achieving change in the web of rules which govern work, and how protest develops over time. The remaining paper, "The Economics of American Negro Slavery, 1830-1860," by Robert Evans, Jr. is another example of the use of quantitative methods in the study of economic history. Using somewhat different measures and data, Evans finds, as did Conrad and Meyer,[1] that the rate of return on capital in slaves was comparable to other investment opportunities. Pointing out that in a declining industry profit rates may be maintained as a result of decreases in the value of specialized capital, Evans goes on to show that the industry evidenced no signs of dying in that the relative price of slave labor services to free men's wages did not decline, the price of slaves as capital goods increased more than railroad stocks rose, and the relative price of females (the producers of new slave capital goods) to male slaves appreciated slightly.

The volume points up the strong trend over the past decade in re-emphasizing economic analysis rather than institutions and human relations in the study of labor. In the reviewer's opinion this has been a salutary, exciting, and productive movement. However, as one of the greatest students of labor economic theory stated:

> It has never been the general rule that wage-rates have been determined simply and solely by supply and demand. . . . There has in consequence always been room for wages to be influenced by non-economic forces. . . .
>
> But the economic forces were not abolished by the increased resistance which could be afforded to them . . .[2]

It is important to keep these words in mind as we move from theory to empirical analysis or policy decisions.

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[1] Alfred H. Conrad and John R. Meyer, "The Economics of Slavery in the Ante Bellum South," *Jour. Pol. Econ.*, April 1958, *66*, 95-130.

[2] J. R. Hicks, "Economic Foundation of Wage Policy," *Econ. Jour.*, Sept. 1955, *65*, p. 390.

Strategy and Collective Bargaining Negotiations. By CARL M. STEVENS. New York: McGraw-Hill Book Co., 1963. Pp. xiii, 192. $6.95.

This book is a valuable contribution to the analytical theory of bargaining strategies in labor-management negotiations. It makes some use of analytical tools borrowed from game theory and utility theory, but its basic theoretical assumptions are derived from Neal E. Miller's model of choice behavior in psychological conflict situations. Under the utility-maximization model used in conventional economic theory, if we are faced with the problem of choosing between two unpleasant alternatives, we shall simply choose the one of lesser disutility. In contrast, under Miller's theory, such a choice situation will give rise to a psychological conflict, so that we may find it very difficult or even impossible to make any choice at all; and instead of choosing one of the alternatives we may resort to all sorts of emotional behavior.

Carl Stevens argues that collective-bargaining negotiations primarily serve the purpose of allowing each party to escape the unpleasant choice between fully accepting the opponent's terms and having to put up with a strike situation, if some mutually acceptable compromise solution can be found. In these negotiations each party has two main strategies. One strategy is trying to convince the opponent that he *overestimates* the costs he would incur by making concessions. The other strategy is trying to convince him that he *underestimates* the costs he would incur by refusing these concessions and allowing a strike situation to arise. Other things being equal, the former strategy is more effective because it tends to mitigate the opponent's inner conflict and so makes concessions psychologically easier for him. The latter strategy rather tends to intensify the opponent's psychological conflict. Though it may possibly succeed in extracting concessions from him, it always involves the risk of freezing the opponent into some sort of emotional-resistance behavior.

In early stages of the negotiations both parties will overstate their minimum demands. But in later stages they must find ways of conveying more realistic information to each other about their true attitudes, since no agreement can be reached if the parties persist in thinking that their demands are irreconcilable. It is difficult to indicate to the opponent one's readiness to accept a compromise without making him regard this as a sign of weakness. Stevens has many interesting things to say on how this can be accomplished. He also has an instructive discussion on negotiation rules, on rationalization vs. persuasion, on threats (both bluff and non-bluff), on the role of mediators, etc.

The reviewer agrees with Stevens in considering Miller's conflict theory to be a useful tool in analyzing certain aspects of bargaining negotiations. It points to the constant danger that a mishandling of the psychological aspects of the negotiations, e.g., excessive reliance on coercive tactics, can easily lead to a breakdown. But in our view it is a serious weakness of Stevens' analysis that he makes no real attempt to integrate Miller's theory of conflict choice with the conventional theory of non-conflict choice based on utility maximization, and more generally with modern decision theory. One possible approach would be to regard psychological conflict as an aspiration-level phenomenon: in choosing between two or more alternatives we experience a conflict if none of these alternatives would yield a high enough utility pay-off to satisfy our aspiration level. (Hence one basic aspect of bargaining is trying to lower the opponent's aspiration level.)[1]

We also agree with Stevens that game-theoretical models (up to quite recently) have abstracted from some important aspects of real-life bargaining (e.g., limited information, the threat-commitment problem, etc.). However, in the reviewer's view, Stevens greatly underestimates the explanatory value of game-theoretical bargaining models, and his analysis would have gained in depth by greater use of game-theoretical tools. For instance, we hardly need the concept of psychological conflict to explain why the two parties are willing to negotiate and to accept some compromise (pp. 16-18). In good agreement with common sense, game theory suggests that the parties enter into negotiations mainly to learn more about each other's attitudes (each other's utility functions), and to discover ways of satisfying each other's minimum demands at the lowest cost to themselves. It also suggests that the parties become willing to accept a compromise basically because they realize that the opponent is unlikely to agree to anything better than that. In particular the Nash-Zeuthen bargaining model shows that certain very natural rationality assumptions about the two parties' bargaining behavior uniquely determine the compromise solution the parties will agree upon. Moreover, Nash's theory of optimal threats enables us to formalize what Stevens calls the "hurt-more" criterion for threats (pp. 87-88) without using interpersonal comparisons of utility (which are hard to interpret in a nonethical context).

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[1] For comments on another aspect of this problem, see my review of Boulding's *Conflict and Defense* in *Am. Econ. Rev.*, Sept. 1962, *52*, 904.

Stevens' reluctance to use Nash's bargaining model is probably due to the fact that he is familiar only with its arbitration-model interpretation and is unaware of the possibility of interpreting Nash's model as a theory of rational concessions motivated by the risk of a conflict (e.g., of a strike).[3]

In spite of these criticisms, this reviewer thinks Stevens' book is an important step forward and sheds new light on many interesting aspects of bargaining tactics in labor-management negotiations.

JOHN C. HARSANYI

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[3] See, e.g., John C. Harsanyi, "Approaches to the Bargaining Problem Before and After the Theory of Games," *Econometrica*, April 1956, *24*, 144-57; and "On the Rationality Postulates Underlying the Theory of Cooperative Games," *Jour. of Conflict Resolution*, June 1961, *5*, 179-96, as well as the further references there quoted.

Problems of Wage Policy for Economic Development—With Special Reference to India. By SHREEKANT A. PALEKAR. New York: Asia Publishing House (Taplinger, distributor), 1962. Pp. xx, 343. $14.50.

Shreekant A. Palekar's interesting work is a comprehensive critique of current industrial wage policy in India. In an introductory chapter on the "dilemmas of wage policy," he delineates his topic and states somewhat aphoristically his major themes: the mobilization of human resources is as vital for industrialization as the mobilization of capital; the planning of wages and of human resource development are as necessary as the planning of investment and of output; rapid economic development may require authoritarian or totalitarian methods of "reconciling" divergent political interests with "economic necessities"; the Indian government has paid lip service to the importance of labor for development, but has not instituted wage planning; pluralistic methods of wage determination and the government's preoccupation with problems of distributive equity have had undesirable effects on wages, employment, and thus development.

In the next two chapters, Palekar takes statistical inventory of past developments. First, he describes the structure of the labor force, the forms of wage payment, and the structure of wages and earnings for the postwar period. Then, concentrating on the years 1950-55 and linking up with his earlier work on the years 1939-50, he presents trends of money wages and relates them to trends in the cost of living, employment, profits, labor productivity, and capital intensity. Both chapters contain a wealth of empirical information and are extremely valuable for this reason alone. Moreover, they are supplemented by comprehensive statistical appendices which also extend the author's major series to the years 1955-59 in a preliminary form.

In the following three chapters, Palekar elaborates upon his initial themes and makes use of Indian as well as other data in support of his arguments. In a chapter on "wage policy and capital formation," he focuses on the relation between worker consumption and worker productivity. He asserts that the level of living of the Indian workers is too low and proposes increases in food intake, etc. in the expectation that consequent increases in labor productivity would lead to greater capital formation. In defense against potential critics, he reviews the employment and price effects of the necessary wage increases. He arrives at the unorthodox conclusion that "it is by no means clear that a rise in wages will invariably reduce employment by the substitution of labor by capital in underdeveloped countries" (p. 165). And he finds that "the problem of inflation" is not attributable to wage increases, but mainly to monetary and fiscal policies which induce cost of living adjustments in money wages. Under the current system of granting dearness allowances, moreover, undesirable distortions of the wage structure result.

In summary, Palekar indicts the Indian government for disregarding and evading in its five-year plans problems of wages and human resource development. He charges it with preoccupying itself with utopian schemes of profit sharing and the like instead of doing "first things first," i.e., providing minimum sustenance for the workers. And he attributes this orientation to an important characteristic of the government's pattern of development planning. "The root cause of this refusal in the second five-year plan to face the basic problems of wage policy on a national level may be sought in its refusal to face the still more basic problem of agrarian reform, commandeering the stocks and controlling the distribution of food stuffs and other necessities of life" (p. 186).

Palekar's own "approach to wage policy for economic development" is implied in this basic criticism. He advocates a radical departure from current practices and the adoption of Soviet forms of wage determination which are to be reinforced by rationing of essential consumer goods. To the extent that wage planning of this type is incompatible with "industrial democracy," the industrial tribunals, unions, and employers should be limited in their rights and powers. He states: "This approach may appear to be authoritarian. In reality, that is exactly what it is . . ., economic planners cannot have the cake and eat it too!" (p. 225).

The critical comments which this reviewer cares to make concern the author's self-imposed topical limitation and his conceptual approach. Palekar's criticism is pertinent, yet it is destructive rather than constructive for the reason that he does not explore the theoretical and political implications of his main argument comprehensively. First, his preoccupation with wage income in the nonagricultural sector rather than with all forms of income in all sectors is unduly restrictive because he deals with an economy in which wage laborers are greatly outnumbered by self-employed craftsmen and peasants and in which, according to his own assertion, rapid development depends decisively on proper solutions to intersectoral transfer problems. Second, Palekar's major criticism of Indian wage policy and implicitly of Indian income policy hinges on an unorthodox view of the relation between labor force consumption and labor force productivity. But he concentrates unduly on its physiological dimension and consequently overlooks many motivational problems which seem to worry Soviet planners considerably. He hardly touches upon problems of labor mobility, of work incentives, and of consumer-goods balancing, for instance. Moreover, in dealing with Soviet policy, he ignores Chinese practices altogether. This reviewer looks forward to hearing more from Palekar on problems of Soviet income policy for Indian economic development in the light of Chinese experiences.

PETER SCHRAN

Yale University

Measuring Employment and Unemployment: Report of the President's Committee to Appraise Employment and Unemployment Statistics. Washington: Superintendent of Documents, 1962. Pp. 412. $1.50.

In November 1961, Arthur Goldberg, who was then Secretary of Labor, reported to President Kennedy that "the recent high level of unemployment has stimulated discussion and debate in the public press concerning the employment and unemployment statistics" which "had demonstrated a great lack of public understanding on this subject and much misinterpretation of the statistics themselves." He therefore recommended the appointment of a committee of experts outside the government to appraise current methodology, concepts, and procedures and to suggest desirable changes and improvements. A committee, chaired by Professor Robert A. Gordon, was accordingly set up.

The Committee's Report, issued after nearly a year of study, provides a comprehensive evaluation of the concepts and measurement of employment and unemployment. Together with its several appendices, the Report is an authoritative summary of the existing programs, and its perusal cannot but generate a more sophisticated understanding of the data.

As might have been expected, the Committee reaffirms that "household and employer surveys are doing well the work they were set up to do, that they have not been used to present a false picture to the public, and that the technical procedures which they utilize are appropriate given the limiting and practical conditions within which they must be operated," and commends the technical staffs and administrators of the several agencies "for the great progress made in the past and for the plans and proposals now under consideration for further technical advance."

While the United States was found to have "the most comprehensive system of statistics on employment and unemployment of any country in the world," the Committee was not hesitant in offering comment on weaknesses that exist and on improvements that can be made. Agricultural data were found to be relatively weak—agricultural employment estimates derived from household samples suffer from sampling variability that is too large, while Department of Agriculture estimates suffer from sampling bias, inadequate bench marks, and obsolete methods of seasonal adjustments. Sampling variability is also found to be too large for many other segments of the labor force measured through household inquiries, while employment estimates for construction and service industries based on employer reports suffer from inadequate sampling and bench marks. State, local, and regional estimates are of varying quality and need strengthening. Nor are data on hours worked obtained in the course of household inquiries as good as they should be, while information on hours paid for, derived from employer reports, is not available for many segments of the work force. These are but a few of the Committee's comments.

The concept of employment and unemployment used in household surveys, in the opinion of the Committee, is basically sound, though it needs sharpening. In part, this may be done by adding questions to permit a more objective determination than at present as to whether the particular individuals are really unemployed. The concept of unemployment could be modified by limiting the period of active job search, while the characteristics of the unemployed could be differentiated on the basis of the hours of work they seek on their next job. At the same time, the Committee recognizes that our knowledge about persons reported as outside the labor force is deficient and requires strengthening. The month-to-month variations in the size of labor force remain baffling. In an attempt to obtain greater insights, the Committee recommends that data on "gross flows" in and out of the labor force should be published, despite their admitted deficiencies.

One is not always certain that the Committee is fully conscious of the limitations on the ability to obtain in-depth detail in the course of household inquiries. It does make, however, a number of experimental suggestions for cross-checking information by means of matching surveys, which at times, given proper financial and technical resources, are worth trying.

The Committee examines many other facets of employment and unemployment statistics. With regard to seasonal adjustments, it recognizes that current practices satisfy "minimal requirements," but urges further experimentation and research into the problems of seasonality, particularly through the use of other than the multiplicative or additive models. It also makes several recommendations for more comprehensive presentation of the data.

It is impossible, within the confines of a relatively brief review, to examine all of the suggestions advanced by the Committee. About one-half of its recommendations call for research, some into new and unchartered areas. The feasibility of the undertaking will depend on the largess of the Congress. The final results of experimentation will have to stand the test of time. One thing is certain—a review of a statistical program by an independent committee does bring forth new insights and understanding, even if one is not ready to accept every recommendation. Periodic reviews of this and other statistical undertakings do lead to ultimate enhancement of our statistical intelligence.

LAZARE TEPER

International Ladies' Garment Workers' Union

Employer Concentration in Local Labor Markets. By ROBERT L. BUNTING. Chapel Hill: The University of North Carolina Press, 1962. Pp. xii, 182. $5.00.

Perhaps the first thing that should be said about this book is that it is *not* a study of the effect of monopsony on wage rates. The author candidly states this at the outset and repeats it later. His more modest aim is "beginning the process of building a body of concentration ratios for labor markets comparable with the established body of concentration data now available to students of product monopoly."

Most of this slim volume is taken up with explorations of the conceptual and statistical problems involved in using the available data—employer reports to the Bureau of Old Age and Survivors Insurance—to determine the degree of employer concentration in local labor markets. Like much methodological discussion, it does not make sparkling reading. In this case, it tends more to be tiring than difficult. Data presentations and explanations are straightforward and require no very sophisticated statistical knowledge to follow.

The findings of this study suggest that it is extremely rare for one or a few employers to employ most of the workers in a local labor market. The severest limitation of this finding is that there is no occupational-skill breakdown in the data. Obviously monopsonies could exist in particular occupations without showing up in global studies of this sort. Even a labor market composed exclusively of monopsonists could generate these same data if there were a large number of occupations. The author acknowledges this limitation, which "made it necessary to limit the study to the not-skilled portion of the labor force," where, presumably, a high degree of substitutability would make occupational monopsony unlikely or ineffectual.

While this is a study of labor market structure rather than behavior, it proceeds under the plausible assumption that monopsonistic behavior is correlated with concentration or at least requires some relatively small number of employers—ten or less—to be effective. Deep in the recesses of the Appendix, the author briefly correlates concentration and wage rates and finds that the more concentrated labor markets in his sample had *higher* wage rates, but he hastens to add that these are "exploratory" and "tentative" findings, based on data which "leave much to be desired."

The fact that this book represents a pioneering research effort in an area hitherto dominated by speculation makes its meager and uncertain findings understandable. But this means that its value and appeal may be limited to the specialized researcher in this area, rather than the general reader seeking enlightenment.

THOMAS SOWELL

Douglass College, Rutgers University

Trade Unions in the Age of Affluence. By WILLIAM H. MIERNYK. New York: Random House, 1962. Pp. 180. Paper, $1.95.

William Miernyk addresses his book to the student and interested layman, not the professional. His purpose is to give insight into the current labor situation, and he disclaims any originality. Despite the claim that it is for students, this reviewer believes that the work is too light to be used effectively as outside reading in a course at the college level. The contents make this a book for the layman, and it is on this ground that it must be judged.

Miernyk's debt to Professor Galbraith is not limited to the title. Thus he asserts that the work in imperfect competition theory "culminated in Galbraith's concept of 'countervailing power' " (p. 78). It is a sign of the depth of the book that the author shows the affluence of our economy by the fact that the "average family (of 3.68 members) has disposable income of $7,102" in 1960 (p. 6). Such an unqualified assertion is not at all helpful. Although he acknowledges that there are islands of poverty, the main tone of the book is predicated on this new affluence. One is then surprised to find that the work's temporal focus is from 1935 to the present. The starting point is due to the rise of the "New Unions" and the legal context of labor-management relations.

Most of the book, which is free from the fetters of a Galbraithian *Weltanschauung,* is a creditable account of the changes that have taken place since the mid-1930's. Miernyk recounts the growth in trade union membership, gains won by the unions, and the increased role of union leadership in the noncollective bargaining area. The 1950's, he observes, have caused changes in the position of labor.

As we approach the mid-1960's, he sees the trade union movement as an ineffectual organism. It has won the offensive battles of recognition, higher wages (the author does not analyze the argument over whether the unions have been successful), and unionization of manufacturers. Credit is given to rigorous union leadership, a cooperative government, an expansionist period, and a management community that was ill-prepared for anything but capitulation.

Currently, the unions face new problems and are defensive in their posture. He notes the ineffectiveness of union political activity and the lack of growth in membership. The blame, he feels, lies with union leadership as well as with the affluence of society. This latter factor along with the malpractices in the unions cause disenchantment with the labor movement.

Two other factors also add to the continuing decline of unions: management's expertise in dealing with labor and the "hard line" of Boulwarism, and the change in the content of bargaining.

One of the most important problems treated by Miernyk is the growing difficulty with working rules. He correctly states that this is the source of most industrial conflict today and tries, albeit unsuccessfully, to state the issues. Clearly he is wrong in thinking that working rules are a creature of the 1940's. Whether they retard technological change, as he asserts, may or may not be the case; the problem calls for a sophisticated and detailed analysis.

All this adds up to the thesis that labor thrives in adversity. This does not make a very satisfying thesis, given the history of trade unions in both times of prosperity and recession.

While an attempt is made to analyze some issues and compare arguments by labor authorities, the book is woefully lacking in analysis. No emphasis is placed on structural phenomena in the growth of unions or their relative power. The emphasis is, unfortunately, on individual personalities who, though they may appear interesting, are marginal in the long-run development of unions. The book may help enlighten some about the union-management milieu, but it would be those who are virtually without any background and sopistication in the subject at all.

PAUL A. WEINSTEIN

Columbia University

Labour Policy and Labour Economics in Canada. By H. D. WOODS AND SYLVIA OSTRY. New York: St. Martin's Press; Toronto: Macmillan of Canada, 1962. Pp. xvii, 534. $8.50.

This book is not a comprehensive text on Canadian labor. Rather it deals selectively in Part I with government regulation of labor unions and in Part II with labor supply and wages. Both sections of the book show a high degree of scholarship.

Canadian labor law has developed from two sources. Australian influences were evident in the compulsory conciliation (compulsory investigations) provisions of the Dominion government's Industrial Disputes Investigation Act of 1907. This Act originally applied only to public utilities and coal-mines but was later extended to other industries. The other major influence on Canadian labor law was the United States' National Labor Relations Act of 1935. Sections of this law have been included in the Canadian labor laws.

Provincial labor legislation is more comprehensive in Canada than state labor legislation in the United States because of a 1925 decision of the Judicial Committee of the Privy Council. The Judicial Committee ruled that Dominion Labour Law infringed on the exclusive authority of the provinces to legislate on matters involving property and civil rights of the provinces. This decision resulted eventually in each of the provinces developing a labor law of its own. Although there are some dissimilarities in each of the provincial laws, all of them more or less reflect earlier Australian and U.S. labor policy. Certain differences between Canadian and U.S. labor policy are evident. In order to be certified in Canada, a majority of the employees of the unit must be members in good standing of the trade union. In the United States, certification is dependent upon securing a majority of those voting in a union election. Multi-employer certification in Canada is more difficult to obtain than in the United States, for not only must all employers agree to this unit but also a majority of workers in each firm. Arbitration of grievances is mandatory, either through specific provision (Ontario) or in provisions similar to the Dominion Law that such disputes must be settled "by arbitration or otherwise." Arbitration in Canada generally calls for a three-man board contrasted to the single arbitrator in the United States. Most of the arbitration chairmen in Canada are judges, whereas this group is used much less frequently as arbitrators in the United States. The major difference between Canadian and U.S. policy is that the former requires compulsory conciliation of disputes. Not only are the causes of the dispute investigated, but recommendations for its settlement are also provided. Although the author of this section of the book, H. D. Woods, feels that the major Canadian policy will not be radically revised in the near future, he is critical of the compulsory conciliation sections of Canadian law. He is not concerned so much about the conciliation process itself as about the suspension of the work-stoppage privileges. Strikes have a positive role in posing a threat of losses to both sides. With strikes delayed, the result may be to weaken the process of bargaining.

Part II of the book provides valuable information on the supply of labor in Canada. As in the United States, population is growing and also the percentage of those aged 65 and over. The decline of the rural population is apparent, as is the movement from the Maritime and Prairie provinces to Quebec, Ontario, and British Columbia. Several million persons immigrated to Canada from 1946 to 1960, but about half of this population was lost due to migration to the United States. As in the United States in recent years, the percentage of women in the Canadian work force is growing, although it has not reached the U.S. level. The tertiary industries (services and trade) have been expanding rapidly, as they have elsewhere in industralized nations. The numbers of professional and skilled workers have been expanding, whereas the number of unskilled workers has been decreasing. The separation rates (per 100 persons on the payroll) show an annual figure of almost 75, a larger figure than in the United States. A major weakness in the Canadian system is that only 9 of every 100 children in grade two enter a university.

Since 1953 the number of those unemployed has tended to grow, a trend similar to the statistics in the United States. Like other countries, Canada has some labor markets with much heavier unemployment than other sections. Retraining of the unemployed has been on the same small scale as in the United States, as contrasted with the much larger programs in some of the countries in Western Europe. No mention was made of an Area Redevelopment Program or a Rural Areas Development Program, both recent innovations in the United States. On the other hand, their federal unemployment insurance program pays from 50 to 65 per cent of wages for one year for those with dependents, a much more liberal program than in the United States.

Four chapters are devoted to a statistical analysis of wages in which such topics as the following are covered: productivity, wages and inflation, distribution of income, occupational differentials, interindustry and geographic differentials.

All of the topics discussed in this book were carefully investigated. Not only will Canada benefit from having much additional information on her labor problems, but the U.S. investigators may profit from the thorough treatment of the data.

PAUL A. BRINKER

University of Oklahoma

Population; Welfare Programs; Consumer Economics

Higher Education: Resources and Finance. By SEYMOUR E. HARRIS. New York: McGraw-Hill Book Co., 1962. Pp. 713. $9.95.

Economics of Higher Education. Edited by SELMA MUSHKIN. Department of Health, Education, and Welfare, Office of Education, Bulletin No. 5. Washington: Superintendent of Docs., 1962. Pp. xviii, 406. $1.50.

The Economics of Education. By JOHN VAIZEY. New York: The Free Press of Glencoe, 1962. Pp. 165. $4.00.

Before comparing and contrasting the substantive views of the authors of these books, a general characterization of each of the volumes is in order.

Seymour Harris has written a big, detailed, wide-ranging, well-documented study of economic aspects of U.S. higher education. Four years of work and a good deal of Ford Foundation money were inputs, and the result is an amazingly comprehensive compilation of opinions and data (including considerable original work). The book is intended for a broad audience and is written in nontechnical language. No one who knows Harris will be surprised to hear that he takes stands on such controversial issues as tuition policy, the spending by universities of capital gains accrued on endowments, and class size. Yet, he is also careful to inform the reader of other positions. All in all, this is an extremely useful book, though it would be overgenerous to describe it as brilliant. Considerable repetition and the lack of careful structuring of the material are the main expositional shortcomings; and it is also necessary to note an occasional failure to emphasize the importance of underlying analytical issues.

The Health, Education, and Welfare (HEW) volume contains 21 individual contributions, which of course vary in quality. If forced to single out the four most stimulating papers, I would choose the ones by Harvey Brazer and Martin David ("Social and Economic Determinants of the Demand for Education"), Richard Eckaus ("Education and Economic Growth"), William Vickrey ("A Proposal for Student Loans"), and Richard Goode ("Educational Expenditures and the Income Tax"). With regard to scope, except for a paper on Britain by Dennis Lees, coverage is restricted to higher education in the United States. Neither the internal operating efficiency of institutions of higher education nor the management of endowment funds is discussed in any papers included in this collection; otherwise the coverage by subject matter is almost as broad as in Harris. The editor (Selma Mushkin) deserves considerable credit for adding helpful cross references and for preventing an inordinate amount of duplication. The individual papers do differ considerably in level: to cite the extremes, Bowman's human capital paper is quite technical, while Folsom's "who should pay" paper reads like a speech.

John Vaizey's book is by far the smallest of these three and by far the most ambitious in coverage, ranging from what Adam Smith and others had to say about education, to recent attempts to measure the returns to education, to TV teaching as a means of raising educational productivity, to trends in the salaries of secondary school teachers in Britain, to manpower planning in Pakistan. The style is entertaining (though also overly "cute" in spots), and the discussion is often provocative and stimulating. The argument is not, in general, rigorous—a characteristic that is likely to bother many professional economists.

The substantive views contained in these volumes can be grouped under three main problem headings: (1) determining the over-all size of a country's educational effort; (2) methods of financing higher education; and (3) "productivity" or "efficiency" aspects of the higher education enterprise.

The "how much higher education should there be" problem is largely ignored by Harris, who says in his introduction (p. v) that he is ". . . concerned primarily with two problems: how to get more resources into higher education and how to use them more effectively." Harris does present a 1970 budget (of $9.8 billion, compared with $3.6 billion in 1958), and he indicates some of the underlying assumptions concerning salary trends, degree of emphasis on research, and so on—but he does not really grapple with the question of what the level of expenditure *ought* to be, or, for that matter, with the question of what criteria are relevant to such a decision.

Vaizey does devote one chapter to "The Returns to Education," but the discussion does not seem to me to be particularly helpful. After discussing the "residual" approach (associated with the names of Solow and Aukrust), Vaizey spends some pages criticizing the attempts by Becker and others to measure the direct returns to education by working with lifetime earnings streams and cost estimates. Here Vaizey repeats the usual criticisms of such studies without, unfortunately, explaining that Becker and others have themselves noted the same difficulties and have also tried to take account of some of them (for instance by adjusting intergroup differences in income by intergroup differences in IQ).

One curious feature of Vaizey's treatment of the returns question is his staunch opposition to the inclusion of any estimate of income foregone by students on the cost side of the ledger. Other writers in books under review here (e.g., Blitz, Bowman, Eckaus, Schultz) differ among themselves as to precisely *how* student opportunity costs are to be measured, but only Vaizey argues for not bothering at all. To ignore income foregone altogether is to ignore a very significant cost side consideration indeed, and one wonders if Vaizey's inclination not to include this type of cost stems in part from his general inclination to make out the most appealing possible case for additional educational expenditures.

The HEW volume contains many comments to the effect that the answer to the question, "how much higher education should there be?" is "much more!" Homer Babbidge, in the introduction to the volume, says (p. xv): "In a democratic society there is really no choice but to accommodate the educational demands of the people." And Lewis Conger, in an essay on enrollment projections, says (p. 3): "The demand for college education—the number of potential applicants for college places—is the controlling factor in future enrollments. There is reason to assume that the availability of college places will be accommodated to the demand." Seymour Wolfbein writes of the growing "need" for professional personnel, William Stewart of our educational "needs" in the field of health, and Thomas Mills of national "requirements" for scientists and engineers.

The most disturbing aspect of this type of discussion is the failure to recognize in this context that it is often necessary to make choices among competing ends, and that to make choices intelligently one must compare marginal benefits with marginal costs. The paper by Eckaus is not subject to this criticism and, while rather loosely structured, does contain a number of thoughtful comments on the logic of educational policy-making. Eckaus explains at length the inadequacy of educational policies based on what he calls "the more the better" maxim and then goes on to discuss ways of approximating educational benefits and of relating these benefits to educational costs. As everyone who has worked in this field recognizes, there is much more work to be done here; but Eckaus at least directs our attention to the basic issues.

Turning now to the topic of methods of finance, the first subsidiary question is: what should be the role of student fees in financing higher education? The arguments for and against higher tuition are discussed at length in all three of these books, and especially by Harris, who devotes 120 pages to tuition policy and another 130 pages to scholarships and loans. Harris comes out in favor of higher tuition in both private and public schools, though he is frank enough to note that his views changed in the four years he spent working on this book and that he is now less committed to ***heavy*** (his emphasis) reliance on tuition fees than he was in 1957.

The increasing interest in the tuition question has been accompanied by increasing interest in various types of financing arrangements which would make it easier for students to meet tuition charges. The lengthy discussion of student loans by Harris should be studied carefully by anyone interested in this subject. Worthy of more serious attention than it is likely to receive is the proposal developed by Vickrey, which is similar in many respects to an earlier "equity-finance" scheme suggested by Milton Friedman. In his contribution to the HEW volume, Vickrey suggests (p. 270) "that funds for the financing of the student's education be made available in liberal amounts in return for an agreement to return dividends, computed as a share of later earnings according to the concepts of income taxation." Exemptions and rates of payment would be so arranged that payments would come out of that portion of a person's income attributable to his having had advanced education. Granted, there are practical difficulties, but they do not seem as foreboding as the public relations problem involved in persuading the populace of the merits of trying out this type of arrangement.

Another financing issue discussed at length in these volumes is the likely (and proper) contribution of various levels of government. The situation in the individual states is discussed at length by Harris (who presents a truly impressive array of state-by-state measures of burden, capacity-to-pay, effort, and achievement) and by Mushkin (in the HEW volume). The interstate differences in financial capacity and educational needs are emphasized by both authors, as well as the difficulties presented by the tax systems characteristic of state governments.

Roy Moor (HEW volume) presents a good discussion of the a priori case for federal government expenditures for higher education and also considers at some length the alternative avenues by which the federal government can channel funds into higher education. Moor favors federal scholarships paid directly to individual students, and he is not adverse to considering financial need as one allocation criterion.

At the present time the federal government's main financial involvement with higher education is in the sponsored-research area, and this is the subject of Rosenberg's essay in the HEW volume. I agree with Rosenberg's conclusion that sponsored-research funds have been of tremendous benefit to universities, but I do not think the distinction between "grants" and "contracts" is as meaningful as Rosenberg implies or that the indirect cost debate is as innocuous as he seems to suggest.

The arguments for various kinds of tax treatment for educational expenditures are analyzed carefully by Goode (again in the HEW volume), and it is hard to disagree with his twin conclusions that educational expenditures deserve to be deducted on an amortization basis, and that there is more logic to assigning such amortization allowances to students than to parents.

Let us now move from methods of finance to questions of economy and educational productivity. Here Harris has the field largely to himself, although Vaizey also devotes one chapter to a forceful plea for more experimentation with modern teaching methods. The 230 pages allocated by Harris to "Costs and Economies" are filled with reports of cost studies at particular universities, with the results of experiments testing the effectiveness of teaching in small and large classes, with the records of a host of teaching-method experiments carried out under the auspices of the Ford Foundation, with figures relating unit costs to type and size of institution, and, finally, with comments on the principles involved in locating new schools.

In a brief review of this kind there is little that can be said about this mountain of material except that it will, I think, surprise most of us to learn how much competent work has been done in this field. Incidentally, Harris argues (and I see no reason to disagree) that "the faculty, . . . more than the administration, are responsible for the wastes and the reluctance to adopt improved techniques in higher education" (p. v).

All of the books reviewed here mention over and over that education is more than just a matter of economics, that the cheapest form of education is not necessarily the best, and so on. All of this is of course true, and perhaps such acknowledgments are necessary. I hope, however, the time is near when concern for the effective use of limited resources will be interpreted as concern for the welfare of the people for whose benefit the resources are employed, when economists who seek to clarify educational issues will be looked on with no more than the ordinary (and proper) degree of suspicion accorded economists generally.

WILLIAM G. BOWEN

Princeton University

The Production and Distribution of Knowledge in the United States. By FRITZ MACHLUP. Princeton: Princeton University Press, 1962. Pp. xix, 416. $7.50.

Without any fanfare or claim that his book is about economic growth, Professor Machlup has made a major contribution to this branch of economics. He does not start with concepts and estimates of labor and capital that abstract from their productivity. Instead, his approach is to relate the production of knowledge to changes in occupational structure and to changes in the form and the size of the national product.

Although Machlup is silent about the growth game as it has been played in recent years, a game that has made many economists ever more adept at disembodying labor and capital, he implicitly rejects this approach. He does not build his analysis on man-hours that leave out skills and knowledge or on some empty shells of tangible capital. Machlup's study rests on the proposition that knowledge is a large and an increasing component in U.S. economic activity, that this knowledge affects the productivity of the factors of production, and that the effects of such knowledge can be identified and measured by analyzing the appropriate industries and occupation.

Machlup's long-standing intellectual interest in the economics of patents, inventions, and research has been an excellent training for the comprehensive task undertaken in this book. It also accounts for the special merit of the 62-page chapter on "Research and Development." The concise section on inventive effort and patent protection is a classic, and so is the treatment of the complementarity between basic research and higher education. But the rapidly rising expenditures for research and development are taken too seriously, for much the larger part of these expenditures has been for "development" which conceals an undetermined amount of routine marketing costs. The analysis of the supply of scientists and engineers stops with the number of doctor's degrees awarded in mathematics and in the physical, biological, and engineering sciences, the types of jobs they take, and the shortage of such scientific personnel; the analysis does not extend to the costs of becoming a scientist and the rate of return to the investment in this class of human capital. But the most important unsettled question in this area still is: what is the rate of return to investment in research? While much progress has been made in determining the rate of return to both private and total costs of education, little has been achieved in this respect that bears on research. Machlup points out that marginal return may be zero though average return is still high. It could be, but this does not tell what it is. Businessmen no doubt tend to keep the private rate of return on research expenditures in line with what they consider to be the normal private rate of return. This implies that so-called developmental research, to the extent that all of the returns accrue to the firm, will be close to an optimum in such research activity. But what about the research that produces a return that cannot be captured by the firm doing the research? Machlup's concept of basic research is predominantly of this type. He correctly considers social costs but, in treating research as an investment, social returns appear to be neglected, although at the end there is a strong section on the "Past Neglect of Basic Research."

In his approach to knowledge that is produced by education, Machlup uses an all-inclusive concept. He specifically includes education in the home, church, and the armed services, and on-the-job training, as well as formal education. He is every inch an economist in facing up to the analytical issues implicit in earnings foregone and other opportunity costs of schooling, and the vexing issues underlying the benefits from schooling whether they are "consumption" or "investment" and whether they accrue to students or to others. At times he inundates the reader with tables and takes some statistics too much at their face value. He closes the 94-page chapter on education with the "Machlup Plan" for school reform.

The section, "Training on the Job," should be read along with Jacob Mincer's pioneering paper,[1] which was published after Machlup's book went to press, though Machlup draws on it, criticizes it, and modestly presents an alternative estimate of the amount of on-the-job training. Mincer's estimates, it seems to me, are the better of the two. It is odd how some economists, mainly in Europe, have been long on excuses for not including the earnings foregone by students as one of the costs of schooling. Machlup's treatment of earnings foregone and of other neglected opportunity costs is not only formally correct but refreshingly clear and remarkably all-inclusive. Empirically, Machlup builds on the estimates of earnings foregone that I published in 1960. He raises my estimates for college students by about 5 per cent and boosts those for high school students by a mighty 70 per cent, following Blitz. Machlup notes (p. 98) that my belief is that his estimates are too high. However, recent data indicate that my published estimates in 1960 may be somewhat too high.

Turning briefly to the economic value of education, including that acquired from on-the-job training, Machlup slips momentarily when he states that, "The time interval between the training and the productive use of the skills produced by it is ordinarily too short, both for firms and for the economic analysts, to consider the cost of on-the-job training as an investment" (p. 110). Both the economic logic and the implied facts in this statement are highly questionable. How long must skills last before they are logically an investment? It is a fact that they last for decades and, further, that the costs of acquiring them are predominantly borne by workers and not by firms. The section, "Returns on Investment in Education," is an exceedingly useful critique of most of the relevant underlying issues. The "Machlup Plan" for school reform raises many moot, pedagogical questions that are beyond economics. But there is one important economic feature that Machlup overlooks. Parents who are members of the labor force have opted for fewer hours of work per week as their earnings have risen; but children, according to Machlup, should study harder and longer per week regardless of the decline in hours worked by parents. It would be strange indeed if the income elasticity of the demand for leisure on the part of the family were to exclude all considerations of leisure for children.

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[1] "Investment in Human Beings," *Jour. Pol. Econ.*, Oct. 1962, *70*, Supplement.

The array of "industries" that are engaged in the distribution of knowledge is impressive. Information machines have been multiplying, some of them like rabbits; there are also many information services. All of these, after three chapters of data, are then expressed in rates of increase along with education and research to determine the growth of total knowledge production (Table IX-2). It comes to 10.6 per cent per year (mainly from 1947 to 1958). Total knowledge production in 1958 was about 29 per cent of the appropriately adjusted GNP (p. 362). The last chapter translates this rapid rise in knowledge production into changes in the occupational structure.

Machlup's conception of knowledge and of its application to the U.S. economy clarifies greatly one of the important components of growth. Knowledge is indeed a large part of the productive essence of labor and capital. Real progress is being made in identifying and measuring the useful knowledge that becomes an integral part of man viewed as a productive agent. But when it comes to nonhuman capital forms, the additional productive essence that comes from knowledge still escapes the economist.

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Hospital and Medical Economics—A Study of Population, Services, Costs, Methods of Payment, and Controls. 2 Vols. By WALTER J. MCNERNEY AND OTHERS. Chicago: Hospital Research and Educational Trust, 1962. Pp. xii, 1459. $20.00 the set.

In these 1459 pages, 538 tables, 42 diagrams, and 35 appendices the authors present the results of their investigation, a number of value judgments, and policy recommendations. Hence it is necessary to review the background of this extensive study.

On November 15, 1955, Michigan Hospital Service, the Blue Cross plan of Michigan, filed a request with the state commissioner of insurance for an increase of 23 per cent in the premium rates, this being the eighth request of its kind since the plan was established in 1939. The request was followed by a variety of comments by various persons and organizations in Michigan which resulted in the appointment of a commission by the then governor, G. Mennen Williams, on February 29, 1956. This Commission filed its report in July, 1962. It was established to examine hospital and medical care costs and health insurance coverage and to make recommendations for future action. In turn, the Commission requested the University of Michigan to undertake the study which is reported in these two volumes. The study agency selected was the Bureau of Hospital Administration, School of Business Administration, of the University of Michigan, under the direction of Walter J. McNerney, who was then the Director of the Bureau. The study was financed by grants totalling more than $380,000 from the W. K. Kellogg Foundation. In formulating its 45 recommendations the Commission accepted some of the recommendations of the study and rejected others. It also commented on some of the research and ignored other parts of the research findings.

Following a very lucid introduction, which provides some of the history of the study (and background materials) and outlines its scope, the first volume is developed in the following parts. First, the authors discuss the "consumers of care"; this covers utilization and expenditures for health care, attitudes, and health insurance coverage. The remaining three parts come under the general heading of the providers of care—the character and effectiveness of hospital use, the changing patterns of care, and an inventory of health manpower. The early part of Volume II is devoted to hospitals and allied institutions, their facilities, programs, and costs; and the accounting and finance problems of hospitals. Later in Volume II some extremely interesting and helpful analyses are presented of prepayment (Blue Cross and Blue Shield), insurance, and government; and the characteristics and coverages of each of the three, experience rating and community rating, effect of group characteristics on claim experience, relationship between benefit levels and hospital utilization, effects of layoffs on continuity of coverage and on claim experience, the role of government in personal health care, and controls within the voluntary health system.

Volume II also treats the following topics: description of hospitals and allied institutions; cost increases beyond the control of hospitals; factors related to hospital costs; relationships between available facilities and use rates; needed, existing, and planned facilities; minimum standards for hospitals; and, finally, the summary and recommendations.

A second way to describe the scope of this broad study is to highlight the functional patterns. First, the study covers the expenditures as well as the attitudes, medical "needs," and utilization patterns of the general population. Second, it presents several studies of the providers of care by analyses of the admissions to hospitals and the application of criteria to the care provided—the latter by committees of physicians. Third, it presents studies of insurance agencies and government as the source of funds. Fourth, it reviews the methods of quality control and the utilization of hospitals. This functional description should indicate to the reader that it is primarily a study of hospital economics. The medical economics (part of the title) is largely restricted to the in-hospital practice of physicians; it does, however, include a few items on health manpower as it affects the practice outside of hospitals. Clearly this study is as exhaustive as any in existence on some topics.

In the preparation of this review it has been deemed necessary to read several hundred additional pages, including the final report of the Commission itself and the reactions to the report of some of the agencies concerned—the hospitals, physicians, and insurance organizations. This extra reading was indicated because the study itself really abounds in value judgments, especially as regards investigational methods in areas not subjected to statistical analysis, and the many recommendations that McNerney and his study staff made. In this review, however, a few topics are selected for comment, particularly those that have become important economic questions in recent years.

As noted, the study really originated in the public protests and discussions aroused by a request for a 23 per cent increase in Blue Cross insurance rates. Was this increase necessary? Or was unnecessary hospitalization the culprit? The section of the study dealing with utilization is probably the most useful. Few economists would want to undertake an evaluation of the effectiveness of an industry. But these investigators boldly set up a program to test the oft-repeated claim that over-utilization of hospitals was the basic reason why the Blue Cross in Michigan (and elsewhere) has found it necessary to request successive increases in the premium rates.

The investigators, with the cooperation of physicians, selected 18 diagnostic categories, which described about half of all patients discharged from the hospitals. Sampling procedures were rigorously followed. A committee of physicians representing major specialties was requested to draw up criteria for the investigators for each diagnostic category. In this part of the analysis, the investigators (laymen) leaned heavily upon members of the medical profession for the establishment of standards to determine which patients should have been admitted to hospitals and which should not. They even developed clinical abstracts of the medical records and requested the physicians to re-examine these abstracts. The cases were then classified according to the appropriateness of hospitalization, the length of stay, and the treatment procedures of physicians in the hospitals. Then they held interviews with the attending physicians in their sample cases in those instances in which the criteria did not seem to have been met. A variety of medical and related information was obtained from the attending physicians and the hospital staff which dealt with socioeconomic and organizational factors that might have affected utilization.

A summary of the findings, devoid of many of the extremely interesting details, is as follows: in terms of days of care the "overuse" represented about 7 per cent of the total days of care, and 3 per cent of the patients should not have been admitted. That is to say, 97 per cent of the admissions and 93 per cent of the days were judged appropriate.

The investigators felt that they had succeeded in identifying and measuring the amount of overuse. Their contention that a 97 per cent score on admissions and a 93 per cent score on days of hospital stay indicated great overuse and a threat to the survival of voluntary health insurance and a waste of community "sources" does not seem to indicate anything that drastic. These findings seem, on the contrary, to indicate a much lower degree of overuse of general hospitals than might have been expected on the basis of many contentions about hospital overuse that have been made in recent years during rapid expansion of hospital insurance. Although it is not possible to select an industry comparable with the "hospital" industry, the findings that measured the overuse of hospital facilities indicate, in my judgment, that the "hospital" industry is a reasonably efficient industry. Although the statistical techniques in this section of the study seemed to be appropriate to the task, I do not feel competent to pass judgment upon the many medical matters which were treated by the investigators with the cooperation of committees of physicians. I do believe, however, that the attempt to expand and to improve upon these technical methods for measuring the effectiveness of hospital utilization is probably the high point of this study; and these methods may be useful for many years to come.

The analyses of inventory of health manpower (chapters 33-38) are not nearly so impressive. The study of physician manpower in Michigan involves data assembled by counties and the computation of physician-population ratios for counties and for groups of counties. In this day and age the county boundary line certainly has very little to do with the practice of medicine. Such grouping of physicians and people can scarcely be considered a new method or a reliable procedure for a measurement of supply and demand relationships. Rather the report and the findings of the Commission itself seem to rely more upon hearsay evidence that there is a shortage of various types of health manpower in Michigan. This study adds practically nothing to the procedures that have been followed in the past as substitutes for an analysis of a supply and demand relationship. It is worth noting that the Commission in its own report made no reference to the statistical findings regarding physician manpower. In passing, I do not feel competent to comment on the obvious desire of the investigators that osteopathic physicians in Michigan should be intermingled with doctors of medicine in all considerations relating to health manpower, thereby increasing the supply of physicans.

A third topic in the report deals with the controversial question of how Blue Cross hospital insurance rates should be determined. The controversy centers primarily on the question of using community rates which average out to a common rate (premiums to be charged) regardless of the age, sex, or the probable hospital usage of various groups. For many years Blue Cross has complained that insurance companies select out some of the best group risks in the state or in the nation; an employed group, for example, having a low percentage of female workers or older workers warrants a lower group insurance premium. Closely allied to this problem of charging uniform premium rates for every group is the matter of the deductible feature in major medical insurance, which is growing by leaps and bounds throughout the United States.

As the Commission stated in its report (p. 27): "They [health insurance companies] are not incorporated to provide services; hence they do not fight fires, bury the dead, care for widows and orphans, fight off bank robbers, nor care for the sick or the hurt." On the other hand, Blue Cross plans, and to some extent Blue Shield plans in Michigan and elsewhere, do actually provide services rather than indemnities. The problem of the lower income sector of the population and the aged is stressed in a number of chapters as reasons against lower premiums for preferred group risks. It seems to the reviewer that the conclusions reached regarding the presence or absence of insurance, the types and degrees of coverage, the diverse factors which enter into decisions as to what kinds and types are to be purchased, and related factors were not examined by the investigators with the thoroughness with which they attacked the problem of overuse of hospitals. It is well known to most economists and citizens that the Blue Cross, and to a large extent the Blue Shield, plans are on one side of the fence and the insurance companies are on the other side. It is a competitive matter. I do not feel that this restatement of the well-known arguments of the Blue Shield and Blue Cross plans against deductibles and lower rates for perferred groups is conclusively supported by the investigation. Rather it seems that positions already taken have been reiterated.

These two different methods accomplish the same purpose: each provides citizens with an opportunity, with or without the aid of employers, to prepay the costs of hospital and medical care. From the standpoint of economists, this is probably the great contribution made by the rapid development of all types of voluntary insurance. Stated another way, the test of medical indigency has been altered in the past 20 or 30 years to the ability to *prepay* regularly the costs of medical care from the (earlier) ability to *postpay* the uncertain, and sometimes large, costs of medical care. There exist competitive ways of attaining this objective.

Studies such as this add greatly to our knowledge of this process. They set forth many of the complicating problems with which the "medical care industry" is confronted. As stated earlier, this is truly a comprehensive study. It is a little difficult for the economist to evaluate because the subject includes many medical evaluations.

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Social Security Perspectives—Essays by Edwin E. Witte. Edited by ROBERT J. LAMPMAN. Madison: University of Wisconsin Press, 1962. Pp. 419. $6.75.

Students of social security in the United States and friends of Professor Witte owe a debt of gratitude to Professor Lampman and his colleagues for compiling this volume of essays written over the period 1938-57 by one who has been called "the Father of Social Security." These essays complete a trilogy of Witte's publications on social security. As is appropriate in a final volume, we have here a complete bibliography of Witte's writings on the subject. The other volumes include *Social Security in America* and *The Development of the Social Security Act*. The latter was written in 1936 but published in 1962. These three volumes give the reader essential factual data and insight into Witte's contribution in the field of social security. The final volume is aptly described by the editor "as a kind of intellectual autobiography" in which the reader can trace the development of Witte's thinking as he became intellectually and practically involved in current problems. The essays, however, carry one beyond Witte's unique contribution. They clarify the historical setting of issues and policies during the first crucial decades after the passage of the Social Security Act.

Witte combined those rare attributes essential for effective leadership in a democratic society—the man of action, the critic, and the philosopher. As an exponent of the Wisconsin school of institutonal economists and an active participant in policy formulations, his contribution was made in terms of a straightforward statement of the nature of problems and their significance for our society, his perceptive insight and consistent stand on issues, and his freedom from animosity even when deeply involved personally. The range of Witte's influence is evident in the variety of groups to whom the essays are addressed. These include economists, specialists in social security and industrial relations, social workers, doctors, political leaders, business men, interested citizens, university students and alumni in the United States, Canada, and Lebanon.

Social Security Perspectives is organized into four parts: General Considerations of Social Security, Old Age, Issues in Unemployment and Issues in Health. Each part is introduced by a brief summary of the main points emphasized in the articles which follow. Within each part, the essays are presented in chronological sequence through which the reader may trace the ideas and movements which have influenced the development of the U.S. system of social security. Although Witte's major interest was directed toward social insurance, he was fully committed to the pluralistic approach and the importance of coordinating supporting programs into a comprehensive and well-rounded system of social security. Thus he has interesting things to say about public assistance, private pension plans, supplementary unemployment benefits, and a high level of employment.

The essays stand out as a classic statement of the American interpretation of the responsibilities of a democratic society for the welfare of its members. Witte regards social security primarily as a good in itself in its capacity to serve the individual and, secondarily, as an alternative to socialism and to financing public welfare expenditures from general revenues. Throughout the essays one becomes impressed by Witte's awareness of the practical approach to social legislation—the necessity of adapting preconceived ideas and goals to what is practically possible and the continuing necessity of adapting programs and policies to changing circumstances.

Economists will build on Witte's works as they explore the theoretical analysis of the problems of social security. To be sure, Witte's work lays no claims to distinction in the field of advanced theory, but his straightforward statements of the nature of the problems dealt with and their significance for our society will stand the test of time and change. This is a volume to be read by all scholars who have an interest in the field of social security.

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NOTES

SEVENTY-SIXTH ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

Statler Hilton Hotel, Boston, Mass.—December 27-29, 1963

Preliminary Announcement of the Program

Thursday, December 26, 1963

6:00 P.M. EXECUTIVE COMMITTEE DINNER MEETING

Friday, December 27, 1963

9:30 A.M. PRINCIPLES OF EFFICIENCY

Chairman: PAUL A. SAMUELSON, Massachusetts Institute of Technology
Papers: The Measurement of Waste
ARNOLD HARBERGER, University of Chicago
The Efficient Allocation of Capital
JACK HIRSHLEIFER, University of California, Los Angeles
Discussants: DALE JORGENSON, University of California, Berkeley
WILLIAM VICKREY, Columbia University
TJALLING C. KOOPMANS, Yale University

RE-APPRAISALS IN AMERICAN ECONOMIC HISTORY

Chairman: DOUGLASS NORTH, University of Washington
Papers: A New Look at Hunter's Hypothesis about the Ante-Bellum Iron Industry
PETER TEMIN, Harvard University
Ante-Bellum Interregional Trade Reconsidered.
ALBERT FISHLOW, University of California and National Bureau of Economic Research
Canals and Development—A Discussion of the Issue
ROGER RANSOM, University of Virginia
Discussant: ROBERT FOGEL, University of Rochester and University of Chicago

EFFICIENCY IN AGRICULTURE (Joint Session with American Farm Economic Association)

Chairman: D. GALE JOHNSON, University of Chicago
Papers: The Efficiency of the Farm Firm
EARL HEADY, Iowa State University
The Efficiency of the Agricultural Factor Markets
GEORGE TOLLEY, North Carolina State College
Discussants: GLENN L. JOHNSON, Michigan State University
WILLARD COCHRANE, U.S. Department of Agriculture
KENNETH L. ROBINSON, Cornell University

2:30 P.M. EFFICIENCY IN THE LABOR MARKETS (Joint Session with Industrial Relations Research Association)

Chairman: HAROLD M. LEVINSON, University of Michigan

Papers: The Effects of Unions on the Allocation of Labor
H. G. Lewis, University of Chicago
The Mobility of Labor
R. L. Raimon, Cornell University
The Featherbedding Problem
P. A. Weinstein, Columbia University
Discussants: William G. Bowen, Princeton University
George Hildebrand, Cornell University

Comparative Costs and Economic Development
Chairman: C. P. Kindleberger, Massachusetts Institute of Technology
Papers: The Experience of India
Wilfred Malenbaum, Wharton School
The African Situation
Walter A. Chudson, United Nations
The Case of Brazil
Werner Baer, Yale University, and Isaac Kerstenetzky
Discussants: Albert Hirschman, Wolfgang Stolper, Raymon Vernon

Problems in Taxation
Chairman: Joseph Pechman, The Brookings Institution
Papers: Shifting of the Corporation Income Tax in Manufacturing
Challis Hall, Yale University
Depreciation Allowances and Dividend Policy
John Brittain, Vanderbilt University
Economic Effects of the Capital Gains Tax
Martin David, University of Wisconsin
Discussants: Richard Musgrave, Princeton University
John Lintner, Harvard University
E. Cary Brown, Massachusetts Institute of Technology

8:00 p.m. Presidential Address
Chairman: Edward S. Mason, Harvard University
Presidential Address: Gottfried Haberler, Harvard University

Saturday, December 28, 1963

9:30 a.m. The Regulated Industries
Chairman: Merrill Roberts, University of Pittsburgh
Papers: The Effect of Rate Regulation on Resource Allocation in Transportation
George Wilson, Indiana University
Direct Regulation and Market Performance in the American Economy
Richard Caves, Harvard University
The Effectiveness of Economic Regulation: A Legal View
Roger Cramton, University of Michigan
Discussants: Ernest Williams, Columbia University
Ronald Coase, University of Virginia

Efficiency in the Government Sector
Chairman: C. Lowell Harriss, Columbia University
Papers: Fiscal Institutions and Efficiency in Collective Outlay
James M. Buchanan, University of Virginia
Structure of Government and Public Investment
Julius Margolis, University of California, Berkeley

Effects of Divergences Between Individual and Total Costs Within Government
ROLAND N. MCKEAN, The RAND Corporation
Discussants: MANCUR OLSON, JR., Princeton University
NATHAN ROSENBERG, Purdue University
JEROME ROTHENBERG, Northwestern University

EFFICIENCY OF THE RUSSIAN ECONOMY
Chairman: GREGORY GROSSMAN, University of California, Berkeley
Papers: The Static Efficiency of Soviet-Type Economy
JOSEPH S. BERLINER, Syracuse University
The Dynamic Efficiency of Soviet-Type Economies
BELA BALASSA, Yale University
Socialist International Division of Labor
EGON NEUBERGER, The RAND Corporation
Discussants: JUDITH THORNTON, University of Washington
EVSEY D. DOMAR, Massachusetts Institute of Technology
FREDERIC L. PRYOR, University of Michigan

12:30 P.M. JOINT LUNCHEON WITH AMERICAN FINANCE ASSOCIATION
Chairman: To be announced
Speaker: To be announced

2:30 P.M. EFFICIENCY IN THE TEACHING OF ECONOMICS: THE PRODUCT
Chairman: BEN LEWIS, Oberlin College
Speakers: LEONARD S. SILK, Business Week
ROBERT NATHAN, Robert Nathan Associates
Discussants: JOSEPH A. KERSHAW, Williams College
KENYON A. KNOPF, Grinnell College

CANADIAN-AMERICAN ECONOMIC RELATIONS: EFFICIENCY AND INDEPENDENCE
Chairman: V. W. BLADEN, University of Toronto
Papers: The Canadian Tariff and the Efficiency of the Canadian Economy
H. C. EASTMAN, University of Toronto
The Efficiency of American Enterprises in Canada
A. E. SAFARIAN, University of Saskatchewan
The U.S. Tariff and the Efficiency of the U.S. Economy
ROBERT M. STERN, University of Michigan
Discussants: LAWRENCE KRAUSE, Yale University and The Brookings Institution
C. P. KINDLEBERGER, Massachusetts Institute of Technology
J. H. YOUNG, University of British Columbia

THE THEORY OF MONOPOLISTIC COMPETITION AFTER THIRTY YEARS
Chairman: JOSEPH J. SPENGLER, Duke University
Papers: The Impact on Industrial Organization
JOE BAIN, University of California, Berkeley
The Impact on General Theory
ROBERT BISHOP, Massachusetts Institute of Technology
The Impact on Welfare Economics
WILLIAM BAUMOL, Princeton University
Discussants: P. O. STEINER, University of Wisconsin

8:00 P.M. RICHARD T. ELY LECTURE
Chairman: SIMON KUZNETS, Harvard University
Invited Lecture: Economic Growth as an Objective of Public Policy
JAMES TOBIN, Yale University

Discussants: Harry G. Johnson, University of Chicago
Herbert Stein, Committee for Economic Development

Sunday, December 29, 1963

9:30 a.m. Invited Dissertations, I
Chairman: William Fellner, Yale University
Dissertations: Monetary Mechanisms
J. L. Pierce, Ph.D. California (Berkeley)
The Term Structure of Interest Rates
Burton Malkiel, Ph.D. Princeton
Commercial Bank Portfolio Adjustments
William Russell, Ph.D. Washington
Discussants: Martin Bailey, University of Chicago
Arthur Okun, Yale University
Richard Quandt, Princeton University

Efficiency in the Use of Natural Resources
Chairman: Edward S. Mason, Harvard University
Papers: The Problem of Water Supply and Demand
Irving K. Fox and
O. C. Herfindahl, Resources for the Future, Inc.
The Marine Resources: A Problem in International Cooperation
James Crutchfield, University of Washington
The International Distribution of Petroleum
M. A. Adelman, Massachusetts Institute of Technology
Discussants: Alfred Kahn, Donald White, Nathaniel Wollman

2:30 p.m. Invited Dissertations, II
Chairman: Franco Modigliani, Massachusetts Institute of Technology
Dissertations: The Determinants of Volume in a Probabilistic Market
F. T. Sparrow, Ph.D. Michigan, Johns Hopkins University
The Post-War River and Harbor Program: A Study in Regional Imports
Robert Haveman, Ph.D. Vanderbilt, Grinnell College
Farmer Response to Price
Walter Falcon, Ph.D. Harvard
Discussants: Charles Holt, Robert Dorfmann, D. Gale Johnson

Financial Factors in Business Cycles
Chairman: Bert Hickman, The Brookings Institution
Papers: Financial Markets in Business Cycles: Simulation of an Aggregate Model
Frank de Leeuw, Board of Governors of Federal Reserve System
Longer Waves in Financial Relations: Financial Factors in the More Severe Depressions
Hyman P. Minsky, University of California, Berkeley
Discussants: Moses Abramovitz, Stanford University (invited)
James Duesenberry, Harvard University
John G. Gurley, Stanford University
Alan H. Meltzer, Carnegie Institute of Technology

5:00 p.m. Business Meeting

6:00 p.m. Executive Committee Dinner Meeting

FELLOWSHIPS AND GRANTS

The Social Science Research Council's annual announcement describing fellowships and grants to be awarded to individuals in 1963-64 is now ready for distribution. It lists the following programs which are to be continued without major changes: Research Training Fellowships, predoctoral and postdoctoral; Faculty Research Fellowships; Grants-in-Aid of Research; Fellowships in Political Theory and Legal Philosophy; and Grants for Research on American Governmental and Legal Processes. International Conference Travel Grants will again be offered to social scientists in the United States for travel to conferences outside North America. Inquiries should be addressed to the Social Science Research Council, Fellowships and Grants, 230 Park Avenue, New York 17, New York. Applications for some categories of awards will be due not later than November 1.

The National Science Foundation announces that the next closing date for receipt of proposals for Basic Research in the Social Sciences will be February 1, 1964. The closing date for proposals for the Renovation and Construction of Graduate-Level Research Facilities will be December 1, 1963. Proposals received prior to these dates will be reviewed and notification of the Foundation's action will be made within four months. Inquiries or proposals requesting support should be addressed to the National Science Foundation, Washington 25, D.C.

The American Council of Learned Societies and the Social Science Research Council announce a limited number of foreign area fellowships for the academic year 1964-65. The fellowships are intended to support the combination of multi-disciplinary area training with the spoken and written language of the area and with training in a major discipline. Applications must be submitted on or before November 1, 1963. Application forms and further information may be obtained from The Foreign Area Fellowship Program, 444 Madison Avenue, New York 22, New York.

The American Association of University Women offers 46 fellowships for American women who are established in professional careers and are recognized for scholarly competence. Applications must reach the Fellowships Office, 2401 Virginia Avenue, N.W., Washington 7, D.C., not later than December 1, 1963.

Announcements

A business meeting will be held in Boston during the American Economic Association meetings to discuss the objectives, procedures, and formation of a new association to be known as the Comparative Economics Association. In order to obtain a meeting place, it must be known in advance how many will attend. All who are interested and wish additional information should write to James R. Elliott, Chairman, Pro Tem, Department of Economics, Northern Illinois University, DeKalb, Illinois.

The 1963 meeting of the Midwest Economics Association was held in St. Louis, April 25-27. The officers of the Association are as follows: President, Leland Pritchard, University of Kansas; Past President, Joseph Coppock, Earlham College; President-Elect, Harold Davey, Iowa State University; First Vice-President, Tom Murphy, Notre Dame University; Second Vice-President, Wallace Peterson, University of Nebraska; and Secretary-Treasurer, Lloyd Orr, Indiana University. The 1964 meeting of the Association will be held at the Sherman Hotel, Chicago, from Thursday evening through Saturday noon, April 16-18. Program suggestions should be sent to President Pritchard. Other correspondence should be directed to the Secretary-Treasurer.

The Office of Naval Research and the Institute of Management Sciences announce research support for the topic of capital budgeting of interrelated projects. Manuscripts on this topic must be submitted before June 30, 1964. The author of the manuscript judged best will be offered a year's research time at a salary of $11,000. Any interested person who has received his last academic degree in the past seven years, or who is still a student, may write to the Logistics and Mathematical Statistics Branch, ONR, Washington 25, D.C., concerning detailed requirements in connection with submitting a manuscript.

Deaths

Wolfram Arendt, Elberon, New Jersey, November 27, 1962.

Jules I. Bogen, professor of finance, Graduate School of Business Administration, New York University, May 17, 1963.

Robert A. Brady, professor emeritus of economics, University of California, Berkeley, White Plains, New York, April 27, 1963.

Julian S. Duncan, University of New Mexico, April 15, 1963.

John Boyd Flynn, University of North Carolina, March 2, 1963.

H. Gordon Hayes, professor emeritus and former chairman of the department of economics, Tulane University, May 4, 1963.

Julius Hirsch, New York, New York.

Glenn E. Hoover, Oakland, California, August 1961.

J. R. Karp, assistant professor of economics, College of Business Administration, University of Florida, May 16, 1963.

Willford I. King, Douglaston, New York, September 17, 1962.

George B. Kohnen, San Antonio, Texas.

Joseph L. McConnell, professor, department of economics, University of Illinois, April 7, 1963.

Milo R. Maltbie, New York, New York.

John L. Miller, Madison, Wisconsin.

Thomas M. Mullady, Chicago, Illinois, April 1, 1963.

Alvin S. Newmeyer, Port Washington, New York, November 2, 1962.

Howard K. Nixon, professor of advertising, Graduate School of Business, Columbia University, June 7, 1963.

Albert L. Olson, Bowling Green, Kentucky, February 26, 1963.

Peter A. Reinertsen, Washington, D.C., February 27, 1962.

O. Glenn Saxon, professor emeritus of economics, Yale University, October 5, 1962.

Solomon Shapiro, Bethesda, Maryland, August 12, 1962.

Roswell H. Whitman, Washington, D.C., November 27, 1962.

Tadeusz Wojcik, associate professor of business and economics, School of Business Administration, University of Maine.

Harvey A. Wooster, Winter Park, Florida, September 22, 1962.

Charles Wunder, Pittsburgh, Pennsylvania, January 21, 1963.

Retirements

John R. Bangs, College of Business Administration, University of Florida, June 1963.

B. Haggott Beckhart, Graduate School of Business, Columbia University, June 1963.

P. William Capatch, Graduate School of Business, University of Pittsburgh, August 1963.

Ernest J. Eberling, professor emeritus of economics and business administration, Vanderbilt University, 1963.

Leland J. Gordon, Denison University, June 1963.

Bernard F. Haley, professor emeritus, Stanford University, August 1963.

Sipa Heller, Graduate School of Business Administration, New York University, September 1963.

John A. Howard, Graduate School of Business, University of Pittsburgh, August 1963.

Adolph Lowe, professor emeritus, New School for Social Research, September 1963.

Merrill R. Mikesell, School of Business, Indiana University.

W. Vern Owen, professor emeritus, Purdue University.

Walter R. Peabody, professor emeritus, Rutgers—The State University, June 1963.

George W. Starr, School of Business, Indiana University.

George W. Stocking, research professor emeritus of economics, Vanderbilt University, 1963.

Ivan Wright, Brooklyn College.

Visiting Foreign Scholars

Ragnar H. Bentzel, The Industrial Institute for Economic and Social Research, Stockholm, Sweden: visiting research professor of economics, Yale University.

R. N. Bhargava, Banares Hindu University, Varanasi, India: visiting professor of economics, University of Colorado, academic year 1963-64.

John Black, Oxford University: George A. Miller visiting professor, University of Illinois, 1963-64.

Rosa C. de Cendrero, National University of Buenos Aires: visiting lecturer in economics, San Diego State College.

Terence W. Hutchinson, University of Birmingham, England: visiting professor of economics, Yale University.

Isaac Kerstenetzky, Brazilian Institute of Economics: visiting associate professor of economics, Yale University.

K. G. V. Krishna, The Royal College, Nairobi, Kenya: visiting lecturer, Syracuse University.

Brian R. Mitchell, University of Cambridge, England: visiting lecturer in economics, University of Colorado, academic year 1963-64.

F. Seton, Nuffield College, Oxford: visiting professor, department of economics, Wharton School, University of Pennsylvania, September to December 1963.

Francis M. Wilkes, University of Birmingham, England: visiting lecturer, department of economics, Northwestern University, academic year 1963-64.

John Wise, University of Birmingham, England: visiting professor of economics, Stanford University, 1963-64.

Promotions

Robert L. Able: associate professor of economics, U.S. Air Force Academy.

Robert L. Allen: professor of economics, University of Oregon.

William R. Allen: professor, department of economics, University of California, Los Angeles.

R. Clifton Andersen: associate professor, University of Texas.

W. H. L. Anderson: associate professor, department of economics, University of Michigan.

John W. Ashley: associate professor of economics, University of Missouri.

A. Asimakopulos: associate professor of economics, McGill University.

Alexander S. Balinky: professor of economics, Rutgers—The State University.

John F. Barron: associate professor, department of economics, University of California, Los Angeles.

George J. Benston: assistant professor of accounting, Graduate School of Business, University of Chicago.

Ivar E. Berg, Jr.: associate professor of business, Graduate School of Business, Columbia University.

Calvin P. Blair: professor of marketing administration, University of Texas.

Julian G. Buckley: professor of finance, Graduate School of Business Administration, New York University.

John C. Burton: assistant professor of accounting, Graduate School of Business, Columbia University.

Jesse Carpenter: professor, New York State School of Industrial and Labor Relations, Cornell University.

Charles R. Carr: associate professor of economics and industrial management, Purdue University.

John W. Chisholm, Jr.: professor of economics, Louisiana State University.

Joseph Chung: assistant professor, Kalamazoo College.

Robert S. Cline: professor of finance and insurance, University of Florida.

Robert W. Clower: professor, department of economics, Northwestern University.

Alice H. Cook: professor, New York State School of Industrial and Labor Relations, Cornell University.

John W. Cordell: assistant professor, University of Arizona.

Karl de Schweinitz: professor, department of economics, Northwestern University.

William S. Devino: professor of business and economics, University of Maine.

Robert Doherty: associate professor, New York State School of Industrial and Labor Relations, Cornell University.

David F. Drake: assistant professor of accounting, Graduate School of Business, University of Chicago.

Leroy Dunn: associate professor of economics, Trinity College, Hartford, Connecticut.

Manoranjan Dutta: assistant professor of economics, Rutgers—The State University.

Charles E. Edwards: associate professor of business administration, School of Business Administration, University of South Carolina.

Melvin A. Eggers: professor of economics, Syracuse University.

Merton W. Ertell: Melvin H. Baker professor of American enterprise, State University of New York, Buffalo.

A.C. Flora, Jr.: professor of economics, School of Bussiness Administration, University of South Carolina.

William M. Fox: professor of industrial relations and management, College of Business Administration, University of Florida.

Helmut J. Frank: associate professor, University of Arizona.

Samuel Frumer: associate professor of accounting, School of Business, Indiana University.

Adam Gifford: professor of economics, San Diego State College.

Irving J. Goffman: associate professor of economics, College of Business Administration, University of Florida.

David Green, Jr.: professor of accounting, Graduate School of Business, University of Chicago.

Reed R. Hansen: associate professor of economics, Washington State University.

W. Lee Hansen: associate professor, department of economics, University of California, Los Angeles.

Kurt Hanslowe: professor, New York State School of Industrial and Labor Relations, Cornell University.

Joseph R. Hartley: professor of transportation, Indiana University.

Robert L. Hatcher: associate professor of economics, New York University.

John S. Herin: associate professor of accounting, School of Business Administration, University of South Carolina.

James G. Hilton: associate professor of economics, School of Business Administration, University of South Carolina.

Charles T. Horngren: professor of accounting, Graduate School of Business, University of Chicago.

Ira Horowitz: associate professor of business administration, Indiana University.

George Horwich: professor of economics, Purdue University.

Ching-Yao Hsieh: assistant professor of economics, George Washington University.

Shane J. Hunt: assistant professor of economics, Yale University.

Thomas H. Jerdee: associate professor, School of Business Administration, University of North Carolina.

Shirley Johnson: instructor in economics, Barnard College, Columbia University.

Allen F. Jung: associate professor of marketing, Graduate School of Business, University of Chicago.

Milton Z. Kafoglis: associate professor of economics, College of Business Administration, University of Florida.

C. Harry Kahn: professor of economics, Rutgers—The State University.

John F. Kain: assistant professor of economics, U.S. Air Force Academy.

Marian Krzyzaniak: associate professor of economics, Wayne State University.

Dick A. Leabo: professor of statistics, Graduate School of Business Administration, University of Michigan.

Robert Lekachman: professor of economics, Barnard College, Columbia University.

Richard I. Levin: associate professor, School of Business Administration, University of North Carolina.

Kari Levitt: assistant professor of economics, McGill University.

James P. Logan: associate professor of management, Graduate School of Business, Columbia University.

Robert Lyon: associate professor of economics, Temple University.

Matityahu Marcus: assistant professor of economics, Rutgers—The State University.

David D. Martin: professor of business economics and public policy, Indiana University.

John M. Mattila: professor of economics, Wayne State University.

Marian Meinkoth: associate professor of economics, Temple University.

H. Laurence Miller, Jr.: associate professor, department of economics, University of California, Los Angeles.

Jerome W. Milliman: professor of business administration, Indiana University.

Paul S. Nadler: associate professor of finance, Graduate School of Business Administration, New York University.

Richard V. Nuttall: associate director, executive development program, assistant professor of business administration, Graduate School of Business, University of Pittsburgh.

James M. Patterson: associate professor of marketing, Indiana University.

Edgar A. Pessemier: professor of industrial management, Purdue University.

Harold A. Petersen: assistant professor of economics, Boston College.

Raymond P. Powell: professor of economics, Yale University.

James P. Quirk: associate professor of economics, Purdue University.

John G. Ranlett: associate professor of economics, Sacramento State College.

Ingrid Rima: associate professor of economics, Temple University.

Sam Rosen: professor of economics, University of New Hampshire.

Jerome Rothenberg: professor, department of economics, Northwestern University.

Thomas J. Schillo: assistant professor of business organization and finance, State University of New York, Buffalo.

Raymond H. Scott: associate professor of business administration, Washington State University.

Ezzedin M. Shamsedin: associate professor of economics, School of Business Administration, University of South Carolina.

Richard Sheridan: professor, department of economics, University of Kansas.

Franklin R. Shupp: assistant professor, department of economics, University of Illinois.

Richard A. Siegel: assistant professor, department of economics, State University of New York, Buffalo.

John W. Skinner: professor of economics, George Washington University.

George H. Sorter: associate professor of accounting, Graduate School of Business, University of Chicago.

Bruno Stein: associate professor of economics, New York University.

Robert M. Stern: associate professor, department of economics, University of Michigan.

Anthony M. Tang: professor of economics and business administration, Vanderbilt University.

Philip S. Thomas: associate professor of economics, Grinnell College.

Wilbur R. Thompson: professor of economics, Wayne State University.

Rollie Tillman: associate professor, School of Business Administration, University of North Carolina.

Vladimir G. Treml: associate professor, department of economics, Franklin and Marshall College.

Brian R. Van Arkadie: assistant professor of economics, Yale University.

Ranbir Varma: associate professor of economics, Long Island University.

Harvey M. Wagner: professor of business administration, industrial engineering and statistics, Stanford University.

L. Leslie Waters: university professor of transportation and business history, Indiana University.

John D. Wells: associate professor of economics and statistics, University of Florida.

James E. Wert: Manufacturers and Traders Trust Company professor of finance, State University of New York, Buffalo.

Thomas L. Whisler: professor of industrial relations, Graduate School of Business, University of Chicago.

George W. Wilson: professor of transportation, Indiana University.

James E. Young: associate professor, University of Arizona.

Administrative Appointments

Robert J. Barr: chairman, department of economics, Marquette University.

Ray Billingsley, Texas Technological College: chief, trade section, economics department, Food and Agriculture Organization, Rome, Italy.

Henry S. Bloch, United Nations Deputy Commissioner for Technical Assistance, director, UN Bureau of Technical Assistance Operations, director, Fiscal and Financial Branch: president, Zinder International, Ltd., Engineers and Economists and adjunct professor of public law, Columbia University.

David A. Boody: associate director of executive programs, director, management program for international operations, Graduate School of Business, Columbia University.

Victor Z. Brink: executive associate, office of the dean, adjunct professor of business policy, Graduate School of Business, Columbia University.

Arthur D. Butler: chairman, department of economics, State University of New York, Buffalo.

Robert Campbell: head, department of economics, University of Oregon.

Peter M. Cody: deputy director, AID Mission to Cambodia.

William S. Devino: director, School of Business Administration, University of Maine.

John F. Due: chairman, department of economics, University of Illinois.

James R. Elliott: professor and head, department of economics, Northern Illinois University.

Grant N. Farr, University of Colorado: professor and head, department of economics, Pennsylvania State University.

Robert R. France: associate provost, University of Rochester.

William Haber: dean, College of Literature, Science and the Arts, University of Michigan.

William L. Haeberle: professor of management and director, Indiana Executive Development Program, School of Business, Indiana University.

Howard H. Hines, Iowa State University: program director for economics, National Science Foundation.

Werner Z. Hirsch: director, Institute of Governmental and Public Affairs, University of California, Los Angeles.

Ronald W. Jones: deputy director, AID Mission to Afghanistan.

John Komives: acting chairman, department of economics, Kalamazoo College, 1963-64.

Harold F. McClelland: dean of the Faculty, Claremont Men's College.

Jack Minkoff: chairman, department of social studies, Pratt Institute.

W. George Pinnell: dean, Graduate School of Business, Indiana University.

J. Richard Powell: chairman, department of economics, Long Beach State College.

Simon Rottenberg, Roosevelt University: dean, School of Business Administration, State University of New York, Buffalo.

Howard G. Schaller: chairman, department of economics, Tulane University.

Tibor Scitovsky: chairman, department of economics, University of California, Berkeley.

Selig A. Taubenblatt: director, capital development and finance, Bureau for the Far East, U.S. Agency for International Development, Washington, D.C.

R. L. Thomas: chairman, department of economics and business administration, Ohio Northern University.

D. A. Van Valkenburg, Taylor University: director of admissions and records and assistant professor of business and economics, Malone College.

Ranbir Varma: chairman, department of economics, Long Island University.

Lewis E. Wagner: professor of economics and director, Bureau of Business and Economic Research, State University of Iowa.

Richard S. Weckstein: chairman, department of economics, Brandeis University, 1963-64.

Arthur M. Weimer: special assistant to the President of Indiana University.

Appointments

Don E. Ackerman: instructor, department of economics, U.S. Air Force Academy.

Joseph Aschheim: associate professor of economics, George Washington University.

Wallace N. Atherton: Michigan State University: associate professor of economics, University of Oregon.

Joseph H. Augusta: instructor in economics, University of Maryland.

Frank T. Bachmura: associate professor of economics, Indiana University.

E. Norman Bailey: assistant professor, University of Texas.

David S. Ball, University of North Carolina: assistant professor of economics, Vanderbilt University.

J. T. Ball: assistant professor of accounting, University of Florida.

Maurice B. Ballabon: assistant professor, department of economics, The City College of New York.

Leo Barnes: visiting professor, department of economics, The City College of New York, September 1963 to August 1964.

John P. Barrados: assistant professor of economics, department of economics and political science, University of Saskatchewan.

Robert L. Basmann: professor of economics, Purdue University.

Ralph E. Beals: assistant professor, department of economics, Northwestern University.

Joseph M. Belth: assistant professor of insurance, Indiana University.

Robert L. Bennett, Jr., University of Texas: assistant professor of economics, University of Maryland.

Arnold Berger: assistant professor of business administration and assistant professor of industrial engineering, Graduate School of Business, University of Pittsburgh.

Joseph S. Berliner, Syracuse University: professor, department of economics, Brandeis University.

Richard C. Bernhard: professor of economics, University of Utah.

Zarko Bilbija: associate professor of economics, Florida State University.

James G. Black, Industrial College of the Armed Forces: director of international economics, Battelle Memorial Institute, Columbus, Ohio.

Michael E. Borus: acting instructor in economics, Yale University.

Allan J. Braff, West Virginia University: Northwestern University.

John M. Brazzel: instructor, department of economics, U.S. Air Force Academy.

Carl T. Brehm, Michigan State University: Kenyon College.

Victor Z. Brink: executive associate and adjunct professor of business, Graduate School of Business, Columbia University.

E. Cary Brown: Ford Foundation visiting professor of business economics and finance, Graduate School of Business, University of Chicago, October 1963 to June 1964.

Ronald K. Calgaard: assistant professor, department of economics, University of Kansas.

Conrad P. Caligaris, University of Maine: assistant professor of economics, Boston College.

Jack W. Carlson: assistant professor of economics, U.S. Air Force Academy.

Gabriel Cazell, Ohio Wesleyan University: associate professor of economics, Bowling Green State University.

David H. Clark: assistant professor of business and economics, University of Maine.

Robert E. Cole: assistant professor of economics, Washington State University.

B. Joe Colwell: assistant professor of economics, University of Texas.

Arnold C. Cooper: visiting associate professor of industrial administration, Purdue University.

Earl F. Davis: associate professor of accounting, School of Business Administration, University of South Carolina.

Ralph L. Day: visiting professor of marketing, Pennsylvania State University.

Dewitt C. Dearborn: professor of business administration, University of North Carolina.

Harold Demsetz: associate professor of business economics, Graduate School of Business, University of Chicago.

Carl Dennler, Jr., West Virgina University: Amherst College.

Anthony N. Doherty, Tulane University: assistant professor of economics, University of Oklahoma.

Gerald F. Downey: instructor in economics, Bowdoin College.

Jacques H. Dreze: Ford Foundation visiting professor of applied mathematics, Graduate School of Business, University of Chicago, September 1963 to June 1964.

Richard D. Duckworth: instructor, department of economics, U.S. Air Force Academy.

Warren W. Eason, Princeton University: associate professor of economics, Syracuse University.

Lloyd J. Elliott, University of Texas: associate professor of economics, William and Mary University, Williamsburg.

Norman H. Erb: instructor in economics, DePauw University.

Richard L. Ernst, University of California: instructor, department of economics, University of Illinois.

Robert P. Fairbanks: assistant professor of economics, Northern Illinois University.

Eugene F. Fama: instructor in finance, Graduate School of Business, University of Chicago.

David I. Fand, Southern Methodist University: professor of economics, State University of New York, Buffalo.

Rashi Fein, University of North Carolina: Brookings Institution.

Max E. Fieser: assistant professor of economics, Sacramento State College.

T. Aldrich Finegan, Princeton University: assistant professor of economics, Vanderbilt University.

Thomas J. Finn: associate professor of economics, Wayne State University.

Fred N. Firestone: associate professor of economics, Lehigh University.

Isaiah Frank, State Department: William L. Clayton professor of international economic relations, Johns Hopkins School of Advanced International Studies, Washington.

Richard E. Gift: instructor in economics, Davidson College.

Herman L. Gilster: instructor, department of economics, U. S. Air Force Academy.

Bert T. Glaze, Otterbein College: associate professor of economics, San Jose State College.

Fred Goldner: assistant professor of business, Graduate School of Business, Columbia University.

Jean S. Goodman: assistant professor of business and economics, University of Maine.

Gerald Gordon: assistant professor of behavioral sciences, Graduate School of Business, University of Chicago.

Michael Gort, U.S. Department of Commerce: professor of economics, State University of New York, Buffalo.

Paul T. Green: assistant professor of economics, Denison University.

Gene K. Groff: associate professor of business administration, Indiana University.

Myron A. Grove, Board of Governors, Federal Reserve System: assistant professor of economics, University of Oregon.

Bernard F. Haley: visiting professor of economics, University of California, Berkeley.

Robert J. Hall: instructor in economics, Lehigh University.

Raymond W. Hamilton: instructor in economics, University of Maryland.

Niles M. Hansen. assistant professor of economics, University of Texas.

Robert W. Hartman, Harvard University: assistant professor, Brandeis University.

Judith Hearne: instructor in business statistics, University of Texas.

Edgar P. Hickman: associate professor of economics, School of Business Administration, University of South Carolina.

Claude Hillinger: assistant professor, department of economics, State University of New York, Buffalo.

George W. Hilton: associate professor, department of economics, University of California, Los Angeles.

Werner Z. Hirsch: professor, department of economics, University of California, Los Angeles.

Marvin Hoffenberg, Research Analysis Corporation: head, cost analysis department, Aerospace Corporation, Los Angeles.

Al Hofflander: assistant professor, University of Texas.

Mary A. Holman: assistant research professor of economics, George Washington University.

Alphonse G. Holtmann: assistant professor of economics, Wayne State University.

John A. Howard: professor of marketing, Graduate School of Business, Columbia University.

Donald L. Huddle: instructor in economics, Vanderbilt University.

Gary Hufbauer: visiting lecturer in economics, University of New Mexico, 1963-64.

James E. Hughes: extension associate, department of economics, Iowa State University.

Steven J. Hunter: assistant professor of economics, George Washington University.

Michael D. Intriligator: acting assistant professor, department of economics, University of California, Los Angeles.

Paul Jonas: assistant professor, department of economics, Brooklyn College.

James M. L. Karns: instructor, department of economics, U.S. Air Force Academy.

Herschel Kasper, Iowa State University: assistant professor of economics, Oberlin College.

Charles W. King, Jr.: assistant professor of industrial management, Purdue University.

Frank H. H. King: associate professor, department of economics, University of Kansas.

Robert W. King: assistant professor of economics, Denison University.

Robert Knox, College of William and Mary: assistant professor of economics, Arizona State University.

John M. Kohlmeier: instructor in accounting, Graduate School of Business, University of Chicago.

Hendrik S. Konijn: professor, department of economics, The City College of New York.

Robert O. Krueger, University of Texas: instructor in economics, Chicago Teachers College.

Ching-wen Kwang, Northeastern University: associate professor of accounting and economics, State University of New York, Buffalo.

John D. Lafky, University of Texas: assistant professor of economics, Orange State College, California.

James Clayburn La Force, Jr.: visiting assistant professor, department of economics, University of California, Los Angeles.

Krishnan Lal: assistant professor of economics, department of economics and political science, University of Saskatchewan.

Eugene W. Lambert, Jr.: assistant professor, University of Texas.

James W. Land: lecturer in economics and research staff economist, Yale University.

Raymond L. Larson: instructor in business administration, Washington State University.

Robert J. Lawrence: assistant professor of economics, University of Texas.

Ralph Laws: acting assistant professor, department of economics, University of California, Los Angeles.

Kurt I. Lewin, Bache & Co.: associate in economics, Columbia University.

Cliff L. Lloyd: assistant professor of economics, Purdue University.

Neal B. Long, Jr.: assistant professor of economics, University of Colorado.

Wesley H. Long: assistant professor of economics, Bowdoin College.

Samuel G. Lutzker: director of research, New York State Division of Housing and Community Renewal.

P. John Lymberopoulos: assistant professor of business statistics, University of Texas.

William McDaniel: associate professor of economics, University of Missouri.

Floyd B. McFarland, University of Texas: assistant professor of economics, Oregon State University.

Timothy W. McGuire: research associate, Carnegie Institute of Technology.

Joseph P. McKenna: professor of economics, Boston College.

Robert N. McMichael, Lamar State College: associate professor of business administration, University of Arkansas.

Markos J. Mamalakis: assistant professor of economics, Yale University.

Donald A. Markwalder, University of Toledo: assistant professor, department of economics, University of Illinois.

Wayne S. Marshall: associate in business, Graduate School of Business, Columbia University.

Kanta Marwah: lecturer in mathematical economics, Boston College.

Robert S. Maust: assistant professor of accounting, West Virginia University.

David I. Meiselman: lecturer in political economy, Johns Hopkins University.

Albert Melançon, University of Texas: department of economics, University of Montreal.

David Mirza, Dartmouth: instructor in economics, Kalamazoo College.

James Modisette, Northeast Louisiana State College: assistant professor of accounting, University of Arkansas.

Bruce W. Morgan, Yale University: instructor, department of economics, University of Illinois.

John R. Moroney, Jr.: assistant professor of economics, Lehigh University.

Elliott R. Morss: assistant professor, University of Michigan.

Demetrius Moutsanides, Seattle First National Bank: assistant professor of economics, Central Washington State College.

R. A. Mundell: visiting professor of economics, McGill University, September 1963 for two years.

Weldon C. Neill, University of Texas: economist, Federal Reserve Bank of Dallas.

Carl L. Nelson: George O. May professor of financial accounting, Graduate School of Business, Columbia University.

Fritz Neumark: Mitchell research professorship, department of economics, Columbia University.

Joseph W. Noah, RAND Corporation: economics division, Operations Evaluation Group, Center for Naval Analyses, Arlington, Virginia.

Virgil J. Norton: assistant professor of economics, University of Missouri.

Dennis O'Connor, New School for Social Research: assistant professor of economics, Arizona State University.

Van Doorn Ooms: acting instructor in economics, Yale University.

W. C. Owen: assistant professor of economics, Butler University.

Howard Pack: lecturer in economics and research staff economist, Yale University.

John E. Parker: assistant professor, University of Michigan.

Alberto M. Piedra: assistant professor, Catholic University of America.

James L. Pierce: lecturer in economics, Yale University.

Giulio Pontecorvo: associate professor of business economics, Graduate School of Business, Columbia University.

Larry E. Price, University of Arkansas: assistant professor of business administration, Georgia Southern College.

Sher J. Rana, American University: associate professor of economics, East Carolina College.

Ruth L. Rasch: assistant professor of economics, Wayne State University.

Cadwell L. Ray, University of Texas: assistant professor of economics, Wisconsin State College, Whitewater.

Donald J. Reeb: assistant professor, School of Business Administration and department of city and regional planning, University of North Carolina.

George E. Rejda, Creighton University: assistant professor, department of economics, University of Nebraska.

Stephen A. Resnick: lecturer in economics and research staff economist, Yale University.

Robert G. Rice, Hofstra College: assistant professor of business economics, Graduate School of Business, and research associate, Health Information Foundation, University of Chicago.

Michael Rieber: assistant professor of economics, Wayne State University.

Charles S. Rockwell: lecturer in economics and research staff economist, Yale University.

Thomas J. Rothenberg: assistant professor, department of economics, Northwestern University.

Sheldon H. Rothstein: research associate, Bureau of Economic Research, Rutgers—The State University.

Stephen W. Rousseas: associate professor of economics, New York University.

John W. Rowe, Jr., Iowa State University: assistant professor of economics, University of Oklahoma.

George Sadler, University of Texas: assistant professor, Baylor University.

Rubin Saposnik, Purdue University: associate professor of economics, State University of New York, Buffalo.

Bernard Sarachek: assistant professor of economics, Rutgers—The State University.

Anthony Scaperlanda, University of Texas: assistant professor of economics, St. Cloud State College, Minnesota.

Herbert I. Schiller, Pratt Institute: research associate professor and editor, *Quarterly Review of Economics and Business,* Bureau of Economics and Business Research, University of Illinois.

Lawrence L. Schkade: assistant professor of business statistics, University of Texas.

Hugh Schwartz: instructor, department of economics, University of Kansas.

Richard A. Scott: assistant professor of marketing, University of Texas.

William B. Seale: assistant professor of marketing, School of Business Administration, University of South Carolina.

Sherman Shapiro, University of Notre Dame: economics staff, Comptroller of the Currency, U.S. Treasury Department, Washington.

Robert P. Sharkey: associate professor of economic history, George Washington University.

William G. Shepherd: assistant professor, University of Michigan.

Jack W. Skeels: associate professor of economics, Northern Illinois University.

Arthur A. Sloane: assistant professor of business administration, Indiana University.

Paul E. Smith, San Fernando Valley State College: associate professor of economics, Michigan State University.

Robert E. Smith, University of Utah: associate professor, University of Oregon.

Raynard M. Sommerfeld: assistant professor of accounting, University of Texas.

Nicholas Spulber: visiting professor, department of economics, The City College of New York.

Donald L. Sternitzke: assistant professor of economics, University of Arizona.

Charles T. Stewart: research professor of economics, George Washington University.

Shanti S. Tangri, Southern Methodist University: associate professor of economics, Wayne State University.

Izumi Taniguchi, University of Texas: assistant professor of economics, Fresno State College.

James D. Thompson: professor of business administration, Indiana University.

Richard S. Thorn: assistant professor of economics, Hunter College, The City University of New York.

William D. Thweatt: visiting associate professor of economics and assistant director of the graduate program in economic development, Vanderbilt University.

Elias Tuma: assistant professor of economics, department of economics and political science, University of Saskatchewan.

Jack T. Turner: visiting professor of business administration, Indiana University, academic year 1963-64.

Willy Van Rijckeghem: visiting associate professor of economics, School of Business Administration, University of North Carolina, academic year 1963-64.

Evangelos A. Voloudakis: acting instructor in economics, Yale University.

William D. Wagoner, University of Texas: assistant professor of economics, Louisiana State University.

Harold W. Watts, Yale University: associate professor, University of Wisconsin.

Murray L. Weidenbaum, Boeing Company: senior economist, Stanford Research Institute.

Robert D. Wheeler, Montana State University: assistant professor, department of economics, Long Beach State College.

Thomson M. Whitin, University of California: professor of economics, Wesleyan University.

C. Glyn Williams: assistant professor of economics, Indiana University.

Thomas H. Williams: assistant professor of accounting, University of Texas.

Jame E. Willis: assistant professor of business statistics, University of Texas.

Andrew Wilson, Claremont: instructor in economics, Arizona State University.

Kevin Winch: instructor in economics, Lehigh University.

Whitten P. Windham: lecturer in accounting, School of Business Administration, University of North Carolina.

William D. Wubben, Davidson College: Charlotte College.

Ronald A. Wykstra, General Mills: instructor in economics, University of Nebraska.

Lucia Y. Yu: instructor in business and economics, University of Maine.

Richard Zock: instructor, department of economics, U.S. Air Force Academy.

Leaves for Special Appointments

Mark A. Alexander, University of Pittsburgh: visiting professor, Department of Defense, Washington, fall term 1963-64.

Philip W. Bell, Haverford College: visiting professor of economic development, Makerere University, Kampala, Uganda, academic year 1963-64.

Warren J. Bilkey, University of Notre Dame: economic advisor to the President of the Dominican Republic.

John W. Birch, University of Illinois: visiting assistant professor of economics, Tulane University, 1963-64.

Melvin G. Blase: appointed assistant professor, Iowa State University, Iowa-Peru project on agrarian reform and economic development, AID, July 1963 for two years.

Kenneth E. Boulding, University of Michigan: visiting professor, Tokyo Christian University, Japan, 1963-64.

John A. Brittain, Vanderbilt University: special resident, Brookings Institution, fall semester 1963.

David W. Brown: appointed visiting associate professor, Iowa State University, Iowa-Peru project on agrarian reform and economic development, AID, May 1963 for two years.

Reynold E. Carlson, Vanderbilt University: Ford Foundation in Brazil, 1963-64.

Sherrill Cleland, Kalamazoo College: Economic Advisor to the Jordan Development Board of the Government of Jordan, 1963-64.

Ed F. Crim, University of Oklahoma: head, Honduras project of the University of Oklahoma in Honduras.

John G. Cummins, Tulane University: visiting assistant professor of economics, Southern Methodist University, fall semester, 1963-64.

John M. Dutton, Purdue University: visiting professor of production, University of Birmingham, England, 1963-64.

Edward C. Fei, University of Wisconsin: AID, U.S. Department of State, Washington.

Eberhard Fels, University of Pittsburgh: Institute for Advanced Study, Vienna, Austria, academic year 1963-64.

Raymond W. Goldsmith, Yale University: vice president and director of research, Development Center, Organisation for Economic Co-operation and Development, Paris, 1963-64 and first term of 1964-65.

Leland J. Gordon, Denison University: Fulbright lecturer, University of Hiroshima, Japan, from October 1963.

David Green, Jr., University of Chicago: visitng professor of accounting, Florida State University, autumn and winter quarters, 1963.

Reginald H. Green, Yale University: visiting lecturer in economics, University of Ghana, Lagon, Ghana, 1963-64.

Thomas F. Keller, Duke University: department of accounting, finance and statistics, University of Washington, academic year 1963-64.

Lee R. Kolmer, Iowa State University: Iowa-Peru project on agrarian reform and economic development, AID, June-September, 1963.

Carl Kreider, Goshen College: Fulbright lecturer in economics, Haile Selassie University, Addis Abbaba, Ethiopia.

John P. Lewis, Indiana University: President's Council of Economic Advisers, academic year 1963-64.

Charles E. Lindblom, Yale University: Economic Adviser to the Director of the AID Mission to India and to the Ambassador, 1963 to 1965.

Robert Lindsay, Federal Reserve Bank of New York: visiting lecturer, University of California, Berkeley, academic year 1963-64.

Timothy W. McGuire, Carnegie Institute of Technology: staff, Council of Economic Advisers, September 1963-August 1964.

Frank Meissner, San Francisco State College: UN International Labor Office in association with the National Institute of Productivity, Buenos Aires, Argentina, academic year 1963-64.

Mary E. Murphy, Los Angeles State College: Fulbright professor of business administration, India.

John G. Myers, University of Colorado: Fulbright lecturer, Finland, academic year 1963-64.

William H. Nicholls, Vanderbilt University: agricultural economic consultant, Vargas Foundation, Rio de Janeiro, Brazil, 1963-64.

Charles E. Ratliff, Jr., Davidson College: professor, Forman Christian College, Lahore, West Pakistan under the auspices of Board of Missions, Methodist Church.

John W. Sharp, Ohio State University: director, agricultural economic research in food storage and processing in foreign countries, Weitz-Hettelsater Engineers, Kansas City, Missouri.

Franklin B. Sherwood, University of Illinois: visiting assistant professor of economics, Honduras project, University of Oklahoma, from July 1963.

Alfred G. Smith, Jr., University of South Carolina: Fulbright professor, Johns Hopkins University, Bologna Center, Italy, academic year 1963-64.

Kenneth Strand, Oberlin College: Manpower and Social Affairs Directorate, OECD, Paris, beginning July 1963.

William A. Terrill, University of North Carolina: Cornell University with the department of business management, Middle Eastern Technical University, Ankara, Turkey, academic year 1963-64.

Harry M. Trebing, Indiana University: expert economic consultant, Office of Planning and Special Projects, Common Carrier Bureau, Federal Communications Commission, first semester 1963-64.

Clarence C. Walton, Columbia University: visiting professor, University of California, Berkeley, September 1963 to January 1964.

Arthur M. Whitehill, Jr., University of North Carolina: senior scholar, Institute of Advanced Projects, University of Hawaii, academic year 1963-64.

Clair Wilcox, Swarthmore College: program specialist, Ford Foundation, Kuala Lumpur, Malaya.

Peter J. D. Wiles, Brandeis University: Wenner-Gren Institute, Stockholm.

Pan A. Yotopoulos, University of Wisconsin, Milwaukee: Center of Economic Research, Athens, Greece, 1963-64.

Resignations

Sanford Cohen, Butler University.

Alfred G. Dale, University of Texas.

Richard M. Duvall, University of Texas.

John E. Dykstra, University of North Carolina.

Donald E. Farrar, University of Wisconsin.

James W. Giese, University of Texas.

Edward Greenberg, University of Wisconsin.

Jefferson M. Hooper, University of Arizona.

Edward Jakubauskas, University of Wisconsin.

Duncan M. McDougall, Purdue University.

Ian M. Matley, Columbia University.

W. David Maxwell, chairman, department of economics, Tulane University.

John O'Leary: Lehigh University.

Leigh Rhett: Lehigh University.

Herbert Spitz, Lehigh University.

E. R. Stapleton, Appalachian State Teachers College.

David C. Townsend, University of Texas.

K. J. Walraven, University of Texas.

H. Martin Weingartner, University of Chicago.

Miscellaneous

Robert Ferber has been named editor of a new publication of the American Marketing Association, ***Journal of Marketing Research.***

SIXTIETH LIST OF DOCTORAL DISSERTATIONS IN POLITICAL ECONOMY IN AMERICAN UNIVERSITIES AND COLLEGES

The present list specifies doctoral degrees conferred during the academic year terminating June 1963, and theses undertaken in the same period.

General Economics; Methodology

Theses in Preparation

ROBERT E. COLE, B.S. Iowa (Ames) 1954; M.A. Connecticut 1956. The economics of privilege. *Northwestern.*

JAMES F. HOFHEINZ, B.A. Texas 1960; M.A. 1963. Pragmatic philosophy and the value problem in economics. *Texas.*

MURRAY WOLFSON, B.S. City (New York) 1948. A critique of Marxian economics. *Wisconsin.*

Price and Allocation Theory; Income and Employment Theory; History of Economic Thought

Degrees Conferred

JOHN J. ARENA, Ph.D. Yale 1963. The Pigou effect and consumption: a statistical inquiry.

EITAN BERGLAS, Ph.D. Chicago 1963. Stimulating investment, technological change and growth.

RONALD G. BODKIN, Ph.D. Pennsylvania 1962. The wage-price-productivity nexus.

MICHAEL P. J. BRUNO, Ph.D. Stanford 1963. Interdependence, resource use and structural change in Israel.

JACK W. CARLSON, Ph.D. Harvard 1963. Value of life saving.

SIMONE CLEMHOUT, Ph.D. Mass. Inst. Technology 1963. Essays in production function analysis and application to the Leontief paradox of international trade.

EDWIN K. CLICKNER, Ph.D. American 1963. The influence of classicism on Marxian economic thought.

WILLIAM E. COSTELLO, Ph.D. Georgetown 1963. The shape of the distribution of personal income in the U.S., 1944-59.

JAMES J. DIAMOND, Ph.D. Northwestern 1962. Distributed lags, growth, profits and capital expenditures.

PETER A. DIAMOND, Ph.D. Mass. Inst. Technology 1963. Essays on optimal economic growth.

FRANK T. DOLBEAR, JR., Ph.D. Yale 1963. Individual choice under uncertainty—an experimental study.

DANIEL ELLSBERG, Ph.D. Harvard 1963. Risk, ambiguity and decision.

JESSE C. FISHER, JR., Ph.D. North Carolina 1962. Real balances and consumption in the writings of four Keynesian theorists.

JAMES W. FRIEDMAN, Ph.D. Yale 1963. The theory of oligopoly.

FRANCIS W. GATHOF, JR., Ph.D. American 1963. Some determinants of factor income distribution in the Post World War II period.

LEONARD GROENEVELD, Ph.D. Florida 1962. An inquiry into the marketing views of Alfred Marshall.

CLIFTON M. GRUBBS, JR., Ph.D. Harvard 1963. Pricing highways: an input-output approach.

ASHOK S. GUHA, Ph.D. Harvard 1963. Disguised unemployment and the theory of factor substitution.

JOSEF HADAR, Ph.D. Minnesota 1962. Stock-flow analysis applied to consumer behavior.

FREDERICK S. HAMMER, Ph.D. Carnegie Inst. Technology 1963. The demand for physical capital: application of a wealth model.

ANDREW JACKSON, Ph.D. George Washington 1963. The development of the subjective theory of value through the eighteenth century with special emphasis upon Condillac.

EDWARD R. KITTRELL, Ph.D. Chicago 1962. The development of the theory of colonization in English classical political economy.

MELVIN M. LEIMAN, Ph.D. Columbia 1963. Jacob N. Cardozo: a study in American economic thought in the ante-bellum South.

DANIEL L. MCFADDEN, Ph.D. Minnesota 1962. Factor substitution in the economic analysis of production.

MATITYAHU MARCUS, Ph.D. Brown 1963. Capital-output ratios and their variations.

VINCENT G. MASSARO, Ph.D. Notre Dame 1963. Divergent views on Marx's increasing misery doctrine.

JACK MELITZ, Ph.D. Princeton 1963. The economics of Henry Sidgwick.

WALTER G. MELLON, Ph.D. Princeton 1963. An approach to a general theory of priorities: an outline of problems and methods.

THOMAS O. NITSCH, Ph.D. Ohio State 1963. The redistribution impact of inflation: an historical and theoretical-methodological study.

MANCUR L. OLSON, JR., Ph.D. Harvard 1963. A general theory of public goods.

KENNETH L. PARKHURST, Ph.D. Southern California 1963. Grade labeling and the demand curve: theoretical implications and empirical evidence.

GIACOMO PATRIZI, Ph.D. Georgetown 1963. The evolution of the conception of profits in a dynamic theory of distribution.

RICHARD E. PASTERNAK, Ph.D. Alabama 1962. Jeremy Bentham as an economist.

JOHN T. RADER, III, Ph.D. Yale 1963. Edgeworth exchange and general economic equilibrium.

BERNARD SAFFRAN, Ph.D. Minnesota 1963. A test of the permanent income hypothesis.

MILTON M. SHAPIRO, Ph.D. Southern California 1963. The inflation process: an economic analysis.

FREDERICK T. SPARROW, Ph.D. Michigan 1963. A queuing theory model of market equilibrium.

DE-MIN WU, Ph.D. Wisconsin 1963. An empirical analysis of household durable goods expenditure.

Theses in Preparation

MOHAMED ABOU-ALI, B.A. Alexandria 1958; M.A. Harvard 1962. Effect of income distribution on savings, income rates, and capital formation. *Harvard.*

PIETRO BALESTRA, Lic. Fribourg 1956; M.A. Kansas 1959; M.S. Stanford 1963. Rational expectations, stochastic phenomena, and inventory models. *Stanford.*

GHALIB M. A. BAQIR, M.A. St. Louis 1960. The historical school in economics. *St. Louis.*

CLEVELAND W. BATEMAN, B.A. Western Maryland 1960. Investment behavior of 4-digit manufacturing industries. *Harvard.*

JAMES P. BENNETT, B.A. Pennsylvania State 1958. An econometric study of investment demand in the United States steel industry. *Michigan.*

G. SADLER BRIDGES, B.B.A. Texas A & M 1958; M.S. 1960. H. S. Davenport: a reexamination in light of recent analysis. *Southern Methodist.*

RONALD BRITTO, B.S. St. Patrick's, Karachi 1956. An analysis of efficiency in the allocation of resources in a growing economy. *Brown.*

ROBERT J. CARLSSON, B.A. Rutgers 1960. The tax variable in growth models. *Rutgers.*

PATRICK B. DE FONTENAY, Litt.B. Paris 1954; L.L.B. 1955; Dipl. 1956; M.A. Yale 1957. Cost structure, growth and inflation: an international comparison. *Yale.*

Mohamed A. El-Hodiri, B.E.C. Cairo 1958; M.A. Minnesota 1961. Optimality criteria of economic growth. *Minnesota.*

Harold E. Fassberg, B.A. Ohio State 1940; M.A. Pittsburgh 1942. Contributions to a capital model with efficient paths of capital accumulation by use of the calculus of variations. *American.*

Lewis R. Gaty II, B.A. Swarthmore 1959. Oligopoly theory. *Stanford.*

Seymour Goldstone, B.A. Antioch 1957. Computer simulation of research and development decision making. *Ohio State.*

George Greening, B.A. Kansas City 1948. Marginal cost pricing and deficit financing: an application of income and employment theory to a pricing controversy. *New York.*

Robert O. Krueger, B.A. Kansas City 1959, M.A. 1961. Problems in the character and significance of the institution of property in modern economic thought. *Texas.*

Bong S. Lee, B.A. Pennsylvania 1959; M.A. Harvard 1962. The place of money in the economic growth theory of mercantilism. *Harvard.*

Robert E. Lucas, B.A. Chicago 1959. Estimating elasticities of substitution between labor and capital. *Chicago.*

William McLaughlin, B.S. Hofstra 1951; M.B.A. Arkansas 1957. The market for antique firearms. *Arkansas.*

Joseph D. Mooney, B.A. British Columbia 1961. Accumulation of capital and the distribution of national income. *Toronto.*

Donald Murry, B.A. Missouri 1959; M.A. 1961. A comparative study of external economies in research and development complexes. *Missouri.*

Robert A. Pollak, B.A. Amherst 1960. Problems in intertemporal resource allocation. *Mass. Inst. Technology.*

Frank W. Puffer, B.S. Brown 1960. Optimum decision rules for stabilization of linear economic models. *Brown.*

Alfredo Roldan, B.A. Ecuador 1957; M.A. Minnesota 1960. Education and income distribution, an empirical study. *Minnesota.*

Matthew A. Stephenson, B.S. South Carolina 1957. Theories of technological change in the English classical school and its precursors. *Tulane.*

Samuel Weiner, B.A. Illinois 1955. Determinants of the decision to invest in fixed plant and equipment in industries; an empirical approach. *Stanford.*

William C. Whitten, Jr., M.S. Georgia Inst. Technology 1951. The economic and social ideas of Thomas Cooper. *Alabama.*

Gordon C. Winston, B.A. Whitman 1950; M.A. Stanford 1961. Income and effort: an international comparison. *Stanford.*

Economic History; Economic Development; National Economies

Degrees Conferred

Mustapha M. Alami, Ph.D. Georgetown 1963. Economic planning and development in Morocco.

Khalil M. H. Al-Shamma, Ph.D. California (Berkeley) 1962. Inflation in the economic development of Iraq, 1950-1960.

Donald W. Baerresen, Ph.D. California (Los Angeles) 1963. An analysis of the use of multiple exchange rates for gaining government revenue from foreign trade: Chile—a case study (1931-1955).

Richard A. Bilas, Ph.D. Virginia 1963. The economic development of the Federation of Malaya: 1950-1960.

Donn W. Block, Ph.D. Harvard 1963. Aims and methods of United States technical and economic assistance to Pakistan, 1951-1961.

Kang Chao, Ph.D. Michigan 1962. Indexes of industrial production of Communist China, 1949-1959.

Nanda K. Choudhry, Ph.D. Wisconsin 1963. An econometric model of the Indian economy.

George C. Constantacopoulos, Ph.D. Columbia 1962. Economic development of Greece in the postwar setting.

Leon A. Danco, Ph.D. Western Reserve 1963. Economic development of Paraguay, 1945-1959.

David G. Davies, Ph.D. North Carolina 1963. Agricultural labor requirements and urban growth.

Jim E. Davis, Ph.D. Wisconsin 1963. The economic development of Southern Italy, 1950-1960.

Edwin R. Dean, Ph.D. Columbia 1963. Economic analysis and African responses to price.

George N. Ecklund, Ph.D. Minnesota 1962. Taxation in Communist China, 1950-1959.

Walter P. Falcon, Ph.D. Harvard 1963. Farmer response to price in an underdeveloped area: a case study of West Pakistan.

Mary Fish, Ph.D. Oklahoma 1963. Public Law 480: the use of agricultural surpluses as aid to underdeveloped countries (with special reference to India).

Albert Fishlow, Ph.D. Harvard 1963. The economic contribution of American railroads before the Civil War.

Delbert A. Fitchett, Ph.D. California (Berkeley) 1963. Defects in the agrarian structure as obstacles to economic development: a study of the northern coast of Peru.

Robert W. Fogel, Ph.D. Johns Hopkins 1963. Railroads and American economic growth: essays in econometric history.

Thomas A. Goldman, Ph.D. Chicago 1962. The wool trade of the United Kingdom, 1920-1938.

Michael A. Gomez, Ph.D. Ohio State 1962. The role of international technical cooperation in the interregional development of Peru.

Ralph Gray, Ph.D. Syracuse 1963. The Arkansas public economy and economic development.

Kee C. Han, Ph.D. Boston 1962. A study of the interregional economies of Korea.

Delmar D. Hartley, Ph.D. Kansas 1963. A subregional application of regional economic growth theory.

Michel E. A. Herve, Ph.D. Harvard 1963. Counterinflationary factors in the French economy 1952-1958.

Khateeb M. Hussain, Ph.D. California (Berkeley) 1963. Linear programming for economic development: problem of constraints using a demonstration model for the Mexican economy.

Charles J. Jorgensen, Ph.D. Washington 1962. Transport improvement and economic growth of the West, 1840-1860.

Chuay Kannawat, Ph.D. Wisconsin 1962. Possibilities of industrialization in Thailand.

Richard H. Kaufman, Ph.D. Harvard 1963. Exchange rates and economic development: the experience of Israel.

Joseph D. Khazzoom, Ph.D. Harvard 1963. The impact of deficit financing in the underdeveloped countries: present and future.

Robert W. Knapp, Ph.D. Michigan 1963. United States direct investment in Canada, 1950-1960.

Stephen R. Lewis, Jr., Ph.D. Stanford 1963. Taxation and growth in the dual economy: an evaluation of tax devices in underdeveloped countries.

Samir A. K. Makdisi, Ph.D. Columbia 1961. Syria: the public sector and economic growth, 1945-1957.

Charles Mansfield, Ph.D. Princeton 1963. Security market development under conditions of an unstable currency: the Mexican case.

Marvin P. Miracle, Ph.D. Stanford 1963. Maize in tropical Africa.

Oscar K. Moore, Ph.D. Florida 1962. The Brazilian coffee economy.

Richard Moorsteen, Ph.D. Columbia 1962. Prices and production of Soviet machinery, 1928-1958.

N. Mubarak, Ph.D. McGill 1963. Coffee and the Kenyan economy.

Wallace M. Nelson, Ph.D. Florida 1962. The economy development of Florida, 1870-1930.

Margaret T. Norton, Ph.D. New York 1963. Canadian economic and financial growth after World War II.

Abhaykumar C. Parikh, Ph.D. American 1963. Banking and economic development in India since independence.

Svetozar Pejovich, Ph.D. Georgetown 1963. Economic systems and development, a case of Yugoslavia.

Rudolph Penner, Ph.D. Johns Hopkins 1963. Foreign investment and Canadian economic growth.

Diomedes D. Psilos, Ph.D. California (Berkeley) 1963. The capital market in Greece: a study of finance in economic growth and development.

Marudarajan Ramachandran, Ph.D. Iowa (Ames) 1963. A structural analysis and linear programming within an input-output model emphasizing agriculture in India.

Narindar S. Randhawa, Ph.D. Iowa (Ames) 1963. Regional programming models for agricultural development planning in India.

Richard D. Raymond, Ph.D. Brown 1963. Interaction between discrimination, interregional migration, and regional economic development.

Clark W. Reynolds, Ph.D. California (Berkeley) 1962. Development problems of an export economy—the historical and developmental relationships of the copper industry to the economy of Chile.

J. Thomas Romans, Ph.D. Brown 1963. Capital exports and growth among U.S. regions.

Frank Sanchez-Aran, Ph.D. New York 1963. Financial aspects of economic development in Puerto Rico.

Emanuel W. Sandberg, Jr., Ph.D. Colorado 1963. The viability potential of Colorado's potato industry: a linear programming analysis of Colorado's competitive position.

Neil Schaller, Ph.D. California (Berkeley) 1962. A recursive programming analysis of regional production response.

Karl A. Scheld, Ph.D. Iowa 1963. A comparative analysis of the level and composition of economic activity in 144 Iowa towns.

Carol H. Schwartz, Ph.D. Columbia 1963. Retail trade development in New York State in the 19th century with special reference to the country store.

Gerardo P. Sicat, Ph.D. Mass. Inst. Technology 1963. Some aspects of capital formation in the Philippines.

Balwir Singh, Ph.D. American 1963. Investment criteria for economic development: the Indian experience.

Hadley E. Smith, Ph.D. Southern California 1963. An international comparison of the role of government in the economic development of developed and emerging economies, with particular reference to government corporations.

Howard F. Smith, Ph.D. American 1963. An analysis of Ceylon's ten year plan of economic development.

Wayne W. Snyder, Ph.D. Harvard 1963. Money in a developing economy: a case study of Pakistan, 1953-1961.

Alan Sokolski, Ph.D. Columbia 1963. The establishment of modern manufacturing industry in the Federation of Nigeria.

Marvin J. Sternberg, Ph.D. California (Berkeley) 1962. Chilean land tenure and land reform.

Jeanette Stoops, Ph.D. Bryn Mawr 1963. The effect of bulk purchase contracts on the New Zealand economy.

Vladimir G. Treml, Ph.D. North Carolina 1963. The 1959 Soviet intersectoral flow table.

Mohammad Uzair, Ph.D. Pennsylvania 1963. The development of commercial banking in Pakistan, 1947-1962.

Bernardo M. Villegas, Ph.D. Harvard 1963. Investment criteria in industrial development: a case study of the Philippines.

Caron R. Waits, Ph.D. Texas 1963. Financial and real growth in Venezuela: a study of the relationship between financial and real variables.

Franklee G. Whartenby, Ph.D. North Carolina 1963. Land and labor productivity in the United States cotton production, 1800-1840.

Theses in Preparation

Alan J. Abouchar, B.A. New York 1954; M.A. 1960. Some mathematical techniques in Soviet economic planning. *California (Berkeley).*

Abdelrahman T. Aburachis, B.S. Detroit Inst. Technology 1958; M.A. American 1960. Financing of economic development in Turkey with special emphasis on the public sector. *American.*

Ziauddin Ahmad, B.A. Muslim 1947; M.A. Sind 1949. Deficit financing and inflation in less developed countries. *Harvard.*

Bilsel H. Alisbah, B.A. Pennsylvania 1959; M.B.A. Princeton 1961. Economic writers and economic change in England, 1760-1825. *Princeton.*

Mehar C. Arora, B.A. Delhi 1949; M.A. Pinjali 1952. Economic development of underdeveloped countries with special emphasis on problems of finance. *Rutgers.*

Antonio V. Ayala, B.A. Ateneo de Manila 1957. The use of economic analysis in national policy planning: the Philippine case. *Georgetown.*

Azhari (initials not supplied), M.A. Indonesia. The public enterprises, economic planning and development in Indonesia. *Indiana.*

Robert N. Baird, B.A. Wittenberg 1960; M.A. Kentucky 1962. An input-output analysis of Eastern Kentucky. *Kentucky.*

Avinash P. Bhagwat, B.A. Bombay 1957; M.A. 1960; M.A. Yale 1962. Evaluation of the P.L. 480 program in India. *Yale.*

Robert E. Bickner, B.A. Florida 1948. Insufficient demand, inflation, and economic development. *Columbia.*

John G. Boyd, B.A. Boston 1951. Foreign aid and economic development, the case of India. *Columbia.*

Louis Bravo-Aguilera, B.A. Mexico 1958; M.A. Harvard 1960. The financing of capital formation in Mexico. *Harvard*

Richard P. Brief, B.A. Dartmouth 1955; M.B.A. 1958. Financial interpretation of economic history. *Columbia.*

William S, Cassels, B.A. Oberlin 1953. A critical survey of criticisms of the use of national income aggregates in underdeveloped countries. *Princeton.*

Michel Chirman, Lic. de Montpellier 1959; Dipl. 1959. Quantitative aspects of regional growth in France. *Harvard.*

Kyou E. Choi, B.S. Korea 1959. Leading factors in the economic development of Florida, 1900-1960, *Florida.*

Apostolos Condos, Law Athens 1958; M.S. Iowa (Ames) 1962. Economic development planning. *Iowa (Ames).*

William A. Cox, B.A. Northwestern 1959. Growth with "growth policy"? The case of Austria, 1953–1962. *Princeton.*

Ghazi Douwaji, B.A. American (Beirut) 1959. The economic system of Egypt. *Duke.*

Stanley W. Driskell, B.S. Illinois Inst. Technology 1959; M.A. California (Berkeley) 1963. An analysis of economic causes and results of labor migration in Tanganyika. *California (Berkeley).*

Floyd Durham, B.A. North Texas State 1951; M.A. 1952. Personal income in Comanche County, Oklahoma. *Oklahoma.*

John R. Erikkson, B.A. Wayne State 1958; M.A. 1962. Wage structures and economic development in Latin America. *California (Berkeley).*

Robert M. Field III, B.A. Harvard 1950; M.A. 1957. The productivity of the industrial labor force in Communist China. *Harvard.*

Leonard P. Fletcher, B. Comm. Mount Allison 1959; M.A. Brown 1961. The major determinants of growth in the Trinidad economy. *Brown.*

Kenneth D. Frederick, B.A. Amherst 1961. Import prospects and requirements of India's development. *Mass. Inst. Technology.*

Joseph C. Furey, B.A. Maryland 1948. The role of the merchant in economic development. *Maryland.*

Eduardo Garcia, Dipl. ECLA/TAA 1957. Inflation in Chile, an econometric analysis. *Mass. Inst. Technology.*

Charles Garrison, B.S. Lincoln Memorial 1958; M.B.A. Kentucky 1960. The Eastern Kentucky resource development project: a preliminary evaluation. *Kentucky.*

Douglas H. Graham, B.A. Yale 1955; M.A. Johns Hopkins 1959. A quantitative analysis of the structural change of Brazilian economy. *Harvard.*

Alan G. Green, B.A. Queen's (Canada) 1957. Canadian regional development. *Harvard.*

Arthur Hillabold, B.A. Oregon 1944; M.A. Stanford 1950. Alliance for Progress. *Stanford.*

Shinsuke Horiuchi, B.A. Int'l Christian (Tokyo) 1958; M.A. Texas 1962. An application of regression method for the interindustry analysis of Texas. *Texas.*

Fenn C. Horton, B.A. Iowa 1950. The theory of investment of the firm in Latin America. *Claremont.*

Marvin R. Jackson, Jr., B.S. Colorado 1954; M.A. 1960. Soviet organization for innovation and technological change. *California (Berkeley).*

James R. Jeffers, B.A. Iowa 1960. An inquiry into the relative contributions of the New Orleans financial system toward southern economic development. *Tulane.*

Mohammad S. Kalla, Lic. en Droit, Syrian 1954. The role of foreign trade in the economic development of Syria, 1920-1960. *American.*

Rita Kapoor, B.A. Punjab 1953; M.A. Stanford 1961. Financing of economic plans in underdeveloped countries with special reference to India. *Illinois.*

Yaqub N. Karkar, B.A. California (Berkeley) 1950; M.A. 1954. Railway development in the Ottoman empire, 1856-1914: an economic interpretation. *Indiana.*

Hyong C. Kim, B.A. Seoul Nat'l. 1955; M.A. San Diego 1960. Factors contributing to the Japanese growth: 1953-1962. *Oregon.*

Richard T. F. King, B.A. Cambridge 1961; M.S. California (Berkeley) 1962. The regional economic development of Mexico: a case study of the Tepalcatepec River Basin. *California (Berkeley).*

Jan Kmenta, B. Econ. Sydney (Australia). Economic effects of post-war immigration in Australia. *Stanford.*

Aubrey Kosson, B.S. New York 1941, M.B.A. 1954. Economic effects of the expiration of tax exemption in Puerto Rico. *New York.*

Thippalessery N. Krishnan, B.A. Travancore 1955; M. Litt. Madras 1955. Price behavior in India—a study of price relationships in a developing economy. *Mass. Inst. Technology.*

Ricardo Lagos, B.A. Chile 1955; M.A. Duke 1963. Economic system of Chile. *Duke.*

Som Liem, B.A. Indonesia 1958; M.A. Harvard 1962. Flexibility of development planning and world price movements. *Harvard.*

Norman N. Lithwick, B.A. Western Ontario 1960; M.A. Harvard 1962. Economic growth in Canada. *Harvard.*

Neal B. Long, B.A. Indiana 1959; M.A. North Carolina 1960. An input-output comparison of the U.S. and Soviet industrial structure. *Indiana.*

Milton D. Lower, B.A. Texas 1954; M.A. 1960. An application and testing of the institutionalist theory of development in Chile. *Texas.*

Bob A. McIlwain, B.S. Oklahoma A & M 1943; M.S. 1949. An investigation of resource combinations leading to intrastate differentials in agricultural income in selected southern U.S. areas. *American.*

M. C. Madhavan, B.A. Annamalai 1955; M.A. 1958. The economic development of Madras and Orissa, 1951-1961: a study in comparative economic growth. *Wisconsin.*

Abdul M. Malik, B.S.C. Ohio 1958; M.A. American 1961. Inflation and monetary policy in a developing economy: the case of Turkey, 1950-1960. *American.*

Sami K. Mohamed, B.C. Cairo 1948; M.A. Chicago 1959. United Arab Republic's balance of payments, commercial policy and economic development, 1952-1962. *Ohio State.*

Norman W. Mosher, B.A. Maine 1943; M.A. 1947; M.A. Fletcher School 1948. Methods of financing capital formation in Ghana. *Fletcher School.*

Marnie W. Mueller, B.A. Wellesley 1959; M.A. Yale 1961. Inflation in Mexico: structural or monetary? *Yale.*

Robert K. Paus, B.S. Sophia 1955. Role of small business in the expanding Japanese economy. *Columbia.*

Andrew J. Petro, B.S. De Paul 1953; M.S. 1954. Postwar regional urban development: occupational distribution, wage levels and growth. *Michigan State.*

Francois Poulin, B.Sc. Laval 1960; M.A. 1961. Regional planning with special reference to Valley of Comayagua (Honduras). *Texas.*

Robert W. Raynsford, Jr., B.A. Williams 1957. Problems of income distribution in post-war French economic development. *Harvard.*

Giovanna Ricoveri, Laurea, Pisa 1955. Southern Italy and the peculiar pattern of Italian economic development (1861-1960). *Columbia.*

Philip P. Robbins, M.S. Columbia 1954. An economic era: the Tarbell Papers and the history of the Standard Oil Company. *Pittsburgh.*

Gilbert L. Rutman, B.A. Boston 1956; M.A. 1961. The role of the Tanganyika Government in economic development. *Duke.*

George Sadler, B.S. Columbia 1952; M.A. 1954. Trends in Mexican economic policy in the twentieth century. *Texas.*

Emile Salameh, B.A. American (Beirut) 1954. Debt and economic growth in Lebanon: a study of Lebanese financial institutions in their relation to economic growth. *New York.*

Emil Salim, Drs. Sardjana, Indonesia; M.A. California (Berkeley) 1962. Economic planning in underdeveloped countries. *California (Berkeley).*

Franklin Sherwood, B.A. Connecticut 1958; M.A. 1959. Problems of Guatemalan economic development. *Illinois.*

Pritam Singh, B.A. Panjab 1937; M.A. 1939. Types of entrepreneurship in Indian economic development. *Michigan.*

Joseph Skehan, B.A. Syracuse 1947. Crisis of British socialism in the mid 20th Century. *Georgetown.*

Chinnawoot Soonthornsima, B.S. Chulachomklao 1958; M.P.A. Thammasat 1959; M.A.

Indiana 1960. A macroeconomic model for economic development of Thailand. *Michigan.*

Joseph J. Stern, B.A. Queens 1960; M.A. Harvard 1962. Commodity stabilization agreements and their use in economic development. *Harvard.*

Donald L. Sternitzke, B.S.C. Iowa 1957; M.A. 1959. The raw materials problem in Latin America. *Iowa.*

Lionel G. Stoleru, Ecole Polytechnique (Paris) 1958; Ecole des Mines (Paris) 1961. A quantitative model of growth of Algerian economy. *Stanford.*

Sang C. Suh, B.A. Clark 1958; M.A. 1959. Structure of the Korean economy since 1945. *Harvard.*

Adrian W. Throop, B.A. Amherst 1959; M.A. Stanford 1963. The role of foreign trade in Mexican economic development. *Stanford.*

Jogindar S. Uppal, B.A. Punjab 1950; M.A. 1952; M.A. Minnesota 1961. Measurement of disguised underemployment—Indian agriculture—a case study. *Minnesota.*

Edward Van Roy, B.B.A. Clark 1960; M.A. 1961. Economic development in Southeast Asia. *Texas.*

Evangelos A. Voloudakis, B.A. Athens School Econ. & Bus. 1953; M.A. Yale 1961. An empirical study of the private investment decision in a developing economy: Greece, a case study. *Yale.*

William P. Wadbrook, S.B. in F.S. Georgetown 1958; M.A. Fletcher School 1959; M.A.L.D. 1961. Banking, inflation and growth in Brazil, 1950-1960. *Fletcher School.*

Harrison G. Wehner, B.A. Michigan 1957; M.A. 1958. The cocoa marketing board and economic development in Ghana: a case study. *Michigan.*

Jerome C. Wells, B.A. Michigan 1958; M.A. Johns Hopkins 1961. An appraisal of agricultural investments in the 1962-1968 Nigerian development program. *Michigan.*

Elliott Wetzler, B.A. Brandeis 1959. A general equilibrium analysis of the interrelationship between economic development and international trade. *Johns Hopkins.*

William T. White, B.S. Arizona 1941; M.S. Columbia 1947. Money and banking in Yugoslavia: a case study of the relationship between money and frictions in economic planning. *Georgetown.*

Jean Wilburn, B.A. California (Berkeley) 1944; M.A. Columbia 1963. New York state and the Second U.S. Bank. *Columbia.*

James F. Willis, B.A. Texas 1951; M.A. 1952. Tax reform and economic development in Mexico. *Southern Methodist.*

Man H. You, B.A. Michigan State 1956; M.A. 1958. Long term prospects for natural rubber in Southeast Asia. *Oregon.*

Robert B. Zevin, B.A. Antioch 1959; M.S. Ohio State 1960. The industrial development of New England: 1800-1860. *Harvard.*

Oleg Zinman, M.B.A. Xavier 1959. Interaction of preference and opportunity functions in long range economic development. *Cincinnati.*

Statistical Methods; Econometrics; Social Accounting; Input-Output

Degrees Conferred

John E. Brandl, Ph.D. Harvard 1963. The early development of econometric method.

Charles B. Millham, Ph.D. Iowa (Ames) 1962. Stochastic linear programming.

David Neft, Ph.D. Columbia 1963. Statistical methods and analysis for areal distributions.

John C. Sawhill, Ph.D. New York 1963. The problem of error in cost-of-living index numbers.

Thomas W. Sharkey, Ph.D. Indiana 1963. The process capability study and the graphic method of multiple regression analysis.

LESTER D. TAYLOR, Ph.D. Harvard 1963. The principal-components-instrumental-variable approach to the estimation of systems of simultaneous equations.

JAMES R. WILLIAMS, Ph.D. Minnesota 1962. An estimation of the parameters in the regression of observed consumption on observed income when both are assumed to include random variable.

Theses in Preparation

TAKESHI AMEMIYA, B.A. Int'l Christian (Toyko) 1958; M.A. American 1961. Bayesian methods in econometrics. *Johns Hopkins.*

CHARLES BARTFELD, M.B.A. Columbia 1961. Systems control in organizations by analog method. *Columbia.*

ANTHONY S. CAMPAGNA, B.S. Rutgers 1960; M.A. 1962. An econometric study of expectation elasticities affecting the investment function. *Rutgers.*

STEPHEN CAMPBELL, B.S. Utah 1957, M.S. 1959. Measuring the effects of selected weighted-moving averages upon economic time series: an experimental study. *Columbia.*

KONG CHU, B. A. Nat'l Taiwan 1952; M.A. California (Los Angeles) 1960. Computer simulation of certain stochastic relationships in microeconomic systems. *Tulane.*

JOHN G. CRAGG, B.A. McGill 1958; B.A. Cambridge 1960. Monte Carlo experiments on techniques for estimating the coefficients of simultaneous linear equations. *Princeton.*

CHRISTIAN G. EHEMANN, B.A. Wooster 1959. Effects of intermodal competition on R: R ratio: statistical analysis. *Northwestern.*

WILFORD L'ESPERANCE, B.A. Columbia 1951; M.A. 1952. The predictive ability of various estimators. *Michigan.*

HENRY PESKIN, B.A. Wesleyan 1959. A proposal to investigate the new Norwegian budget model. *Princeton.*

FREDERIC RAINES, B. A. Harvard 1959. A production smoothing approach to cyclical fluctuations in the two digit industrial sectors. *Wisconsin.*

JUGAL K. SHARMA, B.S. Delhi 1954; M.S. California (Berkeley) 1963. The application of Monte Carlo methods to the evaluation of small sample properties of 3-stage least squares procedure. *California (Berkeley).*

GEORGE J. VIKSNINS, B.A. Temple 1959; M.A. Pennsylvania 1960. Seasonal movements in financial series: a methodological analysis. *Georgetown.*

Economic Systems; Planning and Reform; Cooperation

Degrees Conferred

ABDEL R. ABDEL-MEGUID, Ph.D. Texas 1963. Economic management in a planned society.

NAKE M. KAMRANY, Ph.D. Southern California 1962. The first five year plan of Afghanistan (1956-1962): an economic evaluation.

ELIAS H. TUMA, Ph.D. California (Berkeley) 1962. Economics, politics, and land tenure reform: a comparative study.

Theses in Preparation

ROBERT J. CHENEY, B.A. St. Louis 1950; Ph.L. 1951; M.A. 1953. Goetz A. Briefs on the history and development of modern capitalism: a critical examination. *Georgetown.*

MICHAEL J. LAVELLE, B.A. Loyola (Chicago) 1958; Ph.L. 1960. Soviet image of the U.S. economy, 1953-1963. *Boston College.*

CARL B. TURNER, B.A. Duke 1950; M.A. 1953. An analysis of Soviet views of selected Western economists, 1950-1960. *Duke.*

Business Fluctuations and Forecasting

Degrees Conferred

William Alberts, Ph.D. Chicago 1961. Business cycles, residential construction cycles and the mortgage market.

Richard M. Duvall, Ph.D. North Carolina 1963. The measurement of seasonal variation in theory and practice.

Claude Hillinger, Ph.D. Chicago 1963. A theory of the inventory cycle with application to quarterly data for the U.S. economy 1953-1961.

Chaiho Kim, Ph.D. Columbia 1963. Patterns of fluctuations in common stock offerings.

Robert N. Schweitzer, Ph. D. California (Berkeley) 1963. Price behavior during contractions.

Theses in Preparation

Joel I. Brest, B.A. Brown 1960. An econometric model of investment fluctuations in the determination of business cycle activity. *Brown.*

Richard L. Carson, B.A. Indiana 1958; M.A. Minnesota 1961. A fresh approach to forecasting investment. *Indiana.*

Michael K. Evans, B.A. Brown 1960. A postwar quarterly model of the U.S. economy, 1957-1960. *Brown.*

Charles Friedman, B.A. Hunter 1956; M.A. Columbia 1957. Effects on economic fluctuations of variations in savings placed with intermediaries—U.S., England, France, Germany. *Columbia.*

Harvey A. Krow, M.A. Pittsburgh 1943. The technical approach to the stock market forecasting—a critique. *Pittsburgh.*

LeRay R. McGee, B.S. Citadel 1959; M.A. South Carolina 1962. The cyclical behavior of income and employment in the Southeast: 1945-1961. *Tulane.*

Lon Polk, B.A. San Jose State 1954. The full employment surplus and economic recovery. *Michigan.*

Money, Credit and Banking; Monetary Policy; Consumer Finance and Mortgage Credit

Degrees Conferred

Anatol Balbach, Ph.D. California (Los Angeles) 1963. The demand for money: Clark Warburton's hypotheses reconsidered.

Harry Bartell, Ph.D. Columbia 1963. Unions and pension funds.

Arthur Benavie, Ph.D. Michigan 1963. The impact on the strength of monetary controls of asset shifts involving intermediary claims.

Robert L. Bennett, Ph.D. Texas 1963. Financial intermediaries in Mexican economic development.

George Benston, Ph.D. Chicago 1963. A statistical study of the cost of banking operations.

Jerry T. Bigosinski, Ph.D. New York 1963. The Swiss money market.

William C. Brainard, Ph.D. Yale 1963. Financial intermediaries and a theory of monetary control.

Arthur L. Broida, Ph.D. Chicago 1963. Liquidity as a variable in monetary analysis.

Nae H. Chung, Ph.D. Michigan State 1963. The role of the central bank and inflation in Korea from 1945-1960.

Pham Chung, Ph.D. Pennsylvania 1962. The development of financial institutions and policies in Vietnam.

Jack W. Cox, Ph.D. Oregon 1963. The demand for bank loans by U.S. manufacturing corporations: 1948-1961.

John R. Cox, Ph.D. Southern California 1962. Institutional mortgage lending in the Los Angeles metropolitan area, 1953-54 and 1957-58.

Jose M. Dagnino-Pastore, Ph.D. Harvard 1963. Income, money and financial analysis, Argentina 1935-1960.

William G. Dewald, Ph.D. Minnesota 1962. Monetary control and the distribution of money.

Holger Engberg, Ph.D. Columbia 1963. Mixed banking and economic growth in Germany, 1850-1931.

Edgar Feige, Ph.D. Chicago 1963. The demand for liquid assets: a temporal cross-section analysis.

Charles Frankenhoff, Ph.D. Georgetown 1963. The ineffectiveness of flexible monetary policy in the United States.

Charles A. E. Goodhart, Ph.D. Harvard 1963. National banking system reactions to seasonal variations 1900-1913.

Robert J. Lawrence, Ph.D. Michigan 1963. The regional impact of monetary policy: a case study.

Levern F. Graves, Ph.D. California (Berkeley) 1963. The effects of consumer credit restraint on the distribution of income.

Henry P. Gray, Ph.D. California (Berkeley) 1963. Commercial bank profitability and the effectiveness of monetary policy.

Thomas E. Holland, Ph.D. Duke 1963. Forecasting interest rate movements: time series analyses and functional relationships.

Steven J. Hunter, Ph.D. Virginia 1963. The rationale and effect of the bills-only policy.

Walter L. Johnson, Ph.D. Duke 1963. Comparability of structures of interest rates.

Chandra K. Johri, Ph.D. Michigan 1963. Monetary policy of the Reserve Bank of India and its effect on the operation of the Indian banking system, 1956-61.

Richard A. Krachenberg, Ph.D. Michigan 1963. The marketing of services, with particular reference to commerical bank services.

Mary-Elissa McGuire, Ph.D. Fordham 1963. Bank stocks as investment 1951-1957.

Bruce T. McKim, Ph.D. Iowa 1962. Income velocity and monetary policies in the United States, 1951-1960.

David Meiselman, Ph.D. Chicago 1961. The term structure of interest rates.

George Morrison, Ph.D. Chicago 1963. Liquidity preferences of commercial banks.

Leigh C. Rhett, Ph.D. New York 1962. Convertibility and the reserve currency system.

Barrie Richardson, Ph.D. Indiana 1962. State regulations of retail installment credit—revolving credit sales.

E. Sussman, Ph.D. McGill 1963. The role of mortgage banking in the Canadian economy.

Ronald L. Teigen, Ph.D. Mass. Inst. Technology 1962. Demand and supply for money in the U.S.: some structural estimates.

C. F. J. Tom, Ph.D. Chicago 1963. Monetary problems of an entrepot: the Hong Kong experience.

Ali Wardhana, Ph.D. California (Berkeley) 1962. Monetary problems of an underdeveloped economy: with special reference to Indonesia.

William E. Whitesell, Ph.D. Texas 1963. The Federal Reserve System's "bills only" policy.

Theses in Preparation

John P. Barrados, B. Comm. McGill 1958; M.A. 1960. Monetary policy and growth and stability of the Canadian economy: 1956-1960. *Columbia.*

Peter Berman, B.A. Cornell 1960. The role of the federal funds market in commercial bank portfolio adjustment. *Johns Hopkins.*

David E. Bond, B.A. Dartmouth, 1960; M.A. Yale 1961. The effects of a change in the ceiling rates on deposits at commercial banks. *Yale.*

Charles H. Bubeck, B.A. Toronto 1961, M.A. 1962. The nature and control of monetary velocity. *Toronto.*

Albert H. Cox, Jr., B.B.A. Texas 1954, M.B.A. 1956. An economic analysis of the regulation of interest rates on deposits of commercial banks in the United States. *Michigan.*

Adolfo C. Diz, Grad. Status, Buenos Aires 1951; M.A. Chicago 1957. A monetary history of Argentina, 1935-1960. *Chicago.*

Franklin R. Edwards, B.A. Bucknell 1958; M.A. 1960. Competition in commercial banking. *Harvard.*

David C. Ewert, B.S. Iowa (Ames) 1959; M.S. Minnesota 1960. The capital adequacy question in commercial banks. *Stanford.*

Robert L. Frnka, B.S. Southern Methodist 1957. An empirical study of the non-bank financial intermediaries in relation to monetary policy. *Southern Methodist.*

David C. Gogerty, B.A. Stanford 1956, M.A. 1962. An international comparison of financial intermediation—a study of the differences in forms and levels of financial intermediation in countries differing in per capita incomes. *Stanford.*

William Griggs, B.S. Oklahoma State 1956; M.A. 1957. Monetary policy during the 1960 recession. *Ohio State.*

Leroy J. Grossman, B.S. Washington (St. Louis) 1954. An evaluation of the consistency of the defensive and dynamic objectives of Federal Reserve policy. *Vanderbilt.*

Katharine H. Hsiao, B.A. Kunming 1943; M.A. Columbia 1959. Role of money and monetary policy in Communist China. *Columbia.*

Nguyen T. Hung, Law Dipl. Saigon 1956. Money and banking in Viet-Nam since independence. *Virginia.*

John A. Hynes, B.A. Johns Hopkins 1958. The demand for money in Latin America. *Chicago.*

David H. Kain, B.A. Haverford 1959. The effects of branch banking laws on the allocation of credit. *Michigan.*

Michael Kawaja, B.S. McGill 1960. Competition in the New York State loan industry. *Columbia.*

Harlan N. Kilmer, B.A. Kansas 1949; M.B.A. Northwestern 1958. Incorporation of mutual savings and loan associations. *Northwestern.*

Nicos Koubis, Dipl. Athens School Econ. & Bus. 1953; M.A. Yale 1960. Factors affecting the demand for money in Greece. *Yale.*

Ralph Laws, B.A. Washington College 1957. Competition and the theory of commercial banking. *Duke.*

Burton G. Malkiel, B.A. Harvard 1953; M.B.A. 1955. Problems in the structure of financial markets. *Princeton.*

George Mowbray (earlier degree not supplied). Determinants of the demand for Canadian coins. *Princeton.*

Mohammed I. Nadiri, B.S. Nebraska 1958; M.A. California (Berkeley) 1960. U.S. balance of payment and monetary policy, 1958-1962. *California (Berkeley).*

Eugene Pauley, B.A. Kansas State (Fort Hays) 1959. Term lending by commercial banks: some patterns and implications. *Ohio State.*

Larry Price, B.S. E. E. Arkansas 1959; M.B.A. 1960. Arkansas commercial bank lending policies and industrial expansion. *Arkansas.*

Richard H. Puckett, B.A. Colorado 1958; M.S. Oklahoma State 1960. On the effectiveness of monetary policy. *Maryland.*

David K. Sheppard, B.A. Harvard 1958. 100 years of liquidity preference: a study in the development of financial institutions. *Harvard.*

A. Gary Shilling, B.A. Amherst 1960; M.A. Stanford 1962. Patterns of debt issues by ultimate borrowers in the United States in the post-World War II era. *Stanford.*

Donald P. Tucker, B.A. Swarthmore 1960. Internal credit availability and credit rationing, and their influence on short-run fluctuations in business investment. *Mass. Inst. Technology.*

Richard S. Wallace, B.A. Wofford 1956. Banking performance under differing market structures: a case study of three small cities. *Virginia.*

Edward L. Whalen, B.A. Indiana 1958. A statistical study of the inventory approach to the transactions and precautionary demand for cash. *Princeton.*

Jack W. Wilson, B.B.A. Oklahoma 1954; M.A. 1959. Determinants of the yield of U.S. Government treasury bills, 1952-1962: a theoretical and empirical analysis. *Oklahoma.*

Ronald F. Wippern, B.S. Colorado 1955; M.B.A. 1961. Cost behavior in savings and loan associations: the implications for public policy and managerial decisions. *Stanford.*

William Wrean, B.A. Yale 1957; M.S. Wisconsin 1962. A short term forecast model of the level of commercial bank loans. *Wisconsin.*

Public Finance; Fiscal Policy

Degrees Conferred

Robert E. Berney, Ph.D. Wisconsin 1963. Debt management: a comparison of Canadian and United States experience.

William C. Birdsall, Ph.D. Johns Hopkins 1963. Public finance allocation decisions and the preferences of citizens: some theoretical and empirical considerations.

Benjamin Bridges, Jr., Ph.D. Johns Hopkins 1963. Treatment of state and local nonbusiness taxes under federal individual income tax: tax deductions and tax credits.

Donald J. Curran, Ph.D. Wisconsin 1963. The financial evolution of the Milwaukee metropolitan area.

Stanley L. Engerman, Ph.D. Johns Hopkins 1963. Postwar regional cycles and their implications for fiscal policy.

Norbert L. Enrick, Ph.D. Virginia 1963. An investigation of some aspects of income tax consciousness.

John R. Ervin, Ph.D. Ohio State 1962. An anlysis of severance taxation including possible application to the Ohio tax system.

Moredecai Feinberg, Ph.D. Columbia 1963. The implications of core-city decline for fiscal structure of the core-city.

Fred Hanga, Jr., Ph.D. Syracuse 1963. Public school capital formation and monetary policy: an empirical study covering the period 1948 to 1960.

Ma T. Hlyne, Ph.D. New York 1963. The accounting and financial systems of the Tennessee Valley Authority.

Susan J. Lepper, Ph.D. Yale 1963. Effects of alternative tax structures on individuals' holdings of financial assets.

Robert M. Lovejoy, Ph.D. Michigan 1963. Alternative methods of financing the Jamaican budget.

Will Lyons, Ph.D. Harvard 1963. The power of economic policy: interactions between policy action variables and changing structural parameters.

Natalie J. Marshall, Ph.D. Columbia 1963. Issues in British public finance theory from Mill to Marshall.

Joan E. A. O'Bannon, Ph.D. Virginia 1963. A comparison of federal shared revenues and local potential tax revenues on national forest lands and national wildlife refuges in the State of Louisiana.

Peggy Richman, Ph.D. Johns Hopkins 1963. Economic aspects of the taxation of foreign-earned business income.

Jack L. Robinson, Ph.D. Ohio State 1962. An inquiry into the feasibility of and estimated revenue productivity of individual and corporate income taxes in Ohio.

William O. Shropshire, Ph.D. Duke 1963. Interest rate changes and capital outlay decisions of North Carolina muncipalities, 1955-1958.

Nat Simons, Jr., Ph.D. Ohio State 1962. Financing Ohio's highways.

Anthony H. Stocks, Ph.D. New York (Buffalo) 1963. Some problems in the finance and organization of metropolitan government with particular reference to the Toronto Federation.

William C. Stubblebine, Ph.D. Virginia 1963. The social imbalance hypothesis.

Theses in Preparation

Robert F. Adams, B.A. Oberlin 1958. Determinants of local government expenditures. *Michigan.*

Gordon Bell, B.B.A. Miami 1948. A comparison of local government financial administration in Georgia and North Carolina. *Florida.*

Ronald W. Brady, B.S. Ohio State 1960; M.A. 1962. An econometric model of the tax system of Ohio. *Ohio State.*

John R. Cooper, B.S. Clemson 1955; M.S. 1957. Institutional variables affecting the outcome of school bond referenda. *Virginia.*

Josephine M. Corrigan, B.A. St. Joseph's 1938; M.A. Columbia 1939. The national tax system of the Republic of Ireland. *New York.*

Kenneth L. Deavers, B.S. California (Los Angeles) 1959; M.A. Chicago 1961. Tax effects, expected utility axioms and efficient portfolio selection. *Chicago.*

Eberhard W. Dinkelacker, B.B.A. City (New York) 1959. An empirical investment function for testing fiscal incentives to investment. *Johns Hopkins.*

Ann F. Friedlaender, B.A. Radcliffe 1960. The impact of the interstate highway system upon the United States economy: a theoretical and empirical analysis. *Mass. Inst. Technology.*

Sally Hey, B.A. Mt. Holyoke 1960. The role of the urban property tax in economic development in Colombia. *Johns Hopkins.*

Kern O. Kymn, B.A. Wilmington 1955. Income taxes and the demand for currency relative to deposits in Japan. *Chicago.*

David E. W. Laidler, B.Sc. London 1959; M.A. Syracuse 1960. The income tax incentive to owner occupation of housing. *Chicago.*

Daniel W. Lynch, B.A. Notre Dame 1950; M.A. Fordham 1956. The development of state and local debt in Kentucky since 1890. *Kentucky.*

Robert W. Rafuse, B.A. Harpur. State and local fiscal behavior over the cycle. *Princeton.*

Cadwell L. Ray, B.A. Texas A & M 1959; M.S. 1961. The impact of the Texas fiscal system on the distribution of income, wealth, and welfare. *Texas.*

Stephen A. Resnick, B.S. Econ. Pennsylvania 1960. A study of the effects of stabilization policies by autonomous fiscal bodies in an interrelated economic system: an econometric study of the common market. *Mass. Inst. Technology.*

Leonard G. Rosenberg, B.A. Brooklyn 1948; M.A. Chicago 1951. Taxation of total income from capital by non-financial industry sector, 1953-59. *Chicago.*

Lars G. Sandberg, B.A. Harvard 1961. Swedish economic policy in theory and practice, 1950-1960. *Harvard.*

Rudolf Thunberg, B.A. Duke 1960. Patterns of fiscal centralization in state-local goverment relations. *Princeton.*

EDWARD WIEGNER, B.B.A. Wisconsin 1961. An empirical study of income tax averaging. *Wisconsin.*

RICHARD C. WILLIAMS, B.A. Oregon 1960. The tax structure and private investment in Colombia. *Oregon.*

International Economics

Degrees Conferred

ELAINE B. ANDREWS, Ph.D. Fletcher School 1963. International and foreign bond flotations in London and the United States, 1860-1960.

DONALD V. T. BEAR, Ph.D. Stanford 1962. The foreign trade multiplier.

GARNETT F. BEAZLEY, JR., Ph.D. Pittsburgh 1963. A study of the relationship of United States taxation to United States direct private foreign investment.

EVANGELOS A. CALAMITSIS, Ph.D. Harvard 1963. International trade and economic development. Some problems in the formulation of commercial policy in underdeveloped countries.

RONALD J. CLARK, Ph.D. Indiana 1963. Latin American economic relations with the Soviet bloc.

BENJAMIN J. COHEN, Ph.D. Columbia 1963. Customs unions and international liquidity.

ATTIAT A. FARAG, Ph.D. Michigan 1963. Economic integration—a theoretical and empirical study.

ROBERT A. FLAMMANG, Ph.D. Iowa 1962. The common market movement in Latin America.

E. BRUCE FREDRIKSON, Ph.D. Columbia 1963. Reporting international operations.

HERBERT G. GRUBEL, Ph.D. Yale 1963. A multicountry model of forward exchange: theory, policy, and empirical evidence.

CURTIS E. HARVEY, Ph.D. Southern California 1963. A case study of the adaption of a small national economy's industry to international competition: Austria.

LOTHAR H. HUEHNE, Ph.D. Wisconsin 1963. Financing economic development through national and international organizations.

MANFREDO MANENTE, Ph.D. New York 1963. The treaty establishing the European economic community and political integration of Europe.

ROBERT MILLER, Ph.D. Bryn Mawr 1963. International monetary plans.

GORDON R. MUNRO, Ph.D. Harvard 1963. The British exchange control system 1945-52: a study of exchange control in inflationary times.

JOHN R. NICHOLS, Ph.D. Ohio State 1962. United States foreign dollar bonds and private investment; the 1920's and the 1950's.

JAMES E. PRICE, Ph.D. Mass. Inst. Technology 1963. Financing of international organizations by membership contributions.

MARGARET M. REILLY, Ph.D. Fordham 1963. Alternate views of international monetary cooperation 1956-1960.

RICHARD REIMER, Ph.D. Michigan State 1962. The U. S. demand for imports of materials: 1923-60.

DONALD R. SHERK, Ph.D. Iowa 1963. Exchange and trade controls and implications thereof for underdeveloped economies.

RAYNARD M. SOMMERFELD, Ph.D. Iowa 1963. Tax reform and the Alliance for Progress.

RAYMOND L. STAEPELAERE, Ph.D. Oregon 1963. Factors influencing changes in the shares of industrial countries in world export markets for manufacturers, 1953-1960.

FRANK G. STEINDL, Ph.D. Iowa 1963. The United States balance of payments, 1953-60.

Theses in Preparation

DORRA ALWAN, B.A. American (Egypt) 1956; M.A. Bryn Mawr 1959. Investment criteria in the international bank survey. *Wisconsin.*

Kenneth L. Bauge, B.S. Iowa (Ames) 1955; M.S. 1958. Voluntary export restrictions as a commercial policy. *Michigan State.*

Claude Carpenter, B.S. Western Carolina 1940; M.B.A. Chicago 1951. Economic integration in Western Europe since World War II. *Missouri.*

David Devlin, B.A. Lehigh 1956; B.S. 1957. The adjustable exchange-rate peg and free trade. *Columbia.*

Bruce F. Duncombe, B.A. Amherst 1959. Interstate and interregional multipliers. *Minnesota.*

Donald Eilenstine, B. A. Ottawa 1959; M.A. Kansas 1960. International trade under the Articles of Confederation. *Kansas.*

Osman A. El-Kholei, B.S. Alexandria 1953; M.A. 1958. An econometric analysis of the of the impact of the European Common Market on the economy of the United Arab Republic *California (Berkeley).*

Clarence Elliott, B.S. Arkansas A & M 1958; M.B.A. Arkansas 1960. An inquiry into the problems involved in the consolidation of foreign operations with particular emphasis on exchange rates. *Arkansas.*

Bernard Friedman, B.A. Michigan 1937; M.A. 1939; M.A. Harvard 1944. International trade in cotton and United States policy. *Columbia.*

Richard J. Gelson, B.A. Notre Dame 1960. Discipline in the balance of payment in the light of American experiences in 1893, 1931, 1958-61. *Virginia.*

Erika Georges, B.A. Los Angeles State 1958; M.A. California (Los Angeles) 1961. The European coal and steel community—effects on trade and investments. *California (Los Angeles).*

Paul R. Gormly, B.A. North Texas 1959. An analysis of the cyclical behavior of long-term international capital flows. *Southern Methodist.*

Benson H. Hart, B.A. Haverford 1958; M.A. Ohio State 1961. The Bank of England and the balance of payments of the United Kingdom, 1956-61. *Ohio State.*

James M. Holmes, B.A. Wabash 1959. What determines the relationship between the U.S. price level and the price levels of other countries in the world. *Chicago.*

Kyung M. Huh, B.A. Michigan 1957; M.A. 1959. The development and prospects of Japan's trade in Asia. *Michigan.*

Frank W. Jones, B.A. Iowa 1950; M.A. 1952. Forward exchange operations as an international monetary device. *Iowa.*

Cheul Kang, A.A. Reinhardt 1956; B.A. King 1958; M.S. Wisconsin 1960. India's export promotion program. *Wisconsin.*

John R. Karlik, B.A. Middlebury 1960. An economic analysis of international licensing agreements. *Columbia.*

Mahn J. Kim, B.A. Denver 1957; M.A. Missouri 1961. An analysis of the market for Euro-dollars. *Missouri.*

Robert E. Krainer, B.S. Wisconsin 1957; M.B.A. Michigan 1959. Long-term foreign investment and the American balance of payments 1950-1960. A theoretical and empirical analysis. *Michigan.*

James W. Land, B.A. Southern Methodist 1957, M.A. 1960. Factor-price equalization: an extension of a theorem in the pure theory of international trade. *Princeton.*

Jerome R. La Pittus, B.A. California (Berkeley) 1960; M.A. Yale 1961. The demand for official reserves to finance international trade under a system of fixed exchange rates. *Yale.*

Carol A. Lawson, B.A. California (Berkeley) 1958. The contribution of international capital movements to capital formation and growth. *California (Berkeley).*

Alan Lazar, B.S. California (Berkeley) 1957; M.B.A. 1961. U.S. direct foreign investment: a study of its impact on U.S. exports. *Columbia.*

Kie B. Lee, B.A. California (Berkeley) 1958; M.S. Florida State 1960. Shrinking foreign trade dependency in growing economies. *Virginia.*

LAURENCE E. LYNN, B.A. California (Berkeley) 1959. Tied aid and the fungibility of foreign exchange assets: their effect on the U.S. balance of payments. *Yale.*

SHAH MAHMOUD, B.B.A. Toledo 1960; M.B.A. 1960. The balance of payments problem in underdeveloped nations: a case study of Afghanistan. *Columbia.*

STANLEY MALCUIT, M.L. Pittsburgh 1957. The import quota as a trade control method. *Pittsburgh.*

CLAUDE MASSON, B.A. Laval 1960; M.A. 1960. The importance of the foreign sector in the Canadian economy, an analytical study of the period 1947-1961. *Harvard.*

VICTOR U. MOK, Dipl. Chun Chi 1959; M.A. Michigan State 1960. The foreign trade of Communist China 1949-1960. *Michigan State.*

ROBERT J. MURPHY, B.A. Boston College 1938; M.A. 1951. Employment coefficients of output in escape clause industries. *Fordham.*

MARION M. NOBEL, B.A. Michigan 1942; M.A. Iowa 1957. The United State balance of payments, 1953-1961. *Michigan.*

LAWRENCE H. OFFICER, B.A. McGill 1960; M.A. Harvard 1962. Canada's fluctuating exchange rate: 1950-1962. *Harvard.*

OWEN M. QUATTLEBAUM, B.A. Georgia 1957; M.A. Fletcher School 1958; M.A.L.D. 1959. International trade among centrally planned and market economies within the framework of a world trade organization. *Fletcher School.*

JEAN Y. RIVARD, B.S. Montreal 1957; M.A. 1960. The restoration of the postwar convertibility of the French franc. *Michigan.*

RICHARD L. RUTH, B.A. Wichita 1958; M.S. Wisconsin 1960. The effects of land tenure on international trade: a case study—Mexico and Peru. *Wisconsin.*

ARTHUR I. STONEHILL, B.A. Yale 1953; M.B.A. Harvard 1957. Foreign private enterprises in Norway. *California (Berkeley).*

ALEXANDER K. SWOBODA, B.A. Yale 1960; M.A. 1961. The "Euro-dollar" market: theoretical consideration and policy implications. *Yale.*

JAMES WEAVER, B.A. Arkansas 1955. A new approach to foreign aid: with special reference to the International Development Association. *Oklahoma.*

RICHARD C. WEBB, B.A. St. Andrews (Scotland) 1960. Innovation as a trade-creating factor. *Harvard.*

IRVIN WEINTRAUB, B.A. Johns Hopkins 1950; M.S. Columbia 1951. Multiple exchange rates vs. the unitary rate in a developing country: the case of Israel. *Maryland.*

NANCY H. WILSON, B.A. Vassar 1959; M.A. California (Berkeley) 1963. The integration of international trade into economic planning models. *California (Berkeley).*

KEVIN WINCH, (earlier degree not supplied). Stabilization policies in Argentina and Chile with reference to the Alliance for Progress. *Wisconsin.*

YEONG H. YEH, B.A. Taiwan 1955; M.A. Minnesota 1959. An evaluation of a possible Common Market for the ECAFE region. *Minnesota.*

Business Finance; Investment and Security Markets; Insurance

Degrees Conferred

DAVID ABNER, III, D.B.A. Indiana 1963. A history of negro legal reserve life insurance companies.

JUAN B. APONTE-VAZQUEZ, Ph.D. Pennsylvania 1962. An analysis of government retirement systems for the Commonwealth of Puerto Rico.

ARTHUR J. BONESS, Ph.D. Chicago 1962. A theory and measurement of stock-option value.

GORDON F. BOREHAM, Ph.D. Columbia 1962. Significant changes in the financial structure of Canada.

DAVID A. BOWERS, Ph.D. Southern Methodist 1963. Investor preference and the structure of interest rates in the U.S. Government securities market, 1953-1961.

Eugene F. Brigham, Ph.D. California (Berkeley) 1962. The relative position of various intermediaries in the residential mortgage market.

John C. Burton, Ph.D. Columbia 1963. The management of corporate liquid assets.

Donald O. Cameron, Ph.D. New York 1963. An analysis of direct investment abroad with special reference to the agricultural equipment industry.

Robert M. Crowe, Ph.D. Pennsylvania 1963. Rate making for automobile physical damage insurance.

Alan D. Entine, Ph.D. Columbia 1963. The behavior of U.S. Government securities and cash held by selected financial intermediaries: 1952-1961.

Alfred J. Gobar, Ph.D. Southern California 1963. Factors affecting the growth of small business investment companies on the Pacific Coast.

Mark Hanna, Ph.D. North Carolina 1963. A study of inadequate depreciation for 1956 due to inflation based upon external reports of fifteen corporations selected from the Dow Jones industrial average.

Paul L. Harmon, Ph.D. California (Los Angeles) 1963. An analysis of the investment policies of the small business investment companies.

Sajjad A. Hashmi, Ph.D. Pennsylvania 1962. Social security in Pakistan.

James N. Holtz, Ph.D. Michigan 1963. The financial concept of working capital.

Henry R. Jaenicke, Ph.D. Pennsylvania 1963. The criteria for distinguishing purchases from poolings of interest and their present day applicability.

Oldrich Jicha, Ph.D. Michigan 1963. A conceptual approach to securities valuation and analysis.

Harry M. Johnson, Ph.D. Pennsylvania 1962. Major medical expense insurance.

Keith B. Johnson, Ph.D. Washington (St. Louis) 1963. An analysis of the permanent price change associated with common stock splits.

Eugene W. Lambert, Jr., Ph.D. Alabama 1963. An analysis of the effects of debt financing on investment value.

Nelson D. McClung, Ph.D. Yale 1963. Nonfinancial corporation flows of funds: an explanatory study of the possibilities for developing regression forecasters of nonfinancial corporation non-trade lending and borrowing.

William A. Nye, Ph.D. Pennsylvania 1962. The treatment and rating of substandard automobile liability risks.

Bobbie D. Owens, Ph.D. Pennsylvania 1962. Economic, environmental, and legal problems of international life insurance (with specific reference to American companies).

Santo J. Pullara, Ph.D. Syracuse 1962. Retirement plans: a study of small and large firms.

Edward Reighard, Ph.D. Stanford 1963. A study of the relationship of profit sharing programs and featherbedding practices.

Barnard Seligman, Ph.D. New York 1962. Indexing and financial contracts in the post World War II period.

Robert G. Smith, Ph.D. New York 1963. Determinants of investor judgment in the selecting of mutual funds.

John E. Stinton, D.B.A. Indiana 1962. A ratio analysis as a test of financial strength of United States property insurers.

Gert Von der Linde, Ph.D. California (Berkeley) 1963. Growth, functions, and financial administration of private pension plans.

Hsiu K. Wu, Ph.D. Pennsylvania 1963. Corporate insider trading, profitability, and stock price movement.

Theses in Preparation

Douglas V. Austin, B.A. Ohio Wesleyan 1959; M.A. Indiana 1961. The "causes" of proxy contests: an empirical study, 1956-60. *Indiana.*

NEVINS D. BAXTER, B.A. Columbia 1961. The commercial paper market. *Princeton.*

CHARLES R. BEATON, JR., B.A. Willamette 1960. An empirical study of the relation between the cost of capital and rate of return for the business firm. *Claremont.*

ALVIN B. BISCOE, B.A. Georgia 1952; M.B.A. Chicago 1956. Current liabilities as a permanent source of funds for industrial corporations. *Florida.*

LAWRENCE F. BRAUN, B.S. Northwestern 1951; M.B.A. 1958. Implications of problems of monetary stabilization in the United Kingdom in 1961 for regulation of flow of funds to British sales finance companies. *Northwestern.*

ROBERT S. CARLSON, B.S. Mass. Inst. Technology 1956; M.B.A. Stanford 1962. A model for valuation of convertible bonds and convertible preferred stocks. *Stanford.*

MANUEL H. CHAVEZ, B.A. Utah 1955. Medical malpractice liability insurance. *Pennsylvania.*

EUGENE DIULIO, B.S. Northwestern 1959. The saturation effect: a phenomenon of the capital market. *Columbia.*

SAUL FELDMAN, B.A. City (New York) 1941; M.S. City (New York) and Columbia 1958. An inquiry into the control aspects of company insurance programs. *Columbia.*

WILLIAM K. GHEE, B.S. Kentucky 1951; M.B.A. Syracuse 1956. Group creditors' insurance. *Pennsylvania.*

REYNOLDS GRIFFITH, B.A., Arkansas; M.B.A. (no year supplied). Valuation of life insurance stocks. *Texas.*

ALFRED HOFFLANDER, JR., B.S. Florida State 1958; M.S. 1959. The human life value. *Pennsylvania.*

STANLEY S. C. HUANG, B.A. St. John's (China); M.A. Illinois 1948; M.B.A. New York 1956. Common stock pricing and ownership—an economic analysis. *New York.*

CHE LIN, B.A. Philippines 1953, M.A. Michigan 1957. Marketing of group insurance. *Pennsylvania.*

PETER LUSTIG, B. Comm. British Columbia 1954; M.B.A. Western Ontario 1955. An analysis of the concentration of economic power in the hands of institutional investors —the Canadian prospective. *Stanford.*

BRUCE W. MORGAN, B.A. Yale 1959, M.A. 1960. A theory of corporate finance and the cost of corporate capital. *Yale.*

JOHN R. MORONEY, JR., B.A. Southern Methodist 1960. A study of profits in the United States, 1922-1961. *Duke.*

WILLIAM MORRIS, JR., B.A. Catholic 1950. Public policy and U.S. direct foreign investment, a study of the postwar U.S. experience. *American.*

WELDON C. NEILL, B.A. Texas 1957; M.A. 1961. A theory of the financial firm. *Texas.*

JAMES M. PEARSON, M.S. Baylor 1954. An inquiry into the effectiveness of margin requirements. *Alabama.*

DENNIS REINMUTH, B.A. Yankton 1958; M.A. Pennsylvania 1960. State insurance of property risks in North Dakota. *Pennsylvania.*

HELMUT WENDEL, B.S. Columbia College 1949; M.A. Columbia 1950. Flexibility of financial transactions in the recent U.S. economy. *Columbia.*

NED WILLIAMS, B.S.C. Mississippi 1947; M.S. Columbia 1949. Financing industry in the South: Mississippi's second "BAWI" program. *Columbia.*

GLENN A. WILT, JR., B.A. Occidental 1956; M.B.A. Miami 1958. Use of preferred stocks in corporate financing. *Michigan.*

GLENN L. WOOD, B.S. Arizona State 1960. Life insurance policy loans. *Pennsylvania.*

JOHN W. WORKS, B.A. Brown 1954; LL.B. Ohio Northern 1956; M.B.A. Northwestern 1958. A study of variation of price movements of the electric utility common stocks. *Northwestern.*

JAI-HOON YANG, B.A. California (Los Angeles) 1960; M.A. 1962. Empirical significance of risk aversion in the theory of risk premium. *California (Los Angeles).*

Charles C. N. Ying, B.A. Minnesota 1957; M.A. Yale 1960. An examination of the stochastic process generating prices and volumes of sales in the stock market. *Harvard.*

Business Organization; Managerial Economics; Industrial Management; Marketing; Accounting

Degrees Conferred

Donald K. Abe, Ph.D. California (Berkeley) 1963. Data for managerial control.

Donald G. Anderson, Ph.D. Iowa 1962. The performance of selected functions by wholesale grocers: 1940 through 1960.

Robert B. Andrews, Ph.D. California (Los Angeles) 1963. An investigation of some properties of the energy expenditure rate and heart rate responses to selected dynamic and static muscular work.

Helmy H. Baligh, Ph.D. California (Berkeley) 1963. Determinants of products assortments carried by some wholesale middlemen.

Nicholas Baloff, Ph.D. Stanford 1963. Manufacturing dynamics; developing a predictive model.

Aly A. Baylor, Ph.D. Southern California 1963. A positive approach to advertising and publicity as a function of consumer preference.

Arthur F. Belote, Ph.D. Florida 1962. Current job evaluation practices in hospitals: an appraisal.

Donald H. Brennecke, D.B.A. Indiana 1962. An investigation of the role of element time variability in the assembly line balancing problem.

William J. Bruns, Jr., Ph.D. California (Berkeley) 1963. A simulation study of alternative methods of inventory valuation.

William R. Campbell, D.B.A. Indiana 1962. Management of real estate function in selected industrial firms.

James M. Carman, Ph.D. Michigan 1963. The demand for durable home furniture.

Forest C. Carter, D.B.A. Indiana 1962. An analysis and evaluation of the partnership provisions in the 1954 internal revenue code.

Robert C. Cauthorn, Ph.D. Tulane 1963. Recent contributions to an improved theory of entrepreneurship.

George N. Christie, Ph.D. New York 1963. Development and application of a system for analyzing company performance.

Elmer G. Close, Ph.D. Florida 1962. Some organizational and functional aspects of the Florida watermelon industry.

Edmond Curcuru, Ph.D. Columbia 1963. Critical issues in the development and administration of advanced management education courses sponsored by selected United States business corporations.

Earl F. Davis, Ph.D. Alabama 1962. A critical analysis of the ethical problems and responsibilities of certified public accountants and lawyers as professional tax practitioners.

Jack T. Dobson, Ph.D. Florida 1962. The possibilities and limitations of management consulting as an aid to small business.

David Drake, Ph.D. Chicago 1963. The pattern of depreciation.

Victor G. Edman, Ph.D. Florida 1962. An economic study of Florida hatcheries.

Gerald Eliot, Ph.D. Southern California 1962. Analysis of small manufacturers' pricing policies in Southern California.

Morelle J. Emerson, Jr., Ph.D. Iowa 1963. A multiple regression-correlation analysis of the spatial variation in retail activity in Iowa, 1958.

John Farley, Ph.D. Chicago 1963. "Brand loyalty" and consumer preferences for branded goods.

Russell W. Fenske, Ph.D. Northwestern 1962. The measurement of productivity at the job, department, and plant level.

William L. Furlong, Ph.D. Alabama 1963. Evaluation of techniques and problems of exchange accounting for foreign operations.

William S. Gere, Jr., Ph.D. Carnegie Inst. Technology 1963. A heuristic approach to job scheduling.

Travis P. Goggans, Ph.D. Oklahoma 1963. Economic aspects of liberalized depreciation.

Andrew A. Grindley, Ph.D. California (Los Angeles) 1962. Tandem queues with dynamic priorities.

Melvin P. Grunloh, Ph.D. Chicago 1962. A constant dollar interpretation of the financial report of large manufacturing companies—1940–1958.

David G. Hayes, Ph.D. Brown 1963. Business velocity and the cash-asset ratio, 1946-1958.

Barry J. Hersker, Ph.D. Florida 1962. The women's apparel manufacturing industry in Florida.

Arthur Hoverland, Ph.D. Michigan 1963. A critical study of the compatibility of selected generally accepted accounting principles with management decision making.

George D. Hughes, Ph.D. Pennsylvania 1963. The measurement of changes in attitude induced by personal selling.

Yuji Ijiri, Ph.D. Carnegie Inst. Technology 1963. Goal oriented models for accounting and control.

Paul Leblang, Ph.D. New York 1963. The effect of merchandise, price and weather on department store advertising.

Chen-Tuan Li, Ph.D. Florida 1962. Input interactions in Florida orange production.

Richard Lindhe, Ph.D. Chicago 1962. Accelerated depreciation of income tax purposes—a study of the decision and some firms who made it.

Charles F. Louie, Ph.D. California (Berkeley) 1963. Accounting for pension costs: the measurement of pension costs in the national accounts.

Bruce Mallen, Ph.D. New York, 1963. Interaction of the marketing system.

Raymond Miles, Ph.D. Stanford 1963. Relationship between managerial success and superior-subordinate compatibility of theory.

R. William Millman, Ph.D. Florida 1962. A general system approach to the analysis of managerial functions.

Anelise N. Mosich, Ph.D. California (Los Angeles) 1963. An evaluation of purchase and pooling concepts of accounting for corporate mergers and acquisitions.

Robert G. Murdick, Ph.D. Florida 1962. Product planning—the strategy of the industrial firm.

Russell C. Parker, Ph.D. Wisconsin 1962. Vertical integration by grocery retailers, a market structure analysis.

Harold A. Petersen, Ph.D. Brown 1963. Risk and the capital structure of the firm.

George Prater, Ph.D. Stanford 1963. Management accounting concepts for planning and controlling inventories.

George G. Reisman, Ph.D. New York 1963. The theory of aggregate profit and the average rate of profit.

Stanley E. Rosenberger, Ph.D. Florida 1962. A history of the Florida vegetable industry and state farmers' markets for vegetables.

N. Edward Sanford, Ph.D. Claremont 1963. Sales control and revenue behavior.

Hadley P. Schaefer, Ph.D. Michigan 1963. Post completion analysis of projects to appraise capital expenditure decisions.

Frederic M. Scherer, Ph.D. Harvard 1963. The weapons acquisition process: economic incentives.

Milton Silver, Ph.D. Columbia 1963. An evaluation of the impact of an integrated data processing system on the organization of a manufacturing company.

L. George Smith, D.B.A. Indiana 1963. Economic concepts basic to an understanding of introductory marketing.

Robert E. Smith, Ph.D. California (Los Angeles) 1963. The California grape growing industry: a case study.

George Sorter, Ph.D. Chicago 1963. The boundaries of the accounting universe.

Curtis H. Stanley, Ph. D. Michigan 1963. The role of objectivity in accounting.

Richard E. Stanley, Ph.D. Florida 1962. A marketing appraisal of the promotional activities of the Florida Citrus Commission from 1935 to 1960.

Louis W. Stern, Ph.D. Northwestern 1962. An economic analysis of the policy of retail price maintenance by consumer goods manufacturers.

William C. Struning, Ph.D. New York 1962. Forecasting and projecting the demand for coffee in the United States.

Seymour Sudman, Ph. D. Chicago 1962. On the accuracy of recording of consumer panels.

Donald L. Thompson, Ph.D. California (Berkeley) 1963. Evaluation of the retail potential of a large metropolitan area with particular reference to the San Francisco Bay trading area.

Theodore A. Van Wormer, Ph.D. Carnegie Inst. Technology 1963. The trimmer: a heuristic approach to the trim problem in the corregated container industry.

Richard J. Whiting, Ph.D. Southern California 1962. An analysis of management functions as influenced by organized labor: the early traditional approach.

Jerome D. Wiest, Ph.D. Carnegie Inst. Technology 1963. Computer models for the scheduling of large projects.

Ross J. Wilhelm, Ph.D. Michigan 1963. A psychological-economic model of advertising-sales effort.

Oliver E. Williamson, Ph.D. Carnegie Inst. Technology 1963. The economics of discretionary behavior: nonpecuniary objectives in the theory of the firm.

Samuel M. Willis, Ph.D. Alabama 1962. An analysis of managerial controls in a specialized textile plant.

Leon Winer, Ph.D. Columbia 1963. Marketing planning concepts and procedures.

Phyllis Wright, Ph.D. Columbia 1963. A design for a woman's clothing inventory.

Theses in Preparation

Henry Assael, B.A. Harvard 1957; M.B.A. Pennsylvania 1959. Manufacturer-dealer relationships in the petroleum and automobile industries: a study in the use and effects of strategies of coercion. *Columbia.*

James H. Bearden, M.A. East Carolina 1959. Problems of purchasing agents in Alabama. *Alabama.*

Wayne S. Boutell, B.A. Chicago 1941; M.B.A. 1950. Auditing and electronic data processing. *California (Berkeley).*

John M. Brazzel, B.S. Texas A & M 1960; B.B.A. 1960. An investigation into the probable economic effects of a new firm on a local area: a general approach. *Tulane.*

James H. Brown, B.S. Wayne State 1955; M.A. Michigan State 1956. An investigation into shoppers purchasing both food and nonfood items in combination food and discount-nonfood stores. *Northwestern.*

Murray Cayley, B.S. Syracuse 1956; M.S. Columbia 1957. Advertising in a developing economy—the Mexican case. *Columbia.*

William E. Cox, Jr., B.S. Florida 1952; M.B.A. Michigan 1957. Product life cycles and promotional strategy in the ethical drug industry. *Michigan.*

HOLLIS DIXON, B.A. Texas Tech. 1954; M.B.A. 1958. An evaluation of the certified public accounting profession in Arkansas. *Arkansas.*

WILLIAM P. DOMMERMUTH, B.A. Iowa 1948; M.B.A. Syracuse 1959. A study of the career backgrounds of the presidents of major American industrial firms with emphasis upon possible effects of the marketing concept upon changes in the composition of the group during the period 1945-1960. *Northwestern.*

WILLIAM DUHAMEL, B.S. St. Louis 1959; M.S. 1960. A management science model for marketing decision making. *Stanford.*

DALE EASTER, B.S. Denver 1957; M.S. Colorado 1959. Uncertainty and accounting values: an investigation into the role of futurity in accounting and economics. *Columbia.*

NEAL E. ECKERT, B.S. Illinois 1959; M.S. Cornell 1961. The influence of advertising on apple marketing. *Cornell.*

ALFRED EICHNER, B.A. Columbia 1958. The corporate revolution. *Columbia.*

ERNEST S. L. GOODWIN, B.S. U.S. Naval Academy 1925; M.B.A. Michigan 1958. Development of organization theory. *Michigan.*

JAMES GUNN, B.S. B.A. Arkansas 1960; M.B.A. 1961. An analysis of the effects of the proposed Arkansas business corporation act on equity accounting for Arkansas corporations. *Arkansas.*

WILLIAM HUIZINGH, B.S.B.A. Denver 1952; M.B.A. 1954. Working capital classification in balance sheets. *Michigan.*

GERALD R. JANTSCHER, B.S. California Inst. Tech. 1960. Depreciation accounting and the allocation of resources. *Columbia.*

CHARLES B. JOHNSTON, B.A. Western Ontario 1954; M.B.A. 1957. The implications of retail change for Canadian paint manufacturers. *Northwestern.*

HERSCHEL KANTER, B.S. Carnegie Inst. Technology 1956; M.S. 1957. The allocation of resources in non-profit organizations with emphasis on the proper use of computing facilities in such organizations. *California (Berkeley).*

PAUL W. KUZNETS, B.A. Swarthmore 1953; M.A. Yale 1954. Financial determinants of manufacturing inventory behavior. *Yale.*

DOUGLAS F. LAMONT, M.B.A. Tulane 1960. Marketing institutions in a developing economy: Mexico. *Alabama.*

C. ROBERT MCGOLDRICK, B. Comm. Laval (Quebec) 1949; M. Comm. 1950; M.S. New York 1951. An analysis of the different food retailing structures of Quebec and Ontario on marketing practices at the manufacturer level, with particular reference to the processed-foods industry. *Northwestern.*

BURTON H. MARCUS, B.A. Queens 1956; M.A. Syracuse 1957. The interrelationships between personality, life style, and media. *Northwestern.*

JOSEPH J. MASTER, B.A. Rollins 1949; M.A. Stetson 1958. A managerial approach to auditing. *Florida.*

CHARLES MERCER, B.A. Memphis State 1958, M.B.A. Arkansas 1960. An inquiry into the expenditures accounting procedures for the State of Arkansas Highway Department. *Arkansas.*

FRANK H. MULLING, M.B.A. Pennsylvania 1951. International cooperation in accounting. *Alabama.*

GARY MUNSINGER, B.S.B.A. Kansas State Teachers 1959; M.B.A. Arkansas 1961. Total marketing: the status of the marketing concept in leading manufacturing firms. *Arkansas.*

JOHN G. MYERS, B.S.F. British Columbia 1954; M.B.A. Western Ontario 1958. Social-psychological correlates of consumer brand choice. *Northwestern.*

J. BRUCE PEARSON, B. Comm. McGill 1953; M.B.A. Western Ontario 1958. An analysis of selected marketing problems of Canadian firms in foreign markets. *Northwestern.*

HAL PICKLE, B.B.A. North Texas 1959. M.B.A. 1960. An inquiry into the characteristics of successful small business managers. *Arkansas.*

Cedric J. Pounders, B.S. Oklahoma 1950; M.A. Baylor 1956; M.B.A. Oklahoma 1957. The methods and theory of cost effectiveness. *Southern Methodist.*

Robert H. Renshaw, B.A. Michigan 1956; M.A. Michigan State 1958. Limits to the size of the firm (nature of long-run average cost function). *Michigan State.*

William C. Reynolds, Jr., M.S. Georgia Inst. Technology 1949. Engineering management models. *Alabama.*

Joseph Rotman, B.A. Western Ontario 1957; M. Comm. Toronto 1960. Maintenance of the channel of distribution. *Columbia.*

Danny E. Schendel, B.S. Wisconsin 1956; M.B.A. Ohio State 1959. Administrative problems of marketing management in new product development. *Stanford.*

Rudolf Skandera, M.S. Columbia 1955; Comm. Eng. Prague 1948. Economic and accounting theory of depreciation—evolution of the concept. *Columbia.*

Stanley F. Stasch, B.S.E.E. Notre Dame 1953; M.S.E.E. 1957; M.B.A. Northwestern 1961. A method of dynamically analyzing the stability of the economic structure of channels of distribution. *Northwestern.*

David H. Stimson, B.S. Kansas 1947; M.S. Kansas City 1956. Approximate measures of value in decision making and resource allocation, an empirical investigation. *California (Berkeley).*

Fred D. Sturdivant, B.S. San Jose State 1959; M.B.A. Oregon 1960. An historical analysis of the American Hospital Supply Corporation with special attention given to the backward integration of a channel of distribution. *Northwestern.*

Arthur L. Thomas, B.A. Cornell 1952; M.B.A. 1956. The realization rule, and certain other approaches to revenue recognition. *Michigan.*

Jose Ventura, B.S.I.E. Technische Hochschule 1958; M.S.I.E. 1960. Present limitations of the automation processes by means of digital computers in complex business decisions. *Columbia.*

Alexander G. Vicas, B. Comm. McGill 1960. The lag of unrestricted imitation behind product innovation. *Princeton.*

Joseph B. Washington, M.B.A. Harvard 1951. Linear programming and accountancy. *Alabama.*

Frederick E. Webster, B.A. Dartmounth 1959; M.B.A. 1960. Marketing management for defense procurement. *Stanford.*

Lois N. Weinberg, B.A. North Carolina 1950. Some case studies in new product economics. *Columbia.*

Industrial Organization; Government and Business; Industry Studies

Degrees Conferred

Peter Asch, Ph.D. Princeton 1963. Industry structure and performance.

Joel Bergsman, Ph.D. Stanford, 1963. Investment planning in the electric power industry.

Dean F. Berry, Ph.D. Michigan 1963. The organization and administration of centralized, corporate staff personnel research units: a descriptive study of the *process* of personnel research activities in selected firms.

Charles M. Brantl, Ph.D. Fordham 1963. American Motors Corporation: an empirical study of the firm.

George M. Brinton, Ph.D. Claremont 1963. Nuclear power and the energy balance in the Euraton countries (1960-1975).

David B. Brooks, Ph.D. Colorado 1963. Minor metals as an industry.

James E. Brown, Ph.D. Florida 1962. Property taxation of public utilities in Virginia: assessment administration and practice.

Omer L. Carey, D.B.A. Indiana 1962. The application of economic criteria for government investment in national parks—the Dinosaur National Monument controversy.

JOSEPH L. CARROLL, D.B.A. Indiana 1962. A critique of waterway benefit-cost analysis: the Tennessee Tombigbee.

DOUGLAS C. DACY, Ph.D. Harvard 1963. Prices and productivity in the construction industry, 1947-1961.

HENRY EINHORN, Ph.D. Columbia 1963. Concentration of domestic industrial output: 1939-1958.

SOUHEIL E. ELIA, Ph.D. American 1963. Group procurement in voluntary hospital management: general analysis, case study, and evaluation.

JAMES M. FERGUSON, Ph.D. Chicago 1962. The advertising rate structure in the daily newspaper industry.

DAVID S. FIELDS, Ph.D. American 1963. Business potentials of telephone communication via satellites (an inquiry into cast determination with projections to 1970).

ROBERT GERWIG, Ph.D. Chicago 1963. Natural gas production—a study of the cost of regulation.

WILLIAM H. HANNUM, Ph.D. Michigan 1963. An approach to the problem of analyzing the size distribution of an industry.

JOHN C. HAUSE, Ph.D. Chicago 1962. New firm entry in manufacturing industries.

GEORGE HEITMANN, Ph.D. Princeton 1963. An econometric study of the regional growth of the footwear industry.

ROBERT T. HOLLAND, Ph.D. Southern California 1963. Economic factors influencing the migration of manufacturing firms.

SAMUEL HOLLANDER, Ph.D. Princeton 1963. The sources of increased efficiency: a case study of Du Pont rayon-manufacturing plants.

MARY A. HOLMAN, Ph.D. George Washington 1963. The utilization of government-owned patented inventions.

MARTIN HYDE, Ph.D. McGill 1963. Commercial fisheries in the Patricia District of Ontario.

ROBERT IGOE, Ph.D. New York 1962. An administrative review of the individual functions and overall performance of the Port of New York Authority.

ROBERT L. KNOX, Ph.D. North Carolina 1963. Workable competition in the rubber tire industry.

JAMES E. LANE, D.B.A. Indiana 1962. The public interest in railroad consolidation.

JAMES L. MCCARTHY, Ph.D. Yale 1963. The American copper industry, 1918-1955.

HOWARD E. MORGAN, Ph.D. California (Berkeley) 1963. Retail market structure and economic growth.

CHIKASHI MORIGUCHI, Ph.D. Michigan 1963. Manufacturers' short-term production decisions under the different phases of business cycles as shown by selected industries, 1949-1960.

JOHN G. MYERS, Ph.D. Columbia 1961. Income distribution and economic welfare in New York State.

LEE NEHRT, Ph.D. Columbia 1963. Selling nuclear power plants overseas.

DAVID L. PADEN, D.B.A. Indiana 1963. Air traffic control.

ALBERT C. PIERSON, Ph.D. Columbia 1963. Trends in lodging enterprises, hotels, and motels, 1939-1958.

WARREN A. PILLSBURY, Ph.D. Virginia 1963. Economic considerations in alternative highway location decisions: collateral effect analysis.

JACK A. PONTNEY, Ph.D. Northwestern 1963. An economic analysis of the retail automobile market.

GEORGE R. POST, Ph.D. Northwestern 1963. Capital expenditures in Canadian manufacturing.

BARRY RICHMAN, Ph.D. Columbia 1963. Informal managerial practices and formal enterprise goals in Soviet industry.

A. E. Rickard:, Ph.D. McGill 1963. The role of aviation in the freight transportation system of the U.S.

Laurance Rosenberg, Ph.D. Cornell 1963. Natural gas pipeline cost allocation.

Alfred W. W. Schoennauer, Ph.D. California (Los Angeles) 1962. An analysis into the time-saved factor as a generator of air passenger traffic.

Elbert W. Segelhorst, Ph.D. Columbia 1963. Fare structure of commuter railroads and traffic congestion in the central city.

C. Joseph Sequin, Ph.D. Michigan State 1962. The transportation of new motor vehicles: an analysis of industry and commission performance.

Narendra K. Sethi, Ph.D. New York 1962. Mary Parker Follett—a critique.

William G. Shepherd, Ph.D. Yale 1963. Prices and investment in public firms: the British fuel industries.

Albert J. Simone, Ph.D. Mass. Inst. Technology 1962. Short-run and long-run cost functions for primary aluminum.

Eugene M. Singer, Ph.D. Princeton 1963. The concept of economic leverage in antitrust.

Joyce G. Skeels, Ph.D. Wisconsin 1963. Monetary reform in the progressive era: interest groups, their ideas and impact, 1890-1913.

George M. Smerk, D.B.A. Indiana 1963. The federal role in urban transportation.

Kenneth Smith, Ph.D. Oklahoma 1962. An evaluation of the criteria used by the Commission in deciding cases arising under the escape clause of the Reciprocal Trade Agreements Act.

Valerie L. Sodano, Ph.D. New York 1963. An analysis of management development in the international petroleum industry—a study of seven companies.

Donald Solar, Ph.D. Columbia 1963. The federal interest in local air service: a study in the evolution of economic policy.

A. Tarasofsky, Ph.D. McGill 1962. Market structure of the petroleum industry.

Brian R. Van Arkadie, Ph.D. California (Berkeley) 1963. A study of capital accumulation in the American steel industry, 1946-1960.

Leonard A. White, Jr., Ph.D. St. Louis 1963. Legal and economic determinations in exclusive dealing and trying contracts cases.

Kenneth P. Wickman, Ph.D. Syracuse 1962. Historical and locational aspects of economic decline in the New England furniture industry.

John Williamson, Ph.D. Princeton 1963. Patent licensing and royalty terms.

Theses in Preparation

Wendell Adamson, B.A. Indiana 1928; M.A. 1937. Analysis of costs incident to the development of a space program. *Columbia.*

Rowland K. Amoa, B.Sc. Econ. Ghana 1960. The structure of the cocoa bean and product markets in U.S. *Mass. Inst. Technology.*

Richard Beland, B.Sc. Montreal 1960; M.A. 1961. Forecasting of manpower in industrial manufacturing in the Province of Quebec. *Toronto.*

Dwight Blood, B.S. Wyoming 1953; M.S. Montana State 1955. A cross-section analysis of the domestic intercity travel market. *Michigan.*

John B. Boatwright, B.A. Stanford 1956; M.A. American (Washington) 1961. An evaluation of the mandatory oil import program and its effects on the American petroleum industry. *American.*

Paul G. Bradley, B.C. Cornell 1956. Two quantitative studies: (1) the demand for petroleum fuels for power generation and (2) petroleum development costs and the characteristics of petroleum study. *Mass. Inst. Technology.*

David Bradwell, B.S. Bristol 1955. The sequence and timing of development in the Saskatchewan River Basin. *Oregon.*

Curtis A. Cramer, B.S. Maryland 1960. The economics of the Federal Power Commission's pricing of natural gas. *Maryland.*

Donald G. Cummings, B.A. Coe 1959. Rate-making in the ocean freight industry. *Tulane.*

Anthony N. Doherty, B.S. Fordham 1960. The buyer's liability under Section 2(f) of the Robinson-Patman Act. *Tulane.*

John B. Duncan, Jr., B.A. Austin 1959. The patent system and the pharmaceutical industry. *Tulane.*

Allan Easton, B.S. Hofstra 1957. The identification and quantification of some intangible qualities of American business firms. *Columbia.*

Bruce W. Erickson, B.A. Michigan State 1959; M.A. 1960. Price fixing under the Sherman Act: some case studies in conspiracy. *Michigan State.*

Irwin Feller, B.B.A. City (New York) 1959. Regional variation in the adoption of inventions: the case of the Draper Loom. *Minnesota.*

Marvin Fischbaum, B.A. Queens 1960. The southern capture of the cotton textile industry. *Columbia.*

Herbert J. Funk, B.S. Iowa (Ames) 1951; M.S. Kansas State 1957. Economic effects of new industry on business firms in a community. *Iowa (Ames).*

Richard C. Gerhan, B.S. Case Inst. 1947. An analysis of the first and second modernization programs of the Japanese iron and steel industry. *Michigan.*

Sushila Gidwani, B.S. Bombay 1951; M.B.A. Minnesota 1960. Effects of technological progress in petroleum refining industry on factor costs. *Minnesota.*

Richard E. Gift. B. A. Vanderbilt 1958; M.A. Emory 1962. The measurement of industrial capacity. *Duke.*

Joseph W. Harrison, B.A. Virginia 1952. Eminent domain, just compensation and resettlement costs. *Virginia.*

Richard Harshbarger, B.S. Manchester 1956; M.A. Indiana 1958. Analysis of TVA coal buying policies. *Indiana.*

Glen W. Herrin, M.S. Florida State 1957. The economic, accounting, and financing problems of the commercial airlines, 1945-1962. *Alabama.*

Seiko Higa, B.A. Pacific 1955. Determinants of telephone investment. *Northwestern.*

Constance J. Hinman, B.A. Columbus 1948; LL.B. Pennsylvania 1951. The history of milk regulation in the United States. *Pittsburgh.*

John D. Huffnagle, B.A. Kent 1957; M.A. 1960. Competition and technological change. *Illinois.*

Carlos P. T. Hurtado, B.A. Chile 1959; M.A. Harvard 1962. The pricing of the transportation services. *Harvard.*

S. Roland Jones, B.A. Mississippi State 1957; M.B.A. 1958. The justification of industrial promotion-the Mississippi case. *Arkansas.*

Richard G. Kedersha, B.S. Rutgers 1942; M.S. 1951; M.B.A. 1954. The impact of brand name prescription products on the traditional practices of high prescription volume pharmacies in Northern New Jersey. *New York.*

David A. Kendrick, B.A. Texas 1960. Economics of scale and the Latin American iron and steel industry. *Mass. Inst. Technology.*

Frederick R. Klein, B.B.A. St. Bonaventure 1950; M.B.A. Harvard 1952. Some economic factors in the pre-Civil War adjustment of the American sailing fleet in the advent of steam. *Columbia.*

Curtis H. Knight, B.S. Pacific 1952; M.S. Wisconsin 1954. The economics of the Soviet food industry. *Indiana.*

Henry K. Krauskopf, B.B.A. Cincinnati 1940; M.A. Columbia 1949. Small business and federal purchasing. *Columbia.*

Carl A. Lawrence, B.S. McGill 1950; M.B.A. Pennsylvania 1952. A study of changes, 1930-1961, in the structure and productivity of Canadian retail stores. *Pennsylvania.*

John E. Lewis, M.B.A. Alabama 1958. An analytical business history of the Alabama Alcoholic Beverage Control Board. *Alabama.*

Wesley H. Long, B.A. Michigan 1959; M.A. 1961. An investigation into the determinants of the demand for interurban air travel in the United States. *Michigan.*

David McFarland, B.A. Millsaps 1955. The market concept in antitrust law. *Vanderbilt.*

Gangadhar R. S. Maddala, B.A. Andhra 1955; M.A. Bombay 1957. Productivity substitution and technical change in the bituminous coal industry. *Chicago.*

William C. Merrill, B.S. Iowa (Ames) 1959. The product selection problem. *California (Berkeley).*

Walter R. Miller, Jr., B.A. Dartmouth 1955; M.B.A. Columbia 1957. Marketing planning for a financial institution. *New York.*

Norman D. O'Bannon, B.A. Texas A & M 1957; M.A. Virginia 1958. The geographic association of manufacturing industries in standard metropolitan areas—1950. *Tulane.*

John L. Peterman, B.A. Drew 1959. Structure of national time rates in television. *Virginia.*

William A. Peterson, B.A. Calvin 1953; M.B.A. Michigan 1958. The public utility income tax allocation problem viewed as a problem of depreciation. *Michigan.*

John Rapp, B.A. Missouri 1959; M.A. 1960. The allocation of highway costs. *Missouri.*

Milton Russell, B.A. Texas College 1955; M.A. Oklahoma 1957. An economic analysis of the public utility and area pricing methods of federal regulation of the independent producers of natural gas. *Oklahoma.*

William A. Schaffer, B.S. Georgia Inst. Technology 1956. Structural changes and manufacturing productivity. *Duke.*

Frederic M. Scherer, B.A. Michigan 1954; M.B.A. Harvard 1958. The weapons acquisition process: economic incentives. *Harvard.*

Leonard G. Schifrin, B.A. Texas 1954; M.A. 1958. The ethical drug industry. *Michigan.*

Hugh H. Schwartz, B.A. Cornell 1954; M.A. Yale 1958. The Argentine experience with industrial credit and protection incentives, 1944–1961. *Yale.*

Leon P. Sydor, B.A. Western Ontario 1960. The St. Lawrence Seaway: A public investment in theory and in practice. *Princeton.*

George Thabet, B.A. American (Cairo) 1956. The economics of air transport in Canada. *Toronto.*

Samuel Thorndike, B.A. Swarthmore 1959. Urban transit extensions in the post-war period. *Columbia.*

Samson G. Tuchman, B.A. Brooklyn 1955; M.A. New York 1957. The economics of the waste paper industry. *New York.*

Lee Van Zant, B.A. Arkansas State 1955. State encouragement of railroad construction in Texas. *Texas.*

William D. Walsh, B. Comm. British Columbia 1958; M.A. Yale 1960. The impact of new technology on firm efficiency in the Pennsylvania iron industry 1840-1870. *Yale.*

George M. Wattles, B.A. Carleton 1939. Pricing and output of U.S. postal services. *Virginia.*

Raymond Whitson, B.A. Oklahoma 1952; M.A. 1957. An analysis of water use in Edmond, Oklahoma. *Oklahoma.*

James E. Willis, B.A. Texas A & M 1958. A study of growth in the telephone industry in Louisiana. *Tulane.*

Arthur W. Wright, B.A. Haverford 1960. Investment decisions in the Soviet fuel industry. *Mass. Inst. Technology.*

Martin J. Wyand, B.A. Pennsylvania State 1953; M.A. 1954. The welfare cause, the Supreme Court and American capitalism. *Illinois.*

Land Economics; Agricultural Economics, Economic Geography; Housing

Degrees Conferred

Dennis J. Aigner, Ph.D. California (Berkeley) 1962. The application of linear programming as a predictive device to estimate short and long term supply response of producers in the processed vegetable industry.

Jay C. Anderson, Ph.D. Iowa (Ames) 1962. Optimal farm plans and normative supply schedules for milk and competing products in Northeastern Iowa.

George L. Beckford, Ph.D. Stanford 1963. Secular trends in the growth of major tropical export-crop industries.

Charles H. Benrud, Ph.D. Minnesota 1963. Economic and other factors associated with variation in alfalfa seed production in South Dakota.

David H. Boyne, Ph.D. Chicago 1962. Changes in the real wealth position of owners of agricultural assets.

Marshall R. Burkes, Ph.D. Ohio State 1962. Changes in the financial strength and structure of agricultural business organizations.

Ching-Yuan Chao, Ph.D. Iowa (Ames) 1963. Dynamic and nonlinear programming for optimum farm plans in Taiwan.

Robert K. Davis, Ph.D. Harvard 1963. The value of outdoor recreation: an economic study of the Maine woods.

Kinchloe C. Davis, Ph.D. Wisconsin 1962. The implications of ground-water in land policy development and in current farm resource adjustment in a semi-arid region.

Rollin M. Dennistoun, Ph.D. Minnesota 1963. Some economic aspects of an agricultural machinery leasing program.

Norbert A. Dorow, Ph.D. Minnesota 1962. Economics of forage handling systems.

William D. Eickhoff, Ph.D. Ohio State 1963. Market structure and performance relationships in the Ohio fluid milk industry.

Morelle J. Emerson, Ph.D. Iowa 1963. A multiple regression-correlation analysis of the spatial variation in retail activity in Iowa, 1958.

Duane E. Erickson, Ph.D. Minnesota 1962. An economic analysis of the feeder cattle enterprise.

Lyle P. Fettig, Ph.D. Chicago 1963. Price indexes for new farm tractors in the postwar period.

Robert S. Firch, Ph.D. Chicago 1963. Stabilization of the United States economy and stability of farm income.

Walter Gensurowsky, Ph.D. Iowa (Ames) 1963. Integration of input-output and programming in land resource supply analysis.

James R. Gibbons, Ph.D. Iowa (Ames) 1963. Costs, economics of scale and alternative production methods in steer feeding in relation to overall farm resource use.

D. I. Gooding, Ph.D. Ohio State 1962. Economic efficiency of retailing produce commodities by different methods of merchandising.

Richard J. Goodman, Ph.D. Minnesota 1962. Organization, structure and competitive behavior of the Twin Cities milk market—producer and distributor level.

David N. Harrington, Ph.D. Iowa (Ames) 1962. Statistical analysis of corn storage costs —Iowa elevators.

Richard Heifner, Ph.D. Iowa (Ames) 1963. The conservation reserve program as a means of controlling agricultural production.

James P. Houck, Jr., Ph.D. Minnesota 1963. Demand and price analysis of the United States' soybean market.

Loren A. Ihnen, Ph.D. Iowa (Ames) 1962. Income opportunities from full-time farming in South Central Iowa.

Roger G. Johnson, Ph.D. Minnesota 1962. The relationship of characteristics of farmers to their efficiency of production in the dairy and hog enterprises.

Sanders A. Kahn, Ph.D. New York 1962. The effect of public vehicular facilities upon neighboring property (emphasizing bridges).

Jack L. Knetsch, Ph.D. Harvard 1963. Influence of reservoir projects on land values.

Raj Krishna, Ph.D. Chicago 1961. Farm supply response in the Punjab (India-Pakistan): a case study of cotton.

Lester B. Lave, Ph.D. Harvard 1963. Measurements of technological change in American agriculture: 1850-1960.

Samuel H. Logan, Ph.D. California 1962. An economic analysis of scale economies in beef slaughter plants.

Roger B. Long, Ph.D. Minnesota 1963. An investigation into the nature of supplemental irrigation with an emphasis on the economic, motivation and physical factors involved.

Bobby C. McGough, Ph.D. Florida 1962. A critical analysis of the impact of highway improvements on land values as measured by the Expressway in Jacksonville and Manatee Avenue in Bradenton, Florida.

James F. Madden, Ph.D. Iowa (Ames) 1962. Multiple variable milk production functions with point and interval estimates of derived quantities.

Edgar A. Miller, Jr., Ph.D. Ohio State 1962. A mathematical method locating livestock markets in Ohio to minimize average total cost of transportation and market operation.

James P. Miller, Ph.D. Pittsburgh 1963. Economic problems associated with sanitary waste collection, treatment and disposal.

Carmen O. Nohre, Ph.D. Minnesota 1963. Optimal organizations for farms and normative supply responses for hogs and beef in Southern Central Minnesota.

Hugh O. Nourse, Ph.D. Chicago 1962. The effect of public housing on property values in St. Louis.

Fred L. Olson, Ph.D. Minnesota 1962. An analysis of the proportion of Grade A and Grade B milk patrons in Minnesota and Northwestern Wisconsin.

Ross Parish, Ph.D. Chicago 1962. The economic significance of summer fallowing in the spring-wheat region of North America.

Jin H. Park, Ph.D. Minnesota 1963. Economics of resource use on rice farms in Korea.

William L. Park, Ph.D. Cornell 1963. Compensation of milk producer cooperatives for market-wide services under federal milk orders.

Anthony H. Pavlick, Ph.D. Minnesota 1963. An analysis of the effects of federal foreign programs on the incomes of Appalachian farmers.

William C. Pendleton, Ph.D. Chicago 1963. The value of highway accessibility.

Hans Popp, Ph.D. Chicago 1962. Response of farm production to prices: a case study of the supply of dairy products in Switzerland.

Martin L. Primack, Ph.D. North Carolina 1963. Farm formed capital in American agriculture, 1850-1910.

Lynn Rader, Ph.D. California (Berkeley) 1963. Economic analysis of range improvement practices in beef cattle ranch management for the foothill range areas of California.

Lloyd C. Rixe, Ph.D. Minnesota 1962. Cost economies to size and resource use in Red River Valley farming.

Ezra Sadan, Ph.D. Chicago 1962. Agricultural settlements in Israel.

Lyle P. Schertz, Ph.D. Minnesota 1962. An economic analysis of direct controls on marketings in the feed-livestock sector.

Lawrence E. Schwartz, Ph.D. Harvard 1963. From core to ring: an econometric study of the relocation of the central office and its workers.

David R. F. Simpson, Ph.D. Harvard 1963. An economic analysis of crofting agriculture.

Daniel G. Sisler, Ph.D. Cornell 1962. Direct production payments in the feed-grain-livestock sector of American agriculture.

L. Orlo Sorenson, Ph.D. Minnesota 1963. An economic analysis of government grain storage programs and their impact on grain market organization in Kansas.

Gordon L. Spangler, Ph.D. Fletcher School 1963. Farm parity and trade equality: a study of conflicts between U.S. farm and international trade policies.

Charles M. Studness, Ph.D. Columbia 1963. Development of the Great Plains grain farming economy: frontier to 1953.

Robert W. Thomas Jr., Ph.D. Iowa (Ames) 1962. Land and its resources in a general demand structure.

James F. Thompson, Ph.D. Chicago 1962. Part-time farming and farm resource productivity in Western Kentucky.

Russell G. Thompson, Ph.D. Minnesota 1962. An approach to estimating optimum sizes of butter-powder plants.

Jean-Paul Valette, Ph.D. Colorado 1962. The Moulouya River development: a case study for the testing of welfare hypotheses and the establishment of an economico-political criterion serving as a policy guide for the rational allocation and development of water resources in Northwestern Africa.

Ethel G. Vatter, Ph.D. Iowa 1962. The composition and distribution of Iowa farm incomes by county.

Eduardo Venezian. Ph.D. Iowa (Ames) 1962. Economic optima for fertilizer production functions in relation to weather, crop soil, and location variables.

James V. Walther, Ph.D. Colorado 1962. Analysis of rent, housing and OPA rent control with special reference to the residential housing-occupancy markets in the City of Denver, 1940 to 1960.

Yi Wang, Ph.D. Ohio State 1962. The demand and price structure for various classes of wheat.

John (T. C.) Ying, Ph.D. Minnesota 1963. Relationship of earning and efficiency to acres per farm.

Theses in Preparation

Susan R. Agria, B.A. Duke 1959. Some economic effects of special tax treatment for mineral industries. *Chicago.*

Donald E. Anderson, B.S. North Dakota State 1963; M.S. 1957. Impact of Commodity Credit Corporation activity on grain market structure. *Minnesota.*

Willis E. Anthony, B.S. Minnesota 1960; M.S. 1961. Adjustments in the meat packing industry to changing patterns of production and consumption. *Minnesota.*

Dennis Lee Bawden, B.S. Montana State 1956; M.A. 1959. Interregional competition study of the United States turkey industry. *California (Berkeley).*

Duran Bell, Jr., B.A. California (Berkeley) 1960. The theory of commodity transfer over time and space. *California (Berkeley).*

John Berton, B.S. B.A. Tulsa 1957; M.B.A. Arkansas 1961. Marketing of Arkansas lumber and wood products. *Arkansas.*

Frank H. Bollman, B.Agr.Sc. Queensland 1952; B. Comm. Melbourne 1959. An evaluation of the role of natural resources in regional economic development. *California (Berkeley).*

Gladstone G. Bonnick, B.A. Roosevelt 1958; M.A. Chicago 1962. The United States Sugar Acts: effects on Puerto Rican sugarcane agriculture. *Chicago.*

Gerald Bregar, B.S. B.A. Florida 1952; M.A. Miami 1956. The economics of urban blight and economic criteria for urban renewal. *Arkansas.*

Ray F. Brokken, B.S. Iowa (Ames) 1958. Regional comparative advantage in the production of crops and livestock in the United States. *Iowa (Ames).*

Byron Brown, B.A. Cornell 1960. An econometric study of supply and demand conditions for municipal water. *Johns Hopkins.*

Gardner M. Brown, B.A. Antioch 1959. The distribution of benefits and costs in water resources development: a case study of the southern San Joaquin Valley. *California (Berkeley).*

Jacob A. Brown, B.S. Saskatchewan 1951; M.S. North Dakota State 1953. Land policy research. *Minnesota.*

Kenneth T. Cann, B.S. Georgetown 1950; M.A. Indiana 1960. The effects of real property taxation on land resource distribution in Southern Brazil. *Indiana.*

Kaye D. Capstick, B.S. Detroit 1961. A re-examination of housing market analysis—a case study. *Florida.*

John Murray Cormack, B.S.A. Manitoba 1957; M.S. Nebraska 1961. Alternative procedures for planning small watershed development. *Iowa (Ames).*

Ralph W. Cummings, Jr., B.A. North Carolina 1960. Responsiveness to price incentives in Indian agriculture, 1947-1962. *Michigan.*

Dale C. Dahl, B.S. South Dakota State 1957, M.S. 1959. Structural adjustment in agribusiness sector of the Minnesota economy. *Minnesota.*

Winston A. Dummett, B.A. British Columbia, 1951; B.S. 1957. Postwar development and efficiency relationships in Puerto Rican agriculture. *Chicago.*

Thomas L. Edwards, B.A. Berea 1960. Total costs of the federal price-support program with respect to the basic commodities. *Claremont.*

Rollo L. Ehrich, B.S. Minnesota 1957; M.S. 1959; M.A. Stanford 1961. Analysis of relation among central market wheat prices, with special reference to the impact of the loan program. *Stanford.*

John E. Floyd, B. Comm. Saskatchewan 1958; M.A. Chicago 1962. The distribution effects of farm policy: a comparison of the experience of Canada and the United States. *Chicago.*

Dwight M. Gadsby, B.S. Iowa (Ames) 1954; M.S. 1960. Farm real estate market in Iowa. *Iowa (Ames).*

Ahmed A. M. Goueli, B.Sc. Alexandria 1957; M.S. California (Berkeley) 1962. Economic planning of production and marketing facilities. *California (Berkeley).*

Richard C. Haidacher, B.S. Purdue 1959; M.A. 1960. An econometric study of the California prune industry, investigating the domestic and foreign demand determinants and recognizing sizes, qualities, and utilization outlets. *California (Berkeley).*

John K. Hanes, B.S. Louisiana State 1955; M.S. 1957. Market structure of Red River Valley potato industry. *Minnesota.*

Ian W. Hardie, B.S. California (Berkeley) 1960. Quality based raw product prices for a cooperative processing and marketing walnuts. *California (Berkeley).*

Robin G. Henning, B.S. Ohio State 1951; M.S. 1954. The economics of allocating grape supplies among alternative uses. *Cornell.*

Peter W. House, B.A. American Int'l. 1959; M.A. Clark 1961. Assessment and taxation of farmland in the rural-urban areas in New York State. *Cornell.*

Ahmad Kamali-Nafar, B.S. Tehran 1941; M.S. Minnesota 1961. Economic and social impact of credit institutions on agricultural development in Iran. *Minnesota.*

Hiromitsu Kaneda, B.A. Doshisha 1957; B.A. Amherst 1959; M.A. Stanford 1961. Returns to scale and technical change: an econometric study of the Japanese agricultural development in the post-war years. *Stanford.*

Dennis Keefe, B.A. St. John's 1959. Study of the interrelationships between farm family economic decisions and farm management decisions. *Minnesota.*

Tatsuo Kobayashi, B.A. Beloit 1957; M.A. Missouri 1959. Tax base, tax load, and land-use effects of suburban residential development in agricultural areas. *California (Berkeley).*

Uma J. Lele, B.A. Poona 1960; M.S. Cornell 1963. The effect in the Indian economy of autonomous changes in agricultural production on agricultural prices and marketings. *Cornell.*

Vernon R. McMinimy, B.S. Kansas State 1958; M.S. 1959. The costs and efficiency of distributing wholesale bread. *California (Berkeley).*

Julius Maldutis, B.A. Brooklyn 1960; M.A. Columbia 1962. Lithuanian land reforms: 1923-1939. *Columbia.*

Sebald G. Manger Cats, M.Sc. Holland 1957. Land tenure in Guatemala. *Cornell.*

David E. Moser, B.A. Willamette 1935; M.S. Columbia 1938. The role of transportation in the Missouri livestock industry—an economic analysis. *Columbia.*

Thomas E. Nichols, B.S. North Carolina State 1951; M.S. 1959. Inter-regional competition in the soybean crushing industry with particular attention to the Southeast. *Duke.*

Carlo Perone-Pacifico, Laurea, Naples 1957; M.S. California (Berkeley) 1963. Economics of scale and location: fruit assembly and processing in Southern Italy. *California (Berkeley).*

Yakir Plessner, B.Sc. Hebrew 1961. M.S. 1963. Non-linear programming approaches to regional and interregional agricultural planning problems. *Iowa (Ames).*

Gordon D. Rose, B.S. South Dakota State 1955; M.S. 1958. Economics of land forming. *Iowa (Ames).*

William E. Saupe, B.S. Iowa (Ames) 1951; M.S. 1961. Projected equilibrium characteristics of the farm industry in the North Central region in 1980. *Iowa (Ames).*

Stuart G. Schmid, B.S. Stanford 1956; M.A. California (Los Angeles) 1961. A measurement of the value of commuting time using location differentials in housing prices. *California (Los Angeles).*

Robert D. Schultz, B.S. Purdue 1958; M.S. 1960. Decision making in a wildland fire game. *California (Berkeley).*

Jerome B. Siebert, B.S. California (Berkeley) 1960. An investigation into the optimum costs and efficiencies of orange packing houses taking into consideration the optimum number, size and location. *California (Berkeley).*

Melvin D. Skold, B.S. Colorado State 1958; M.S. 1959. Programming national food production to meet demand growth. *Iowa (Ames).*

Gordon W. Smith, B.A. Washington (St. Louis) 1956. Marketing of agricultural products in Brazil. *Harvard.*

Lorna Stokenbury, B.S. B.A. Arkansas 1957; M.B.A. 1958. A source of new, single-family housing. *Arkansas.*

Gary C. Taylor, B.S. Cornell 1952; M.S. 1958. The economic planning of water conveyance systems with particular reference to irrigation development in California. *California (Berkeley).*

Dennis E. Teeguarden, B.S. Michigan College Mining and Technology 1953; M.F. California (Berkeley) 1958. Stability and efficiency of markets for forest products from small forest ownerships in the Central Sierra Nevada region. *California (Berkeley).*

Alfred J. Van Tassel, B.S. California 1934. Mechanization and labor productivity in the lumber industry since 1940. *Columbia.*

Thomas F. Weaver, B.S. Pennsylvania State 1958; M.S. Cornell 1961. The economics of water reallocation in Raipur District, India. *Cornell.*

Norman K. Whittlesey, B.S. Colorado State 1955; M.S. 1960. Regional and national effects of various wheat, cotton and feed grain programs. *Iowa (Ames).*

Robert E. Will, B.A. Carleton 1950; M.A. Yale 1951. A study of the relationships between certain specific social costs and urban size. *Yale.*

Labor Economics

Degrees Conferred

Theodore M. Alfred, Ph.D. Mass. Inst. Technology 1963. Labor market analysis of the staffing function.

Federico M. Barela, Ph.D. North Carolina 1963. The Puerto Rico labor relations act: a state labor policy and its applications.

Lawrence J. Bell, Ph.D. Harvard 1963. Standards of internal wage structure consistency.

John C. Bixby, Ph.D. Southern California 1963. The evolution of the labor injunction with particular emphasis upon contemporaneous factors influencing its application.

David H. Clark, Ph.D. Wisconsin 1962. An analytical view of the history of health insurance 1910-1959.

Eldon J. Dvorak, Ph.D. Washington 1962. The prospective growth of trade union membership in the United States.

Walter A. Fogel, Ph.D. Mass. Inst. Technology 1962. The relationship between job rate ranges and unit wage costs in a retail food firm.

Izzat I. Ghurani, Ph.D. Houston 1963. An evaluative study of the international labor organization manpower program during the development decades.

Theodore I. Goldberg, Ph.D. Toronto 1962. Trade union interest in medical care and voluntary health insurance: a study of two collectively bargained programmes.

Jack H. Gore, Ph.D. Colorado 1962. The seasonal agricultural labor market in Colorado.

Hebert (no first name supplied), Ph.D. McGill 1963. L'extension juridique des conventions collectives dans l'industrie de la construction dans la province de Québec: 1934-62.

E. E. Herman, Ph.D. McGill 1962. Freedom of association and recognition of labour organizations.

Daniel L. Horowitz, Ph.D. Harvard 1963. The Italian labor movement.

Robbin R. Hough, Ph.D. Mass. Inst. Technology 1962. The National Labor Board under the Taft-Hartley Act: a quantitative study.

Mary D. Houska, Ph.D. Mass. Inst. Technology 1963. Substitution of engineers for non-engineers in industry.

D. L. Howell, Ph.D. Alabama 1963. The right-to-work law: an instrument for social control of the union security problem.

George C. Hoyt, Ph.D. California (Berkeley) 1962. A study of retirement problems.

Ethel Jones, Ph.D. Chicago 1961. Hours of work in the United States, 1900-1957.

Hirschel Kasper, Ph.D. Minnesota 1963. Relation of duration of unemployment to workers' "reservation" wages.

Truly E. Kincey, Ph.D. Ohio State 1963. A study of the relationship between firm size and some of the factors important in establishing supplemental unemployment benefits programs in the Ohio rubber industry.

Cyril C. Ling, D.B.A. Indiana 1962. A history of the personnel relations function in American business.

Charles A. Lininger, Ph.D. Chicago 1962. The effect of weekly unemployment benefit amounts on the duration of unemployment benefits.

Allan B. Mandelstamm, Ph.D. Michigan 1963. The effects of unions on efficiency in the residential construction industry: a case study.

Mack A. Moore, Ph.D. Wisconsin 1963. The role of temporary help services in the clerical labor market.

Scott E. Pardee, Ph.D. Mass. Inst. Technology 1962. A study of inter-city wage differentials.

DAVID J. PARK, Ph.D. Southern California 1962. Employment distribution, income, and city size: a statistical analysis.

LEONARD RAPPING, Ph.D. Chicago 1961. The impact of federal subsidies and maritime unionism on the relative earnings of seamen.

PHILIP ROSS, Ph.D. Brown 1963. The government as a source of union power: an examination and evaluation of the role of the duty to bargain.

ERNST W. STROMSDORFER, Ph.D. Washington (St. Louis) 1962. Structural unemployment in a local labor market area: a theoretical and empirical investigation of the process of labor force transformation.

ERIC W. VETTER, Ph.D. Michigan 1963. Company long-range manpower planning-method approach.

DONALD L. WINKELMANN, Ph.D. Minnesota 1963. Some factors affecting the agricultural labor supply in Minnesota.

FREDERICK A. ZELLER, Ph.D. Ohio State 1963. Some attitudes and behavior of local union members.

Theses in Preparation

LEWIS M. ABERNATHY, B.B.A. Mississippi 1954; M.B.A. 1959. Economic characteristics of the labor force of the Aeronautical Center of the Federal Aviation Center, Oklahoma City. *Oklahoma.*

TREVOR BAIN, B.A. City (New York) 1953; M.I.L.R. Cornell 1957. Labor relations in the flat glass industry. *California (Berkeley).*

SARVAN BHATIA, B.A. Punjab 1952: M.A. 1955. The role of the International Labour Organization in developing countries with special reference to India. *Ohio State.*

MICHAEL E. BORUS, B.A. Trinity 1959; M.A. Yale 1960. The effects of retraining the unemployed in Connecticut. *Yale.*

JOHN F. BURTON, B.S. Cornell 1957; LL.B. Michigan 1960. Determinants of interstate differences in the employers' cost of workmen's compensation in selected Midwestern States. *Michigan.*

GLEN G. CAIN, B.A. Lake Forest 1955; M.A. California (Berkeley) 1957. Labor force participation of married women. *Chicago.*

PEI YANG CHANG, LL.M. New York 1953. Picketing. *New York.*

FORREST E. COOKSON, B.S. Clemson 1953; M.A. Princeton 1957. The theory of the Phillips Curve. *Georgetown.*

JOHN W. CORDELL, (Earlier degree not supplied). The arbitration of senority disputes: standards and decision patterns. *Claremont.*

JOHN W. DORSEY, JR., B.S. Maryland 1958; M.A. Harvard 1962. The Mack Truck case: a study in unemployment. *Harvard.*

STANLEY L. FRIEDLANDER, B.A. City (New York) 1959; M.A. Illinois 1960. Post World War II Puerto Rican labor migration and its effect on the economic growth of Puerto Rico. *Mass. Inst. Technology.*

DAVID G. FUNK, B.A. Amherst 1960. Labor immobility. *Harvard.*

JOSEPH GLATT, Lic. Brussels 1934, 1937. A case study of capital investment in human beings in Israel. *Columbia.*

CY W. GONICK, B.A. California (Berkeley) 1957; M.A. 1963. Some aspects of unemployment in Canada. *California (Berkeley).*

DAVID HAMPTON, B.A. Michigan 1955; M.B.A. Southern California 1960. The contribution of an integrated system of labor-management relations. *Columbia.*

CHONG C. HU, M.B.A. Xaxier (Ohio) 1960. The temporary extended unemployment compensation program 1961-62: a study of experience. *St. Louis.*

PAUL B. HUBER, B.A. Yale 1956; M.A. 1960. Disguised unemployment in the United States. *Yale.*

Clinton E. Jencks, B.A. Colorado 1939; M.A. California (Berkeley) 1963. The impact of nationalization upon working conditions and grievance procedures of the British coal-mine workers. *California (Berkeley)*

Lamar B. Jones, B.A. Louisiana Poly. Institute 1959; M.A. Louisiana State 1961. Comparative agricultural labor movements: foreign and American. *Texas.*

S. Kazi, B. Comm. McGill 1959; M.A. 1961. Organized labour in Pakistan—an underdeveloped economy. *McGill.*

Kye C. Kim, (earlier degree not supplied). Wage structure and wage changes. *New York (Buffalo).*

Vane B. Lucas Jr., B.A. Northwest Missouri 1961. Impact of collective bargaining on the private pension mechanism. *Pennsylvania.*

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UNCERTAINTY AND THE WELFARE ECONOMICS OF MEDICAL CARE

By KENNETH J. ARROW*

I. *Introduction: Scope and Method*

This paper is an exploratory and tentative study of the specific differentia of medical care as the object of normative economics. It is contended here, on the basis of comparison of obvious characteristics of the medical-care industry with the norms of welfare economics, that the special economic problems of medical care can be explained as adaptations to the existence of uncertainty in the incidence of disease and in the efficacy of treatment.

It should be noted that the subject is the *medical-care industry,* not *health.* The causal factors in health are many, and the provision of medical care is only one. Particularly at low levels of income, other commodities such as nutrition, shelter, clothing, and sanitation may be much more significant. It is the complex of services that center about the physician, private and group practice, hospitals, and public health, which I propose to discuss.

The focus of discussion will be on the way the operation of the medical-care industry and the efficacy with which it satisfies the needs of society differ from a norm, if at all. The "norm" that the economist usually uses for the purposes of such comparisons is the operation of a competitive model, that is, the flows of services that would be offered and purchased and the prices that would be paid for them if each individual in the market offered or purchased services at the going prices as if his decisions had no influence over them, and the going prices were such that the amounts of services which were available equalled the total amounts which other individuals were willing to purchase, with no imposed restrictions on supply or demand.

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* The author is professor of economics at Stanford University. He wishes to express his thanks for useful comments to F. Bator, R. Dorfman, V. Fuchs, Dr. S. Gilson, R. Kessel, S. Mushkin, and C. R. Rorem. This paper was prepared under the sponsorship of the Ford Foundation as part of a series of papers on the economics of health, education, and welfare.

The interest in the competitive model stems partly from its presumed descriptive power and partly from its implications for economic efficiency. In particular, we can state the following well-known proposition (First Optimality Theorem). If a competitive equilibrium exists at all, and if all commodities relevant to costs or utilities are in fact priced in the market, then the equilibrium is necessarily *optimal* in the following precise sense (due to V. Pareto): There is no other allocation of resources to services which will make all participants in the market better off.

Both the conditions of this optimality theorem and the definition of optimality call for comment. A definition is just a definition, but when the *definiendum* is a word already in common use with highly favorable connotations, it is clear that we are really trying to be persuasive; we are implicitly recommending the achievement of optimal states.[1] It is reasonable enough to assert that a change in allocation which makes all participants better off is one that certainly should be made; this is a value judgment, not a descriptive proposition, but it is a very weak one. From this it follows that it is not desirable to put up with a nonoptimal allocation. But it does not follow that if we are at an allocation which is optimal in the Pareto sense, we should not change to any other. We cannot indeed make a change that does not hurt someone; but we can still desire to change to another allocation if the change makes enough participants better off and by so much that we feel that the injury to others is not enough to offset the benefits. Such interpersonal comparisons are, of course, value judgments. The change, however, by the previous argument ought to be an optimal state; of course there are many possible states, each of which is optimal in the sense here used.

However, a value judgment on the desirability of each possible new distribution of benefits and costs corresponding to each possible reallocation of resources is not, in general, necessary. Judgments about the distribution can be made separately, in one sense, from those about allocation if certain conditions are fulfilled. Before stating the relevant proposition, it is necessary to remark that the competitive equilibrium achieved depends in good measure on the initial distribution of purchasing power, which consists of ownership of assets and skills that command a price on the market. A transfer of assets among individuals will, in general, change the final supplies of goods and services and the prices paid for them. Thus, a transfer of purchasing power from the well to the ill will increase the demand for medical services. This will manifest itself in the short run in an increase in the price of medical services and in the long run in an increase in the amount supplied.

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[1] This point has been stressed by I. M. D. Little [19, pp. 71-74]. For the concept of a "persuasive definition," see C. L. Stevenson [27, pp. 210-17].

With this in mind, the following statement can be made (Second Optimality Theorem): If there are no increasing returns in production, and if certain other minor conditions are satisfied, then every optimal state is a competitive equilibrium corresponding to some initial distribution of purchasing power. Operationally, the significance of this proposition is that if the conditions of the two optimality theorems are satisfied, and if the allocation mechanism in the real world satisfies the conditions for a competitive model, then social policy can confine itself to steps taken to alter the distribution of purchasing power. For any given distribution of purchasing power, the market will, under the assumptions made, achieve a competitive equilibrium which is necessarily optimal; and any optimal state is a competitive equilibrium corresponding to some distribution of purchasing power, so that any desired optimal state can be achieved.

The redistribution of purchasing power among individuals most simply takes the form of money: taxes and subsidies. The implications of such a transfer for individual satisfactions are, in general, not known in advance. But we can assume that society can *ex post* judge the distribution of satisfactions and, if deemed unsatisfactory, take steps to correct it by subsequent transfers. Thus, by successive approximations, a most preferred social state can be achieved, with resource allocation being handled by the market and public policy confined to the redistribution of money income.[2]

If, on the contrary, the actual market differs significantly from the competitive model, or if the assumptions of the two optimality theorems are not fulfilled, the separation of allocative and distributional procedures becomes, in most cases, impossible.[3]

The first step then in the analysis of the medical-care market is the

[2] The separation between allocation and distribution even under the above assumptions has glossed over problems in the execution of any desired redistribution policy; in practice, it is virtually impossible to find a set of taxes and subsidies that will not have an adverse effect on the achievement of an optimal state. But this discussion would take us even further afield than we have already gone.

[3] The basic theorems of welfare economics alluded to so briefly above have been the subject of voluminous literature, but no thoroughly satisfactory statement covering both the theorems themselves and the significance of exceptions to them exists. The positive assertions of welfare economics and their relation to the theory of competitive equilibrium are admirably covered in Koopmans [18]. The best summary of the various ways in which the theorems can fail to hold is probably Bator's [6].

comparison between the actual market and the competitive model. The methodology of this comparison has been a recurrent subject of controversy in economics for over a century. Recently, M. Friedman [15] has vigorously argued that the competitive or any other model should be tested solely by its ability to predict. In the context of competition, he comes close to arguing that prices and quantities are the only relevant data. This point of view is valuable in stressing that a certain amount of lack of realism in the assumptions of a model is no argument against its value. But the price-quantity implications of the competitive model for pricing are not easy to derive without major—and, in many cases, impossible—econometric efforts.

In this paper, the institutional organization and the observable mores of the medical profession are included among the data to be used in assessing the competitiveness of the medical-care market. I shall also examine the presence or absence of the preconditions for the equivalence of competitive equilibria and optimal states. The major competitive preconditions, in the sense used here, are three: the *existence* of competitive equilibrium, the *marketability* of all goods and services relevant to costs and utilities, and *nonincreasing returns*. The first two, as we have seen, insure that competitive equilibrium is necessarily optimal; the third insures that every optimal state is the competitive equilibrium corresponding to some distribution of income.[4] The first and third conditions are interrelated; indeed, nonincreasing returns plus some additional conditions not restrictive in a modern economy imply the existence of a competitive equilibrium, i.e., imply that there will be some set of prices which will clear all markets.[5]

The concept of marketability is somewhat broader than the traditional divergence between private and social costs and benefits. The latter concept refers to cases in which the organization of the market does not require an individual to pay for costs that he imposes on others as the result of his actions or does not permit him to receive compensation for benefits he confers. In the medical field, the obvious example is the spread of communicable diseases. An individual who fails to be immunized not only risks his own health, a disutility which presumably he has weighed against the utility of avoiding the procedure, but also that of others. In an ideal price system, there would be a price which he would have to pay to anyone whose health is endangered, a price sufficiently high so that the others would feel compensated; or, alternatively, there would be a price which would be paid to him by others to induce him to undergo the immunization procedure.

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[4] There are further minor conditions, for which see Koopmans [18, pp. 50-55].

[5] For a more precise statement of the existence conditions, see Koopmans [18, pp. 56-60] or Debreu [12, Ch. 5].

Either system would lead to an optimal state, though the distributional implications would be different. It is, of course, not hard to see that such price systems could not, in fact, be practical; to approximate an optimal state it would be necessary to have collective intervention in the form of subsidy or tax or compulsion.

By the absence of marketability for an action which is identifiable, technologically possible, and capable of influencing some individual's welfare, for better or for worse, is meant here the failure of the existing market to provide a means whereby the services can be both offered and demanded upon payment of a price. Nonmarketability may be due to intrinsic technological characteristics of the product which prevent a suitable price from being enforced, as in the case of communicable diseases, or it may be due to social or historical controls, such as those prohibiting an individual from selling himself into slavery. This distinction is, in fact, difficult to make precise, though it is obviously of importance for policy; for the present purposes, it will be sufficient to identify nonmarketability with the observed absence of markets.

The instance of nonmarketability with which we shall be most concerned is that of risk-bearing. The relevance of risk-bearing to medical care seems obvious; illness is to a considerable extent an unpredictable phenomenon. The ability to shift the risks of illness to others is worth a price which many are willing to pay. Because of pooling and of superior willingness and ability, others are willing to bear the risks. Nevertheless, as we shall see in greater detail, a great many risks are not covered, and indeed the markets for the services of risk-coverage are poorly developed or nonexistent. Why this should be so is explained in more detail in Section IV.C below; briefly, it is impossible to draw up insurance policies which will sufficiently distinguish among risks, particularly since observation of the results will be incapable of distinguishing between avoidable and unavoidable risks, so that incentives to avoid losses are diluted.

The optimality theorems discussed above are usually presented in the literature as referring only to conditions of certainty, but there is no difficulty in extending them to the case of risks, provided the additional services of risk-bearing are included with other commodities.[6]

However, the variety of possible risks in the world is really staggering. The relevant commodities include, in effect, bets on all possible occurrences in the world which impinge upon utilities. In fact, many of these "commodities," i.e., desired protection against many risks, are simply not available. Thus, a wide class of commodities is nonmarketable, and a basic competitive precondition is not satisfied.[7]

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[6] The theory, in variant forms, seems to have been first worked out by Allais [2], Arrow [5], and Baudier [7]. For further generalization, see Debreu [11] and [12, Ch. 7].

There is a still more subtle consequence of the introduction of risk-bearing considerations. When there is uncertainty, information or knowledge becomes a commodity. Like other commodities, it has a cost of production and a cost of transmission, and so it is naturally not spread out over the entire population but concentrated among those who can profit most from it. (These costs may be measured in time or disutility as well as money.) But the demand for information is difficult to discuss in the rational terms usually employed. The value of information is frequently not known in any meaningful sense to the buyer; if, indeed, he knew enough to measure the value of information, he would know the information itself. But information, in the form of skilled care, is precisely what is being bought from most physicians, and, indeed, from most professionals. The elusive character of information as a commodity suggests that it departs considerably from the usual marketability assumptions about commodities.[8]

That risk and uncertainty are, in fact, significant elements in medical care hardly needs argument. I will hold that virtually all the special features of this industry, in fact, stem from the prevalence of uncertainty.

The nonexistence of markets for the bearing of some risks in the first instance reduces welfare for those who wish to transfer those risks to others for a certain price, as well as for those who would find it profitable to take on the risk at such prices. But it also reduces the desire to render or consume services which have risky consequences; in technical language, these commodities are complementary to risk-bearing. Conversely, the production and consumption of commodities and services with little risk attached act as substitutes for risk-bearing and are encouraged by market failure there with respect to risk-bearing. Thus the observed commodity pattern will be affected by the nonexistence of other markets.

[7] It should also be remarked that in the presence of uncertainty, indivisibilities that are sufficiently small to create little difficulty for the existence and viability of competitive equilibrium may nevertheless give rise to a considerable range of increasing returns because of the operation of the law of large numbers. Since most objects of insurance (lives, fire hazards, etc.) have some element of indivisibility, insurance companies have to be above a certain size. But it is not clear that this effect is sufficiently great to create serious obstacles to the existence and viability of competitive equilibrium in practice.

[8] One form of production of information is research. Not only does the product have unconventional aspects as a commodity, but it is also subject to increasing returns in use, since new ideas, once developed, can be used over and over without being consumed, and to difficulties of market control, since the cost of reproduction is usually much less than that of production. Hence, it is not surprising that a free enterprise economy will tend to underinvest in research; see Nelson [21] and Arrow [4].

The failure of one or more of the competitive preconditions has as its most immediate and obvious consequence a reduction in welfare below that obtainable from existing resources and technology, in the sense of a failure to reach an optimal state in the sense of Pareto. But more can be said. I propose here the view that, when the market fails to achieve an optimal state, society will, to some extent at least, recognize the gap, and nonmarket social institutions will arise attempting to bridge it.[9] Certainly this process is not necessarily conscious; nor is it uniformly successful in approaching more closely to optimality when the entire range of consequences is considered. It has always been a favorite activity of economists to point out that actions which on their face achieve a desirable goal may have less obvious consequences, particularly over time, which more than offset the original gains.

But it is contended here that the special structural characteristics of the medical-care market are largely attempts to overcome the lack of optimality due to the nonmarketability of the bearing of suitable risks and the imperfect marketability of information. These compensatory institutional changes, with some reinforcement from usual profit motives, largely explain the observed noncompetitive behavior of the medical-care market, behavior which, in itself, interferes with optimality. The social adjustment towards optimality thus puts obstacles in its own path.

The doctrine that society will seek to achieve optimality by nonmarket means if it cannot achieve them in the market is not novel. Certainly, the government, at least in its economic activities, is usually implicitly or explicitly held to function as the agency which substitutes for the market's failure.[10] I am arguing here that in some circumstances other social institutions will step into the optimality gap, and that the medical-care industry, with its variety of special institutions, some ancient, some modern, exemplifies this tendency.

It may be useful to remark here that a good part of the preference for redistribution expressed in government taxation and expenditure policies and private charity can be reinterpreted as desire for insurance. It is noteworthy that virtually nowhere is there a system of subsidies that has as its aim simply an equalization of income. The subsidies or other governmental help go to those who are disadvantaged in life by events the incidence of which is popularly regarded as unpredictable: the blind, dependent children, the medically indigent. Thus, optimality, in a context which includes risk-bearing, includes much that appears to be motivated by distributional value judgments when looked at in a narrower context.[11]

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[9] An important current situation in which normal market relations have had to be greatly modified in the presence of great risks is the production and procurement of modern weapons; see Peck and Scherer [23, pp. 581-82] (I am indebted for this reference to V. Fuchs) and [1, pp. 71-75].

[10] For an explicit statement of this view, see Baumol [8]. But I believe this position is implicit in most discussions of the functions of government.

This methodological background gives rise to the following plan for this paper. Section II is a catalogue of stylized generalizations about the medical-care market which differentiate it from the usual commodity markets. In Section III the behavior of the market is compared with that of the competitive model which disregards the fact of uncertainty. In Section IV, the medical-care market is compared, both as to behavior and as to preconditions, with the ideal competitive market that takes account of uncertainty; an attempt will be made to demonstrate that the characteristics outlined in Section II can be explained either as the result of deviations from the competitive preconditions or as attempts to compensate by other institutions for these failures. The discussion is not designed to be definitive, but provocative. In particular, I have been chary about drawing policy inferences; to a considerable extent, they depend on further research, for which the present paper is intended to provide a framework.

II. *A Survey of the Special Characteristics of the Medical-Care Market*[12]

This section will list selectively some characteristics of medical care which distinguish it from the usual commodity of economics textbooks. The list is not exhaustive, and it is not claimed that the characteristics listed are individually unique to this market. But, taken together, they do establish a special place for medical care in economic analysis.

A. *The Nature of Demand*

The most obvious distinguishing characteristics of an individual's demand for medical services is that it is not steady in origin as, for example, for food or clothing, but irregular and unpredictable. Medical services, apart from preventive services, afford satisfaction only in the event of illness, a departure from the normal state of affairs. It is hard, indeed, to think of another commodity of significance in the average budget of which this is true. A portion of legal services, devoted to defense in criminal trials or to lawsuits, might fall in this category but the incidence is surely very much lower (and, of course, there are, in fact, strong institutional similarities between the legal and medical-care markets.)[13]

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[11] Since writing the above, I find that Buchanan and Tullock [10, Ch. 13] have argued that all redistribution can be interpreted as "income insurance."

[12] For an illuminating survey to which I am much indebted, see S. Mushkin [20].

In addition, the demand for medical services is associated, with a considerable probability, with an assault on personal integrity. There is some risk of death and a more considerable risk of impairment of full functioning. In particular, there is a major potential for loss or reduction of earning ability. The risks are not by themselves unique; food is also a necessity, but avoidance of deprivation of food can be guaranteed with sufficient income, where the same cannot be said of avoidance of illness. Illness is, thus, not only risky but a costly risk in itself, apart from the cost of medical care.

B. *Expected Behavior of the Physician*

It is clear from everyday observation that the behavior expected of sellers of medical care is different from that of business men in general. These expectations are relevant because medical care belongs to the category of commodities for which the product and the activity of production are identical. In all such cases, the customer cannot test the product before consuming it, and there is an element of trust in the relation.[14] But the ethically understood restrictions on the activities of a physician are much more severe than on those of, say, a barber. His behavior is supposed to be governed by a concern for the customer's welfare which would not be expected of a salesman. In Talcott Parsons's terms, there is a "collectivity-orientation," which distinguishes medicine and other professions from business, where self-interest on the part of participants is the accepted norm.[15]

A few illustrations will indicate the degree of difference between the behavior expected of physicians and that expected of the typical businessman.[16] (1) Advertising and overt price competition are virtually eliminated among physicians. (2) Advice given by physicians as to further treatment by himself or others is supposed to be completely

[13] In governmental demand, military power is an example of a service used only irregularly and unpredictably. Here too, special institutional and professional relations have emerged, though the precise social structure is different for reasons that are not hard to analyze.

[14] Even with material commodities, testing is never so adequate that all elements of implicit trust can be eliminated. Of course, over the long run, experience with the quality of product of a given seller provides a check on the possibility of trust.

[15] See [22, p. 463]. The whole of [22, Ch. 10] is a most illuminating analysis of the social role of medical practice; though Parsons' interest lies in different areas from mine, I must acknowledge here my indebtedness to his work.

[16] I am indebted to Herbert Klarman of Johns Hopkins University for some of the points discussed in this and the following paragraph.

divorced from self-interest. (3) It is at least claimed that treatment is dictated by the objective needs of the case and not limited by financial considerations.[17] While the ethical compulsion is surely not as absolute in fact as it is in theory, we can hardly suppose that it has no influence over resource allocation in this area. Charity treatment in one form or another does exist because of this tradition about human rights to adequate medical care.[18] (4) The physician is relied on as an expert in certifying to the existence of illnesses and injuries for various legal and other purposes. It is socially expected that his concern for the correct conveying of information will, when appropriate, outweigh his desire to please his customers.[19]

Departure from the profit motive is strikingly manifested by the overwhelming predominance of nonprofit over proprietary hospitals.[20] The hospital per se offers services not too different from those of a hotel, and it is certainly not obvious that the profit motive will not lead to a more efficient supply. The explanation may lie either on the supply side or on that of demand. The simplest explanation is that public and private subsidies decrease the cost to the patient in nonprofit hospitals. A second possibility is that the association of profit-making with the supply of medical services arouses suspicion and antagonism on the part of patients and referring physicians, so they do prefer nonprofit institutions. Either explanation implies a preference on the part of some group, whether donors or patients, against the profit motive in the supply of hospital services.[21]

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[17] The belief that the ethics of medicine demands treatment independent of the patient's ability to pay is strongly ingrained. Such a perceptive observer as René Dubos has made the remark that the high cost of anticoagulants restricts their use and may contradict classical medical ethics, as though this were an unprecedented phenomenon. See [13, p. 419]. "A time *may come* when medical ethics will have to be considered in the harsh light of economics" (emphasis added). Of course, this expectation amounts to ignoring the scarcity of medical resources; one has only to have been poor to realize the error. We may confidently assume that price and income do have some consequences for medical expenditures.

[18] A needed piece of research is a study of the exact nature of the variations of medical care received and medical care paid for as income rises. (The relevant income concept also needs study.) For this purpose, some disaggregation is needed; differences in hospital care which are essentially matters of comfort should, in the above view, be much more responsive to income than, e.g., drugs.

[19] This role is enhanced in a socialist society, where the state itself is actively concerned with illness in relation to work; see Field [14, Ch. 9].

[20] About 3 per cent of beds were in proprietary hospitals in 1958, against 30 per cent in voluntary nonprofit, and the remainder in federal, state, and local hospitals; see [26, Chart 4-2, p. 60].

[21] C. R. Rorem has pointed out to me some further factors in this analysis. (1) Given the social intention of helping all patients without regard to immediate ability to pay, economies of scale would dictate a predominance of community-sponsored hospitals. (2)

Conformity to collectivity-oriented behavior is especially important since it is a commonplace that the physician-patient relation affects the quality of the medical care product. A pure cash nexus would be inadequate; if nothing else, the patient expects that the same physician will normally treat him on successive occasions. This expectation is strong enough to persist even in the Soviet Union, where medical care is nominally removed from the market place [14, pp. 194-96]. That purely psychic interactions between physician and patient have effects which are objectively indistinguishable in kind from the effects of medication is evidenced by the use of the placebo as a control in medical experimentation; see Shapiro [25].

C. *Product Uncertainty*

Uncertainty as to the quality of the product is perhaps more intense here than in any other important commodity. Recovery from disease is as unpredictable as is its incidence. In most commodities, the possibility of learning from one's own experience or that of others is strong because there is an adequate number of trials. In the case of severe illness, that is, in general, not true; the uncertainty due to inexperience is added to the intrinsic difficulty of prediction. Further, the amount of uncertainty, measured in terms of utility variability, is certainly much greater for medical care in severe cases than for, say, houses or automobiles, even though these are also expenditures sufficiently infrequent so that there may be considerable residual uncertainty.

Further, there is a special quality to the uncertainty; it is very different on the two sides of the transaction. Because medical knowledge is so complicated, the information possessed by the physician as to the consequences and possibilities of treatment is necessarily very much greater than that of the patient, or at least so it is believed by both parties.[22] Further, both parties are aware of this informational inequality, and their relation is colored by this knowledge.

To avoid misunderstanding, observe that the difference in information relevant here is a difference in information as to the consequence of a purchase of medical care. There is always an inequality of information as to production methods between the producer and the purchaser of any commodity, but in most cases the customer may well

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Some proprietary hospitals will tend to control total costs to the patient more closely, including the fees of physicians, who will therefore tend to prefer community-sponsored hospitals.

[22] Without trying to assess the present situation, it is clear in retrospect that at some point in the past the actual differential knowledge possessed by physicians may not have been much. But from the economic point of view, it is the subjective belief of both parties, as manifested in their market behavior, that is relevant.

have as good or nearly as good an understanding of the utility of the product as the producer.

D. *Supply Conditions*

In competitive theory, the supply of a commodity is governed by the net return from its production compared with the return derivable from the use of the same resources elsewhere. There are several significant departures from this theory in the case of medical care.

Most obviously, entry to the profession is restricted by licensing. Licensing, of course, restricts supply and therefore increases the cost of medical care. It is defended as guaranteeing a minimum of quality. Restriction of entry by licensing occurs in most professions, including barbering and undertaking.

A second feature is perhaps even more remarkable. The cost of medical education today is high and, according to the usual figures, is borne only to a minor extent by the student. Thus, the private benefits to the entering student considerably exceed the costs. (It is, however, possible that research costs, not properly chargeable to education, swell the apparent difference.) This subsidy should, in principle, cause a fall in the price of medical services, which, however, is offset by rationing through limited entry to schools and through elimination of students during the medical-school career. These restrictions basically render superfluous the licensing, except in regard to graduates of foreign schools.

The special role of educational institutions in simultaneously subsidizing and rationing entry is common to all professions requiring advanced training.[23] It is a striking and insufficiently remarked phenomenon that such an important part of resource allocation should be performed by nonprofit-oriented agencies.

Since this last phenomenon goes well beyond the purely medical aspect, we will not dwell on it longer here except to note that the anomaly is most striking in the medical field. Educational costs tend to be far higher there than in any other branch of professional training. While tuition is the same, or only slightly higher, so that the subsidy is much greater, at the same time the earnings of physicians rank highest among professional groups, so there would not at first blush seem to be any necessity for special inducements to enter the profession. Even if we grant that, for reasons unexamined here, there is a social interest in subsidized professional education, it is not clear why the rate of subsidization should differ among professions. One might expect that the tuition of medical students would be higher than that of other students.

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[23] The degree of subsidy in different branches of professional education is worthy of a major research effort.

The high cost of medical education in the United States is itself a reflection of the quality standards imposed by the American Medical Association since the Flexner Report, and it is, I believe, only since then that the subsidy element in medical education has become significant. Previously, many medical schools paid their way or even yielded a profit.

Another interesting feature of limitation on entry to subsidized education is the extent of individual preferences concerning the social welfare, as manifested by contributions to private universities. But whether support is public or private, the important point is that both the quality and the quantity of the supply of medical care are being strongly influenced by social nonmarket forces.[24, 25]

One striking consequence of the control of quality is the restriction on the range offered. If many qualities of a commodity are possible, it would usually happen in a competitive market that many qualities will be offered on the market, at suitably varying prices, to appeal to different tastes and incomes. Both the licensing laws and the standards of medical-school training have limited the possibilities of alternative qualities of medical care. The declining ratio of physicians to total employees in the medical-care industry shows that substitution of less trained personnel, technicians, and the like, is not prevented completely, but the central role of the highly trained physician is not affected at all.[26]

E. *Pricing Practices*

The unusual pricing practices and attitudes of the medical profession are well known: extensive price discrimination by income (with an extreme of zero prices for sufficiently indigent patients) and, formerly, a strong insistence on fee for services as against such alternatives as prepayment.

[24] Strictly speaking, there are four variables in the market for physicians: price, quality of entering students, quality of education, and quantity. The basic market forces, demand for medical services and supply of entering students, determine two relations among the four variables. Hence, if the nonmarket forces determine the last two, market forces will determine price and quality of entrants.

[25] The supply of Ph.D.'s is similarly governed, but there are other conditions in the market which are much different, especially on the demand side.

[26] Today only the Soviet Union offers an alternative lower level of medical personnel, the feldshers, who practice primarily in the rural districts (the institution dates back to the 18th century). According to Field [14, pp. 98-100, 132-33], there is clear evidence of strain in the relations between physicians and feldshers, but it is not certain that the feldshers will gradually disappear as physicians grow in numbers.

The opposition to prepayment is closely related to an even stronger opposition to closed-panel practice (contractual arrangements which bind the patient to a particular group of physicians). Again these attitudes seem to differentiate professions from business. Prepayment and closed-panel plans are virtually nonexistent in the legal profession. In ordinary business, on the other hand, there exists a wide variety of exclusive service contracts involving sharing of risks; it is assumed that competition will select those which satisfy needs best.[27]

The problems of implicit and explicit price-fixing should also be mentioned. Price competition is frowned on. Arrangements of this type are not uncommon in service industries, and they have not been subjected to antitrust action. How important this is is hard to assess. It has been pointed out many times that the apparent rigidity of so-called administered prices considerably understates the actual flexibility. Here, too, if physicians find themselves with unoccupied time, rates are likely to go down, openly or covertly; if there is insufficient time for the demand, rates will surely rise. The "ethics" of price competition may decrease the flexibility of price responses, but probably that is all.

III. *Comparisons with the Competitive Model under Certainty*

A. *Nonmarketable Commodities*

As already noted, the diffusion of communicable diseases provides an obvious example of nonmarket interactions. But from a theoretical viewpoint, the issues are well understood, and there is little point in expanding on this theme. (This should not be interpreted as minimizing the contribution of public health to welfare; there is every reason to suppose that it is considerably more important than all other aspects of medical care.)

Beyond this special area there is a more general interdependence, the concern of individuals for the health of others. The economic manifestations of this taste are to be found in individual donations to hospitals and to medical education, as well as in the widely accepted responsibilities of government in this area. The taste for improving the health of others appears to be stronger than for improving other aspects of their welfare.[28]

In interdependencies generated by concern for the welfare of others there is always a theoretical case for collective action if each participant derives satisfaction from the contributions of all.

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[27] The law does impose some limits on risk-shifting in contracts, for example, its general refusal to honor exculpatory clauses.

[28] There may be an identification problem in this observation. If the failure of the market system is, or appears to be, greater in medical care than in, say, food an individual otherwise equally concerned about the two aspects of others' welfare may prefer to help in the first.

B. *Increasing Returns*

Problems associated with increasing returns play some role in allocation of resources in the medical field, particularly in areas of low density or low income. Hospitals show increasing returns up to a point; specialists and some medical equipment constitute significant indivisibilities. In many parts of the world the individual physician may be a large unit relative to demand. In such cases it can be socially desirable to subsidize the appropriate medical-care unit. The appropriate mode of analysis is much the same as for water-resource projects. Increasing returns are hardly apt to be a significant problem in general practice in large cities in the United States, and improved transportation to some extent reduces their importance elsewhere.

C. *Entry*

The most striking departure from competitive behavior is restriction on entry to the field, as discussed in II.D above. Friedman and Kuznets, in a detailed examination of the pre-World War II data, have argued that the higher income of physicians could be attributed to this restriction.[29]

There is some evidence that the demand for admission to medical school has dropped (as indicated by the number of applicants per place and the quality of those admitted), so that the number of medical-school places is not as significant a barrier to entry as in the early 1950's [28, pp. 14-15]. But it certainly has operated over the past and it is still operating to a considerable extent today. It has, of course, constituted a direct and unsubtle restriction on the supply of medical care.

There are several considerations that must be added to help evaluate the importance of entry restrictions: (1) Additional entrants would be, in general, of lower quality; hence, the addition to the supply of medical care, properly adjusted for quality, is less than purely quantitative calculations would show.[30] (2) To achieve genuinely competitive conditions, it would be necessary not only to remove numerical restrictions on entry but also to remove the subsidy in medical education. Like any other producer, the physician should bear all the costs of production,

[29] See [16, pp. 118-37]. The calculations involve many assumptions and must be regarded as tenuous; see the comments by C. Reinold Noyes in [16, pp. 407-10].

[30] It might be argued that the existence of racial discrimination in entrance has meant that some of the rejected applicants are superior to some accepted. However, there is no necessary connection between an increase in the number of entrants and a reduction in racial discrimination; so long as there is excess demand for entry, discrimination can continue unabated and new entrants will be inferior to those previously accepted.

including, in this case, education.[31] It is not so clear that this change would not keep even unrestricted entry down below the present level. (3) To some extent, the effect of making tuition carry the full cost of education will be to create too few entrants, rather than too many. Given the imperfections of the capital market, loans for this purpose to those who do not have the cash are difficult to obtain. The lender really has no security. The obvious answer is some form of insured loans, as has frequently been argued; not too much ingenuity would be needed to create a credit system for medical (and other branches of higher) education. Under these conditions the cost would still constitute a deterrent, but one to be compared with the high future incomes to be obtained.

If entry were governed by ideal competitive conditions, it may be that the quantity on balance would be increased, though this conclusion is not obvious. The average quality would probably fall, even under an ideal credit system, since subsidy plus selected entry draw some highly qualified individuals who would otherwise get into other fields. The decline in quality is not an over-all social loss, since it is accompanied by increase in quality in other fields of endeavor; indeed, if demands accurately reflected utilities, there would be a net social gain through a switch to competitive entry.[32]

There is a second aspect of entry in which the contrast with competitive behavior is, in many respects, even sharper. It is the exclusion of many imperfect substitutes for physicians. The licensing laws, though they do not effectively limit the number of physicians, do exclude all others from engaging in any one of the activities known as medical practice. As a result, costly physician time may be employed at specific tasks for which only a small fraction of their training is needed, and which could be performed by others less well trained and therefore less expensive. One might expect immunization centers, privately operated, but not necessarily requiring the services of doctors.

In the competitive model without uncertainty, consumers are presumed to be able to distinguish qualities of the commodities they buy. Under this hypothesis, licensing would be, at best, superfluous and exclude those from whom consumers would not buy anyway; but it might exclude too many.

D. *Pricing*

The pricing practices of the medical industry (see II.E above) depart sharply from the competitive norm. As Kessel [17] has pointed out with great vigor, not only is price discrimination incompatible with the competitive model, but its preservation in the face of the large number of physicians is equivalent to a collective monopoly. In the past, the opposition to prepayment plans has taken distinctly coercive forms, certainly transcending market pressures, to say the least.

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[31] One problem here is that the tax laws do not permit depreciation of professional education, so that there is a discrimination against this form of investment.

[32] To anticipate later discussion, this condition is not necessarily fulfilled. When it comes to quality choices, the market may be inaccurate.

Kessel has argued that price discrimination is designed to maximize profits along the classic lines of discriminating monopoly and that organized medical opposition to prepayment was motivated by the desire to protect these profits. In principle, prepayment schemes are compatible with discrimination, but in practice they do not usually discriminate. I do not believe the evidence that the actual scale of discrimination is profit-maximizing is convincing. In particular, note that for any monopoly, discriminating or otherwise, the elasticity of demand in each market at the point of maximum profits is greater than one. But it is almost surely true for medical care that the price elasticity of demand for all income levels is less than one. That price discrimination by income is not completely profit-maximizing is obvious in the extreme case of charity; Kessel argues that this represents an appeasement of public opinion. But this already shows the incompleteness of the model and suggests the relevance and importance of social and ethical factors.

Certainly one important part of the opposition to prepayment was its close relation to closed-panel plans. Prepayment is a form of insurance, and naturally the individual physician did not wish to assume the risks. Pooling was intrinsically involved, and this strongly motivates, as we shall discuss further in Section IV below, control over prices and benefits. The simplest administrative form is the closed panel; physicians involved are, in effect, the insuring agent. From this point of view, Blue Cross solved the prepayment problem by universalizing the closed panel.

The case that price discrimination by income is a form of profit maximization which was zealously defended by opposition to fees for service seems far from proven. But it remains true that this price discrimination, for whatever cause, is a source of nonoptimality. Hypothetically, it means everyone would be better off if prices were made equal for all, and the rich compensated the poor for the changes in the relative positions. The importance of this welfare loss depends on the actual amount of discrimination and on the elasticities of demand for medical services by the different income groups. If the discussion is simplified by considering only two income levels, rich and poor, and if the elasticity of demand by either one is zero, then no reallocation of medical services will take place and the initial situation is optimal. The only effect of a change in price will be the redistribution of income as between the medical profession and the group with the zero elasticity of demand. With low elasticities of demand, the gain will be small. To illustrate, suppose the price of medical care to the rich is double that to the poor, the medical expenditures by the rich are 20 per cent of those by the poor, and the elasticity of demand for both classes is .5; then the net social gain due to the abolition of discrimination is slightly over 1 per cent of previous medical expenditures.[33]

The issues involved in the opposition to prepayment, the other major anomaly in medical pricing, are not meaningful in the world of certainty and will be discussed below.

IV. *Comparison with the Ideal Competitive Model under Uncertainty*

A. *Introduction*

In this section we will compare the operations of the actual medical-care market with those of an ideal system in which not only the usual commodities and services but also insurance policies against all conceivable risks are available.[34] Departures consist for the most part of

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[33] It is assumed that there are two classes, rich and poor; the price of medical services to the rich is twice that to the poor, medical expenditures by the rich are 20 per cent of those by the poor, and the elasticity of demand for medical services is .5 for both classes. Let us choose our quantity and monetary units so that the quantity of medical services consumed by the poor and the price they pay are both 1. Then the rich purchase .1 units of medical services at a price of 2. Given the assumption about the elasticities of demand, the demand function of the rich is $D_R(p) = .14\ p^{-.5}$ and that of the poor is $D_P(p) = p^{-.5}$. The supply of medical services is assumed fixed and therefore must equal 1.1. If price discrimination were abolished, the equilibrium price, $\bar{p}$, must satisfy the relation,

$$D_R(\bar{p}) + D_P(\bar{p}) = 1.1,$$

and therefore $\bar{p} = 1.07$. The quantities of medical care purchased by the rich and poor, respectively, would be $D_R(\bar{p}) = .135$ and $D_P(\bar{p}) = .965$.

The inverse demand functions, the price to be paid corresponding to any given quantity are $d_R(q) = .02/q^2$, and $d_P(q) = 1/q^2$. Therefore, the consumers' surplus to the rich generated by the change is:

$$\int_{.1}^{.135} (.02/q^2)dq - \bar{p}(.135 - .1), \tag{1}$$

and similarly the loss in consumers' surplus by the poor is:

$$\int_{.965}^{1} (1/q^2)dq - \bar{p}(1 - .965) \tag{2}$$

If (2) is subtracted from (1), the second terms cancel, and the aggregate increase in consumers' surplus is .0156, or a little over 1 per cent of the initial expenditures.

[34] A striking illustration of the desire for security in medical care is provided by the expressed preferences of *émigrés* from the Soviet Union as between Soviet medical practice and German or American practice; see Field [14, Ch. 12]. Those in Germany preferred the German system to the Soviet, but those in the United States preferred (in a ratio of 3 to 1) the Soviet system. The reasons given boil down to the certainty of medical care, independent of income or health fluctuations.

insurance policies that might conceivably be written, but are in fact not. Whether these potential commodities are nonmarketable, or, merely because of some imperfection in the market, are not actually marketed, is a somewhat fine point.

To recall what has already been said in Section I, there are two kinds of risks involved in medical care: the risk of becoming ill, and the risk of total or incomplete or delayed recovery. The loss due to illness is only partially the cost of medical care. It also consists of discomfort and loss of productive time during illness, and, in more serious cases, death or prolonged deprivation of normal function. From the point of view of the welfare economics of uncertainty, both losses are risks against which individuals would like to insure. The nonexistence of suitable insurance policies for either risk implies a loss of welfare.

B. *The Theory of Ideal Insurance*

In this section, the basic principles of an optimal regime for risk-bearing will be presented. For illustration, reference will usually be made to the case of insurance against cost in medical care. The principles are equally applicable to any of the risks. There is no single source to which the reader can be easily referred, though I think the principles are at least reasonably well understood.

As a basis for the analysis, the assumption is made that each individual acts so as to maximize the expected value of a utility function. If we think of utility as attached to income, then the costs of medical care act as a random deduction from this income, and it is the expected value of the utility of income after medical costs that we are concerned with. (Income after medical costs is the ability to spend money on other objects which give satisfaction. We presuppose that illness is not a source of satisfaction in itself; to the extent that it is a source of dissatisfaction, the illness should enter into the utility function as a separate variable.) The expected-utility hypothesis, due originally to Daniel Bernoulli (1738), is plausible and is the most analytically manageable of all hypotheses that have been proposed to explain behavior under uncertainty. In any case, the results to follow probably would not be significantly affected by moving to another mode of analysis.

It is further assumed that individuals are normally risk-averters. In utility terms, this means that they have a diminishing marginal utility of income. This assumption may reasonably be taken to hold for most of the significant affairs of life for a majority of people, but the presence of gambling provides some difficulty in the full application of this view. It follows from the assumption of risk aversion that if an individual is given a choice between a probability distribution of income, with a given mean m, and the certainty of the income m, he would prefer the latter. Suppose, therefore, an agency, a large insurance company plan, or the government, stands ready to offer insurance against medical costs on an actuarially fair basis; that is, if the costs of medical care are a random variable with mean *m*, the company will charge a premium *m*, and agree to indemnify the individual for all medical costs. Under these circumstances, the individual will certainly prefer to take out a policy and will have a welfare gain thereby.

Will this be a social gain? Obviously yes, if the insurance agent is suffering no social loss. Under the assumption that medical risks on different individuals are basically independent, the pooling of them reduces the risk involved to the insurer to relatively small proportions. In the limit, the welfare loss, even assuming risk aversion on the part of the insurer, would vanish and there is a net social gain which may be of quite substantial magnitude. In fact, of course, the pooling of risks does not go to the limit; there is only a finite number of them and there may be some interdependence among the risks due to epidemics and the like. But then a premium, perhaps slightly above the actuarial level, would be sufficient to offset this welfare loss. From the point of view of the individual, since he has a strict preference for the actuarially fair policy over assuming the risks himself, he will still have a preference for an actuarially unfair policy, provided, of course, that it is not too unfair.

In addition to a residual degree of risk aversion by insurers, there are other reasons for the loading of the premium (i.e., an excess of premium over the actuarial value). Insurance involves administrative costs. Also, because of the irregularity of payments there is likely to be a cost of capital tied up. Suppose, to take a simple case, the insurance company is not willing to sell any insurance policy that a consumer wants but will charge a fixed-percentage loading above the actuarial value for its premium. Then it can be shown that the most preferred policy from the point of view of an individual is a coverage with a deductible amount; that is, the insurance policy provides 100 per cent coverage for all medical costs in excess of some fixed-dollar limit. If, however, the insurance company has some degree of risk aversion, its loading may also depend on the degree of uncertainty of the risk. In that case, the Pareto optimal policy will involve some element of coinsurance, i.e., the coverage for costs over the minimum limit will be some fraction less than 100 per cent (for proofs of these statements, see Appendix).

These results can also be applied to the hypothetical concept of insurance against failure to recover from illness. For simplicity, let us assume that the cost of failure to recover is regarded purely as a money cost, either simply productive opportunities foregone or, more generally, the money equivalent of all dissatisfactions. Suppose further that, given that a person is ill, the expected value of medical care is greater than its cost; that is, the expected money value attributable to recovery with medical help is greater than resources devoted to medical help. However, the recovery, though on the average beneficial, is uncertain; in the absence of insurance a risk-averter may well prefer not to take a chance on further impoverishment by buying medical care. A suitable insurance policy would, however, mean that he paid nothing if he doesn't benefit; since the expected value is greater than the cost, there would be a net social gain.[35]

C. *Problems of Insurance*

1. *The moral hazard.* The welfare case for insurance policies of all sorts is overwhelming. It follows that the government should undertake insurance in those cases where this market, for whatever reason, has failed to emerge. Nevertheless, there are a number of significant practical limitations on the use of insurance. It is important to understand them, though I do not believe that they alter the case for the creation of a much wider class of insurance policies than now exists.

One of the limits which has been much stressed in insurance literature is the effect of insurance on incentives. What is desired in the case of insurance is that the event against which insurance is taken be out of the control of the individual. Unfortunately, in real life this separation can never be made perfectly. The outbreak of fire in one's house or business may be largely uncontrollable by the individual, but the probability of fire is somewhat influenced by carelessness, and of course arson is a possibility, if an extreme one. Similarly, in medical policies the cost of medical care is not completely determined by the illness suffered by the individual but depends on the choice of a doctor and his willingness to use medical services. It is frequently observed that widespread medical insurance increases the demand for medical care. Coinsurance provisions have been introduced into many major medical policies to meet this contingency as well as the risk aversion of the insurance companies.

To some extent the professional relationship between physician and patient limits the normal hazard in various forms of medical insurance. By certifying to the necessity of given treatment or the lack thereof, the physician acts as a controlling agent on behalf of the insurance companies. Needless to say, it is a far from perfect check; the physicians themselves are not under any control and it may be convenient for them or pleasing to their patients to prescribe more expensive medication, private nurses, more frequent treatments, and other marginal variations of care. It is probably true that hospitalization and surgery are more under the casual inspection of others than is general practice and therefore less subject to moral hazard; this may be one reason why insurance policies in those fields have been more widespread.

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[35] It is a popular belief that the Chinese, at one time, paid their physicians when well but not when sick.

2. *Alternative methods of insurance payment.* It is interesting that no less than three different methods of coverage of the costs of medical care have arisen: prepayment, indemnities according to a fixed schedule, and insurance against costs, whatever they may be. In prepayment plans, insurance in effect is paid in kind—that is, directly in medical services. The other two forms both involve cash payments to the beneficiary, but in the one case the amounts to be paid involving a medical contingency are fixed in advance, while in the other the insurance carrier pays all the costs, whatever they may be, subject, of course, to provisions like deductibles and coinsurance.

In hypothetically perfect markets these three forms of insurance would be equivalent. The indemnities stipulated would, in fact, equal the market price of the services, so that value to the insured would be the same if he were to be paid the fixed sum or the market price or were given the services free. In fact, of course, insurance against full costs and prepayment plans both offer insurance against uncertainty as to the price of medical services, in addition to uncertainty about their needs. Further, by their mode of compensation to the physician, prepayment plans are inevitably bound up with closed panels so that the freedom of choice of the physician by the patient is less than it would be under a scheme more strictly confined to the provision of insurance. These remarks are tentative, and the question of coexistence of the different schemes should be a fruitful subject for investigation.

3. *Third-party control over payments.* The moral hazard in physicians' control noted in paragraph 1 above shows itself in those insurance schemes where the physician has the greatest control, namely, major medical insurance. Here there has been a marked rise in expenditures over time. In prepayment plans, where the insurance and medical service are supplied by the same group, the incentive to keep medical costs to a minimum is strongest. In plans of the Blue Cross group, there has developed a conflict of interest between the insurance carrier and the medical-service supplier, in this case particularly the hospital.

The need for third-party control is reinforced by another aspect of the moral hazard. Insurance removes the incentive on the part of individuals, patients, and physicians to shop around for better prices for hospitalization and surgical care. The market forces, therefore, tend to be replaced by direct institutional control.

4. *Administrative costs.* The pure theory of insurance sketched in Section B above omits one very important consideration: the costs of operating an insurance company. There are several types of operating costs, but one of the most important categories includes commissions and acquisition costs, selling costs in usual economic terminology. Not only does this mean that insurance policies must be sold for considerably more than their actuarial value, but it also means there is a great differential among different types of insurance. It is very striking to observe that among health insurance policies of insurance companies in 1958, expenses of one sort or another constitute 51.6 per cent of total premium income for individual policies, and only 9.5 per cent for group policies [26, Table 14-1, p. 272]. This striking differential would seem to imply enormous economies of scale in the provision of insurance, quite apart from the coverage of the risks themselves. Obviously, this provides a very strong argument for widespread plans, including, in particular, compulsory ones.

5. *Predictability and insurance.* Clearly, from the risk-aversion point of view, insurance is more valuable, the greater the uncertainty in the risk being insured against. This is usually used as an argument for putting greater emphasis on insurance against hospitalization and surgery than other forms of medical care. The empirical assumption has been challenged by O. W. Anderson and others [3, pp. 53-54], who asserted that out-of-hospital expenses were equally as unpredictable as in-hospital costs. What was in fact shown was that the probability of costs exceeding $200 is about the same for the two categories, but this is not, of course, a correct measure of predictability, and a quick glance at the supporting evidence shows that in relation to the average cost the variability is much lower for ordinary medical expenses. Thus, for the city of Birmingham, the mean expenditure on surgery was $7, as opposed to $20 for other medical expenses, but of those who paid something for surgery the average bill was $99, as against $36 for those with some ordinary medical cost. Eighty-two per cent of those interviewed had no surgery, and only 20 per cent had no ordinary medical expenses [3, Tables A-13, A-18, and A-19 on pp. 72, 77, and 79, respectively].

The issue of predictability also has bearing on the merits of insurance against chronic illness or maternity. On a lifetime insurance basis, insurance against chronic illness makes sense, since this is both highly unpredictable and highly significant in costs. Among people who already have chronic illness, or symptoms which reliably indicate it, insurance in the strict sense is probably pointless.

6. *Pooling of unequal risks.* Hypothetically, insurance requires for its full social benefit a maximum possible discrimination of risks. Those

in groups of higher incidences of illness should pay higher premiums. In fact, however, there is a tendency to equalize, rather than to differentiate, premiums, especially in the Blue Cross and similar widespread schemes. This constitutes, in effect, a redistribution of income from those with a low propensity to illness to those with a high propensity. The equalization, of course, could not in fact be carried through if the market were genuinely competitive. Under those circumsances, insurance plans could arise which charged lower premiums to preferred risks and draw them off, leaving the plan which does not discriminate among risks with only an adverse selection of them.

As we have already seen in the case of income redistribution, some of this may be thought of as insurance with a longer time perspective. If a plan guarantees to everybody a premium that corresponds to total experience but not to experience as it might be segregated by smaller subgroups, everybody is, in effect, insured against a change in his basic state of health which would lead to a reclassification. This corresponds precisely to the use of a level premium in life insurance instead of a premium varying by age, as would be the case for term insurance.

7. *Gaps and coverage.* We may briefly note that, at any rate to date, insurances against the cost of medical care are far from universal. Certain groups—the unemployed, the institutionalized, and the aged—are almost completely uncovered. Of total expenditures, between one-fifth and one-fourth are covered by insurance. It should be noted, however, that over half of all hospital expenses and about 35 per cent of the medical payments of those with bills of $1,000 a year and over, are included [26, p. 376]. Thus, the coverage on the more variable parts of medical expenditure is somewhat better than the over-all figures would indicate, but it must be assumed that the insurance mechanism is still very far from achieving the full coverage of which it is capable.

D. *Uncertainty of Effects of Treatment*

1. There are really two major aspects of uncertainty for an individual already suffering from an illness. He is uncertain about the effectiveness of medical treatment, and his uncertainty may be quite different from that of his physician, based on the presumably quite different medical knowledges.

2. *Ideal insurance.* This will necessarily involve insurance against a failure to benefit from medical care, whether through recovery, relief of pain, or arrest of further deterioration. One form would be a system in which the payment to the physician is made in accordance with the degree of benefit. Since this would involve transferring the risks from the patient to the physician, who might certainly have an aversion to bearing them, there is room for insurance carriers to pool the risks,

either by contract with physicians or by contract with the potential patients. Under ideal insurance, medical care will always be undertaken in any case in which the expected utility, taking account of the probabilities, exceeds the expected medical cost. This prescription would lead to an economic optimum. If we think of the failure to recover mainly in terms of lost working time, then this policy would, in fact, maximize economic welfare as ordinarily measured.

3. *The concepts of trust and delegation.* In the absence of ideal insurance, there arise institutions which offer some sort of substitute guarantees. Under ideal insurance the patient would actually have no concern with the informational inequality between himself and the physician, since he would only be paying by results anyway, and his utility position would in fact be thoroughly guaranteed. In its absence he wants to have some guarantee that at least the physician is using his knowledge to the best advantage. This leads to the setting up of a relationship of trust and confidence, one which the physician has a social obligation to live up to. Since the patient does not, at least in his belief, know as much as the physician, he cannot completely enforce standards of care. In part, he replaces direct observation by generalized belief in the ability of the physician.[36] To put it another way, the social obligation for best practice is part of the commodity the physician sells, even though it is a part that is not subject to thorough inspection by the buyer.

One consequence of such trust relations is that the physician cannot act, or at least appear to act, as if he is maximizing his income at every moment of time. As a signal to the buyer of his intentions to act as thoroughly in the buyer's behalf as possible, the physician avoids the obvious stigmata of profit-maximizing. Purely arms-length bargaining behavior would be incompatible, not logically, but surely psychologically, with the trust relations. From these special relations come the various forms of ethical behavior discussed above, and so also, I suggest, the relative unimportance of profit-making in hospitals. The very word, "profit," is a signal that denies the trust relations.

Price discrimination and its extreme, free treatment for the indigent, also follow. If the obligation of the physician is understood to be first of all to the welfare of the patient, then in particular it takes precedence over financial difficulties.

As a second consequence of informational inequality between physician and patient and the lack of insurance of a suitable type, the patient must delegate to the physician much of his freedom of choice.

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[36] Francis Bator points out to me that some protection can be achieved, at a price, by securing additional opinions.

He does not have the knowledge to make decisions on treatment, referral, or hospitalization. To justify this delegation, the physician finds himself somewhat limited, just as any agent would in similar circumstances. The safest course to take to avoid not being a true agent is to give the socially prescribed "best" treatment of the day. Compromise in quality, even for the purpose of saving the patient money, is to risk an imputation of failure to live up to the social bond.

The special trust relation of physicians (and allied occuptions, such as priests) extends to third parties so that the certifications of physicians as to illness and injury are accepted as especially reliable (see Section II.B above). The social value to all concerned of such presumptively reliable sources of information is obvious.

Notice the general principle here. Because there are barriers to the information flow and because there is no market in which the risks involved can be insured, coordination of purchase and sales must take place through convergent expectations, but these are greatly assisted by having clear and prominent signals, and these, in turn, force patterns of behavior which are not in themselves logical necessities for optimality.[37]

4. *Licensing and educational standards*. Delegation and trust are the social institutions designed to obviate the problem of informational inequality. The general uncertainty about the prospects of medical treatment is socially handled by rigid entry requirements. These are designed to reduce the uncertainty in the mind of the consumer as to the quality of product insofar as this is possible.[38] I think this explanation, which is perhaps the naive one, is much more tenable than any idea of a monopoly seeking to increase incomes. No doubt restriction on entry is desirable from the point of view of the existing physicians, but the public pressure needed to achieve the restriction must come from deeper causes.

The social demand for guaranteed quality can be met in more than one way, however. At least three attitudes can be taken by the state or other social institutions toward entry into an occupation or toward the production of commodities in general; examples of all three types exist. (1) The occupation can be licensed, nonqualified entrants being simply excluded. The licensing may be more complex than it is in medicine; individuals could be licensed for some, but not all, medical activities, for example. Indeed, the present all-or-none approach could be criticized as being insufficient with regard to complicated specialist treatment, as well as excessive with regard to minor medical skills. Graded licensing may, however, be much harder to enforce. Controls could be exercised analogous to those for foods; they can be excluded as being dangerous, or they can be permitted for animals but not for humans. (2) The state or other agency can certify or label, without compulsory exclusion. The category of Certified Psychologist is now under active discussion; canned goods are graded. Certification can be done by nongovernmental agencies, as in the medical-board examinations for specialists. (3) Nothing at all may be done; consumers make their own choices.

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[37] The situation is very reminiscent of the crucial role of the focal point in Schelling's theory of tacit games, in which two parties have to find a common course of action without being able to communicate; see [24, esp. pp. 225 ff.].

[38] How well they achieve this end is another matter. R. Kessel points out to me that they merely guarantee training, not continued good performance as medical technology changes.

The choice among these alternatives in any given case depends on the degree of difficulty consumers have in making the choice unaided, and on the consequences of errors of judgment. It is the general social consensus, clearly, that the *laissez-faire* solution for medicine is intolerable. The certification proposal never seems to have been discussed seriously. It is beyond the scope of this paper to discuss these proposals in detail. I wish simply to point out that they should be judged in terms of the ability to relieve the uncertainty of the patient in regard to the quality of the commodity he is purchasing, and that entry restrictions are the consequences of an apparent inability to devise a system in which the risks of gaps in medical knowledge and skill are borne primarily by the patient, not the physician.

Postscript

I wish to repeat here what has been suggested above in several places: that the failure of the market to insure against uncertainties has created many social institutions in which the usual assumptions of the market are to some extent contradicted. The medical profession is only one example, though in many respects an extreme one. All professions share some of the same properties. The economic importance of personal and especially family relationships, though declining, is by no means trivial in the most advanced economies; it is based on nonmarket relations that create guarantees of behavior which would otherwise be afflicted with excessive uncertainty. Many other examples can be given. The logic and limitations of ideal competitive behavior under uncertainty force us to recognize the incomplete description of reality supplied by the impersonal price system.

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APPENDIX

On Optimal Insurance Policies

The two propositions about the nature of optimal insurance policies asserted in Section IV.B above will be proved here.

Proposition 1. If an insurance company is willing to offer any insurance policy against loss desired by the buyer at a premium which depends only on the policy's actuarial value, then the policy chosen by a risk-averting buyer will take the form of 100 per cent coverage above a deductible minimum.

Note: The premium will, in general, exceed the actuarial value; it is only required that two policies with the same actuarial value will be offered by the company for the same premium.

Proof: Let W be the initial wealth of the individual, X his loss, a random variable, $I(X)$ the amount of insurance paid if loss X occurs, P the premium, and $Y(X)$ the wealth of the individual after paying the premium, incurring the loss, and receiving the insurance benefit.

$$Y(X) = W - P - X + I(X). \tag{1}$$

The individual values alternative policies by the expected utility of his final wealth position, $Y(X)$. Let $U(y)$ be the utility of final wealth, y; then his aim is to maximize,

$$E\{U[Y(X)]\}, \tag{2}$$

where the symbol, E, denotes mathematical expectation.

An insurance payment is necessarily nonnegative, so the insurance policy must satisfy the condition,

$$I(X) \geq 0 \quad \text{for all} \quad X. \tag{3}$$

If a policy is optimal, it must in particular be better in the sense of the criterion (2), than any other policy with the same actuarial expectation, $E[I(X)]$. Consider a policy that pays some positive amount of insurance at one level of loss, say X_1, but which permits the final wealth at some other loss level, say X_2, to be lower than that corresponding to X_1. Then, it is intuitively obvious that a risk-averter would prefer an alternative policy with the same actuarial value which would offer slightly less protection for losses in the neighborhood of X_1 and slightly higher protection for those in the neighborhood of X_2, since risk aversion implies that the marginal utility of $Y(X)$ is greater when $Y(X)$ is smaller: hence, the original policy cannot be optimal.

To prove this formally, let $I_1(X)$ be the original policy, with $I_1(X)>0$ and $Y_1(X_1)>Y_2(X_2)$, where $Y_1(X)$ is defined in terms of $I_1(X)$ by (I). Choose δ sufficiently small so that,

(4) $I_1(X) > 0 \quad \text{for} \quad X_1 \leq X \leq X_1 + \delta,$

(5) $Y_1(X') < Y_1(X) \quad \text{for} \quad X_2 \leq X' \leq X_2 + \delta, \quad X_1 \leq X \leq X_1 + \delta.$

(This choice of δ is possible if the functions $I_1(X)$, $Y_1(X)$ are continuous; this can be proved to be true for the optimal policy, and therefore we need only consider this case.)

Let π_1 be the probability that the loss, X, lies in the interval $\langle X_1, X_1+\delta\rangle$, π_2 the probability that X lies in the interval $\langle X_2, X_2+\delta\rangle$. From (4) and (5) we can choose $\epsilon>0$ and sufficiently small so that,

(6) $I_1(X) - \pi_2\epsilon \geq 0 \quad \text{for} \quad X_1 \leq X \leq X_1 + \delta,$

(7) $Y_1(X') + \pi_1\epsilon < Y_1(X) - \pi_2\epsilon$

$\qquad \text{for} \quad X_2 \leq X' \leq X_2 + \delta, \quad X_1 \leq X \leq X_1 + \delta.$

Now define a new insurance policy, $I_2(X)$, which is the same as $I_1(X)$ except that it is smaller by $\pi_2\epsilon$ in the interval from X_1 to $X_1+\delta$ and larger by $\pi_1\epsilon$ in the interval from X_2 to $X_2+\delta$. From (6), $I_2(X)\geq 0$ everywhere, so that (3) is satisfied. We will show that $E[I_1(X)]=E[I_2(X)]$ and that $I_2(X)$ yields the higher expected utility, so that $I_1(X)$ is not optimal.

Note that $I_2(X)-I_1(X)$ equals $-\pi_2\epsilon$ for $X_1\leq X\leq X_1+\delta$, $\pi_1\epsilon$ for $X_2\leq X\leq X_2+\delta$, and 0 elsewhere. Let $\phi(X)$ be the density of the random variable X. Then,

$$
\begin{aligned}
E[I_2(X) - I_1(X)] &= \int_{X_1}^{X_1+\delta}[I_2(X) - I_1(X)]\phi(X)dX \\
&\quad + \int_{X_2}^{X_2+\delta}[I_2(X) - I_1(X)]dX \\
&= (-\pi_2\epsilon)\int_{X_1}^{X_1+\delta}\phi(X)dX + (\pi_1\epsilon)\int_{X_2}^{X_2+\delta}\phi(X)dX \\
&= -(\pi_2\epsilon)\pi_1 + (\pi_1\epsilon)\pi_2 = 0,
\end{aligned}
$$

so that the two policies have the same actuarial value and, by assumption, the same premium.

Define $Y_2(X)$ in terms of $I_2(X)$ by (1). Then $Y_2(X)-Y_1(X)=I_2(X)-I_1(X)$. From (7),

(8) $Y_1(X') < Y_2(X') < Y_2(X) < Y_1(X)$

$\qquad \text{for} \quad X_2 \leq X' \leq X_2 + \delta, \quad X_1 \leq X \leq X_1 + \delta.$

Since $Y_1(X)-Y_2(X)=0$ outside the intervals $\langle X_1, X_1+\delta\rangle$, $\langle X_2, X_2+\delta\rangle$, we can write,

$$(9)\quad E\{U[Y_2(X)] - U[Y_1(X)]\} = \int_{X_1}^{X_1+\delta}\{U[Y_2(X)] - U[Y_1(X)]\}\phi(X)dX + \int_{X_2}^{X_2+\delta}\{U[Y_2(X)] - U[Y_1(X)]\}\phi(X)dX.$$

By the Mean Value Theorem, for any given value of X,

$$(10)\quad U[Y_2(X)] - U[Y_1(X)] = U'[Y(X)][Y_2(X) - Y_1(X)] = U'[Y(X)][I_2(X) - I_1(X)],$$

where $Y(X)$ lies between $Y_1(X)$ and $Y_2(X)$. From (8),

$$Y(X') < Y(X) \quad \text{for} \quad X_2 \leq X' \leq X_2 + \delta, \quad X_1 \leq X \leq X_1 + \delta,$$

and, since $U'(y)$ is a diminishing function of y for a risk-averter,

$$U'[Y(X')] > U'[Y(X)]$$

or, equivalently, for some number u,

$$(11)\quad \begin{aligned} U'[Y(X')] &> u \quad \text{for} \quad X_2 \leq X' \leq X_2 + \delta, \\ U'[Y(X)] &< u \quad \text{for} \quad X_1 \leq X \leq X_1 + \delta. \end{aligned}$$

Now substitute (10) into (9),

$$E\{U[Y_2(X)] - U[Y_1(X)]\} = -\pi_2\epsilon \int_{X_1}^{X_1+\delta} U'[Y(X)]\phi(X)dX + \pi_1\epsilon \int_{X_2}^{X_2+\delta} U'[Y(X)]\phi(X)dX.$$

From (11), it follows that,

$$E\{U[Y_2(X)] - U[Y_1(X)]\} > -\pi_2\epsilon u\pi_1 + \pi_1\epsilon u\pi_2 = 0,$$

so that the second policy is preferred.

It has thus been shown that a policy cannot be optimal if, for some X_1 and X_2, $I(X_1) > 0$, $Y(X_1) > Y(X_2)$. This may be put in a different form: Let $Y_{\min}$ be the minimum value taken on by $Y(X)$ under the optimal policy; then we must have $I(X) = 0$ if $Y(X) > Y_{\min}$. In other words, a minimum final wealth level is set; if the loss would not bring wealth below this level, no benefit is paid, but if it would, then the benefit is sufficient to bring up the final wealth position to the stipulated minimum. This is, of course, precisely a description of 100 per cent coverage for loss above a deductible.

We turn to the second proposition. It is now supposed that the insurance company, as well as the insured, is a risk-averter; however, there are no administrative or other costs to be covered beyond protection against loss.

Proposition 2. If the insured and the insurer are both risk-averters and there are no costs other than coverage of losses, then any nontrivial Pareto-optimal policy, $I(X)$, as a function of the loss, X, must have the property, $0<dI/dX<1$.

That is, any increment in loss will be partly but not wholly compensated by the insurance company; this type of provision is known as coinsurance. Proposition 2 is due to Borch [9, Sec. 2]; we give here a somewhat simpler proof.

Proof: Let $U(y)$ be the utility function of the insured, $V(z)$ that of the insurer. Let W_0 and W_1 be the initial wealths of the two, respectively. In this case, we let $I(X)$ be the insurance benefits less the premium; for the present purpose, this is the only significant magnitude (since the premium is independent of X, this definition does not change the value of dI/dX). The final wealth positions of the insured and insurer are:

$$\begin{aligned} Y(X) &= W_0 - X + I(X), \\ Z(X) &= W_1 - I(X), \end{aligned} \tag{12}$$

respectively. Any given insurance policy then defines expected utilities, $u=E\{U[Y(X)]\}$ and $v=E\{V[Z(X)]\}$, for the insured and insurer, respectively. If we plot all points (u, v) obtained by considering all possible insurance policies, the resulting expected-utility-possibility set has a boundary that is convex to the northeast. To see this, let $I_1(X)$ and $I_2(X)$ be any two policies, and let (u_1, v_1) and (u_2, v_2) be the corresponding points in the two-dimensional expected-utility-possibility set. Let a third insurance policy, $I(X)$, be defined as the average of the two given ones,

$$I(X) = (\tfrac{1}{2})I_1(X) + (\tfrac{1}{2})I_2(X),$$

for each X. Then, if $Y(X)$, $Y_1(X)$, and $Y_2(X)$ are the final wealth positions of the insured, and $Z(X)$, $Z_1(X)$, and $Z_2(X)$ those of the insurer for each of the three policies, $I(X)$, $I_1(X)$, and $I_2(X)$, respectively,

$$\begin{aligned} Y(X) &= (\tfrac{1}{2})Y_1(X) + (\tfrac{1}{2})Y_2(X), \\ Z(X) &= (\tfrac{1}{2})Z_1(X) + (\tfrac{1}{2})Z_2(X), \end{aligned}$$

and, because both parties have diminishing marginal utility,

$$\begin{aligned} U[Y(X)] &\geq (\tfrac{1}{2})U[Y_1(X)] + (\tfrac{1}{2})U[Y_2(X)], \\ V[Z(X)] &\geq (\tfrac{1}{2})V[Z_1(X)] + (\tfrac{1}{2})V[Z_2(X)]. \end{aligned}$$

Since these statements hold for all X, they also hold when expectations are taken. Hence, there is a point (u, v) in the expected-utility-possibility set for which $u\geq(\frac{1}{2})u_1+(\frac{1}{2})u_2$, $v\geq(\frac{1}{2})v_1+(\frac{1}{2})v_2$. Since this statement holds for every pair of points (u_1, y_1) and (u_2, v_2) in the expected-utility-possibility set, and in particular for pairs of points on the northeast boundary, it follows that the boundary must be convex to the northeast.

From this, in turn, it follows that any given Pareto-optimal point (i.e., any point on the northeast boundary) can be obtained by maximizing a linear function, $\alpha u+\beta v$, with suitably chosen α and β nonnegative and at least one positive, over the expected-utility-possibility set. In other words, a Pareto-optimal insurance policy, $I(X)$, is one which maximizes,

$$\alpha E\{U[Y(X)]\} + \beta E\{V[Z(X)]\} = E\{\alpha U[Y(X)] + \beta V[Z(X)]\},$$

for some $\alpha \geq 0$, $\beta \geq 0$, $\alpha > 0$ or $\beta > 0$. To maximize this expectation, it is obviously sufficient to maximize:

$$\alpha U[Y(X)] + \beta V[Z(X)], \tag{13}$$

with respect to $I(X)$, for each X. Since, for given X, it follows from (12) that,

$$dY(X)/dI(X) = 1, \qquad dZ(X)/dI(X) = -1,$$

it follows by differentiation of (13) that $I(X)$ is the solution of the equation,

$$\alpha U'[Y(X)] - \beta V'[Z(X)] = 0. \tag{14}$$

The cases $\alpha = 0$ or $\beta = 0$ lead to obvious trivialities (one party simply hands over all his wealth to the other), so we assume $\alpha > 0$, $\beta > 0$. Now differentiate (14) with respect to X and use the relations, derived from (12),

$$dY/dX = (dI/dX) - 1, \qquad dZ/dX = -(dI/dX).$$

$$\alpha U''[Y(X)][(dI/dX) - 1] + \beta V''[Z(X)](dI/dX) = 0,$$

or

$$dI/dX = \alpha U''[Y(X)]/\{\alpha U''[Y(X)] + \beta V''[Z(X)]\}.$$

Since $U''[Y(X)] < 0$, $V''[Z(X)] < 0$ by the hypothesis that both parties are risk-averters, Proposition 2 follows.

FACTOR PRICES, PRODUCTIVITY, AND ECONOMIC GROWTH

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The recent publication by the National Bureau of Economic Research of consistent estimates of real private product, labor and capital inputs, and factor incomes, prices, and productivity furnishes the statistical basis for analysis in this article of the trends and interrelationships of these variables in the course of U. S. economic development, 1919-60.[1] In particular, the trends in the real prices of the factors and in the quantities of factor services supplied are of interest as determinants of trends in the real incomes of the owners of the factors. To go a step further, changes in the relative prices of labor and capital and in the relative quantities of the factor inputs determine the changes in functional distribution of the national income and influence size distribution. And since the proportion of incomes saved affects the relative growth of the real capital stock and its services, the saving ratio is relevant to factor prices, productivity, and incomes. So also are intangible investment and the associated saving, and the resulting stock of productive capacities, embodied in the tangible factors, which influence the trends in factor productivity as measured. But in the absence of good figures on intangibles, we can only speculate as to their movement and influence.

In the body of the article, we first describe the trends of the key variables in the U. S. private economy, 1919-60, with some reference to the shorter postwar trends, 1948-60. Then we attempt some interpretation and analysis of the trends and interrelationships from several points of view. In Appendix A the precise interrelationships of the variables chosen for study are set forth in mathematical form. Appendix B contains a brief description of the concepts and sources of the estimates, together with tables containing the key variables in index-number form, and their rates of change during the periods 1919-60 and 1948-60. The analysis is confined to the private economy, since this is the sector that reflects the play of market forces, and since, by Commerce Department convention, government product is artificially confined to employee compensation alone.

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[1] See John W. Kendrick [4, especially Ch. 5] and the *42nd Annual Report* of the National Bureau, [14, p. 40] for an updating through 1960 of the productivity estimates.

I. *Summary*

The historical estimates show that there has been little trend in the rate of return on capital over the past 40 years, reflecting a secular standoff between the tendency towards diminishing return to capital and its offsets, primarily technological advance and rising labor input. The relatively constant rate of return has been enough to induce tangible capital formation in excess of the rate of growth of labor input, and a growing intangible investment associated with strong productivity advance. As a result of the increasing relative abundance of capital and the constant rate of return, the real wage rate has risen in excess of the growth of labor productivity, resulting in the capture by labor of almost the entire productivity increment. As a result, the labor share of income has risen, while investment return remained sufficient for continuing growth of the stocks of tangible and intangible capital. There have, of course, been continuing adjustment problems over shorter time periods.

It is sometimes asked if the trends noted are equitable—should labor get all the productivity increment and a rising share of income? One can always reply that economics is neutral on normative questions. But it might be pointed out that the outcome does seem to have been operationally efficient. It is true that a constant rate of return to capital means that property holders do not share the productivity increment. But the rate of return seems to have been sufficient to induce a relatively high rate of total saving and investment and consequent growth of capital stock and productivity. Further, if property returns are largely reinvested, property owners enjoy a rise in their income, even with a constant rate of return. And the rising share of labor in the national income and the associated movement towards somewhat greater equality of the size-distribution of income have probably made somewhat easier the task of finding an adequate market for our increasing productive potential and thus the maintenance of relatively full employment.

The estimates provided in this paper of an elasticity of substitution well under unity support the view that the conventional Cobb-Douglas production function is not an adequate description of the U. S. economy in recent decades. In a concluding section of the paper, we discuss an alternative production function that allows for change in the factor shares in the course of economic growth. A graphic presentation of the rates of growth in the key U. S. macroeconomic variables for recent decades is appended.

II. *Major Trends*

A. *Total Real Factor Income and Input*

The basic economic theorem that the real income and real product of an economy are equal merely says that a nation may consume or add to stock that which it produces. The two are also equal in current as well as constant dollars by the Commerce concept when net national product is valued at factor cost (i.e., exclusive of indirect business taxes less net subsidies).

Since real income and product are equal, it follows that real income per unit of factor input changes in proportion to changes in total factor productivity, defined as the ratio of real product to the sum of real tangible factor inputs. An increase in total factor productivity is distributed to the owners of the factors by means of an increase in average factor prices proportionately greater than the increase in average product prices (unit factor costs). That is, proportionate movements of the ratio of average factor prices to average product prices equal proportionate movements in total factor productivity. This must be so since the factor price index is obtained as the ratio of current-dollar product (income) to real product, and total factor productivity is the ratio of real product to real factor input.[3]

The proposition that real income per unit of factor input can rise only in proportion to total factor productivity is fundamental, but it has less interest than a comparison of real income, productivity, and prices of each of the broad factor classes, human and nonhuman capital—or "labor" and "capital" as we shall call them for short. That is, if the average prices of the two factor input classes show differing movements, changes in the real income per unit of each will differ; and if the real income of each moves differently from the corresponding "partial" productivity ratios (output/labor and output/capital), factor shares of income will change. It is to an analytical description of these relationships in recent U. S. economic history that we now turn. The analysis is confined to the two major factor classes, but the same framework could be used for multifactor analysis. There is special interest in the two-factor analysis per se, however, due to the relevance of the related functional distribution of income to size distributions and to the importance of the real price of capital in the saving and investment decisions of the community which react back on the relative input quantities and prices of the factors of production.

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[3] Using and supplementing the symbols given in Appendix A:

Y^* = income and product in current prices

Y = income and product in constant prices

$f(K, L)$ = total factor input (capital and labor)

P_Y = product prices $= Y^*/Y$

P_f = factor prices $= Y^*/f$

T = total factor productivity $= Y/f$

So $Y^*/f \div Y^*/Y = Y/f = P_f/P_Y = T$.

B. *Relative Factor Prices*

Over the long period, 1919-60, the real price of labor (real labor compensation per hour) in relation to the real price of capital increased by about 1.8 per cent a year, on average. Real average hourly labor compensation alone increased by 2.6 per cent a year, while the real price of capital rose by 0.7, on average over the period as a whole.

The price of capital (i.e., the cost per unit obtained as the quotient of capital compensation and real capital stock) has two main components: the general price level and the rate of return on capital.[3] The average price level of capital goods rose significantly more than the general price level over the period 1919 to 1960, while the rate of return on capital assets showed little or no trend.

Of course the rate of return fluctuated widely over the short business cycle, and to a mild extent from one cycle peak or cycle average to the next, describing what Kuznets would call a "long swing." Between the postwar cycle peaks of 1948 and 1960, the real price of capital fell by about 2 per cent a year on average. This was an unusually sharp drop in an intermediate-term period, reflecting capital shortages, buoyant demand and abnormally high rates of return in the early postwar period, and the gradual reversal of these conditions during the 1950's.

Real average hourly labor compensation showed a larger than average increase of 3.6 per cent a year between 1948 and 1960. Thus, the relative real price of labor rose at an average annual rate of 5.5 per cent a year 1948-60, triple its long-term average.

The average rate of return on capital over the entire period was sufficient to induce enough net investment to cause the real stock of tangible capital to grow 1 per cent a year faster than real labor input, on average—by 1.8 per cent compared with 0.8 per cent. The relative

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[3] This can easily be seen in the following formulation, in which K is the real capital stock, P_Y the average prices, π the rate of return on the capital stock in current prices (KP_Y), and C is the current-dollar compensation of capital:

$$\frac{C}{K} = \frac{\pi K P_Y}{K} = \pi P_Y.$$

growth of capital was much greater in the 1948-60 period—3.4 per cent compared with 0.5 per cent, at average annual rates.

C. *Relative Factor Productivity*

Given the average annual rate of growth of real net product (at factor cost) of around 3.2 per cent during both 1919-60 and 1948-60, it is plain that the average output-labor ratio ("labor productivity") increased faster than the average output-capital ratio ("capital productivity") especially during the postwar period. Average labor productivity increased by 2.4 per cent a year over the long period and by 2.8 per cent in the postwar period, on average, compared with increases in capital productivity of 1.3 and —0.1 per cent over the longer and shorter periods. When the two "partial productivity" ratios are combined, the resulting measure of "total tangible factor productivity" shows an increase of approximately 2.1 per cent a year, on average, over both the long period and the shorter postwar period. The total productivity measure is gotten by relating real product to both factor inputs, weighting each by its base-period price, which is assumed to approximate its marginal product at that time (see Mathematical Appendix A, equation 4). This is the same as weighting index numbers of each partial productivity ratio by base-period shares of income.[4] The combined or total productivity measure reflects the *net* saving of tangible inputs per unit of output, and thus the increase in productive efficiency due to intangible investments and changing organization of the economy and its productive units. Each of the partial productivity ratios reflects not only changes in efficiency but also factor substitutions.

D. *Real Unit Factor Costs*

A relationship of interest between factor prices and the partial productivity ratios comes from an analysis of change in unit factor cost (price). Factor cost per unit of output is the sum of unit labor cost and unit capital cost. Each type of factor cost can be further decomposed into factor price and the corresponding partial productivity ratio. (See definitions of α and β at the beginning of Appendix A.) Holding unit factor cost constant between two periods, the real unit cost of each factor remains constant only if the change in its real price is exactly offset by a corresponding change in its average productivity. If the increase in real price of one factor exceeds its productivity increase, this discrepancy must be offset by an opposite divergence between the real price and productivity of the other factor (with due allowance for the relative weights—factor shares—of each in the base period).

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[4] If G_T = the rate of technical progress or the rate of total productivity increases, G_k = the rate of growth of the output-capital ratio, or the rate of increase in the partial productivity ratio of capital, and G_l = the rate of growth of the output-labor ratio, or the rate of increase in the partial productivity ratio of labor, then $G_T = \alpha G_k + \beta G_l$, where α and β are the weighting index numbers of the shares of capital and labor respectively. (See equation 7 of Appendix A.)

Thus, over the 41-year period, a 2.6 average annual rate of increase in the real price of labor exceeded the increase in labor productivity of 2.4 per cent by 0.2 percentage point; while the increase in the real price of capital of 0.7 per cent a year fell short of the 1.3 per cent increase in capital productivity by 0.6 percentage point. The 0.2 per cent rise in real unit labor cost when weighted by the 0.75 average share of labor is exactly offset by the 0.6 per cent a year decline in unit capital cost weighted by the 0.25 average capital share of national income.[5]

The picture was even more favorable to labor during the postwar period 1948-60. An increase in real average hourly earnings of 3.6 per cent a year exceeded the increase in labor productivity of 2.8 per cent by 0.8 percentage point. The real price of capital dropped by 1.8 per cent a year from the high 1948 level, whereas capital productivity edged down slightly—by 0.1 per cent a year. The 0.8 per cent a year rise in real unit labor cost, weighted by 0.75, approximately equals the 1.7 per cent annual decrease in unit property costs weighted by the factor of 0.25. The substantial decrease in the rate of return on capital 1948-60 was somewhat abnormal, representing a movement from a high rate in the early postwar to a rate in 1960 that was associated with a less-than-full-employment volume of new investment.

To put it somewhat differently—with more relevance for appraisal of the labor productivity criterion for noninflationary wage increases: real average hourly labor compensation can be raised by *more or less* than the increase in labor productivity to the extent of the difference between the rates of change in the output-capital ratio and the real price of capital, weighted by the ratio between the capital and labor shares of income. Thus, the 0.2 percentage-point difference between the rates of increase of real wages and labor productivity is equal to the 0.6 percentage-point difference between the rates of change in capital productivity and the real price of capital reduced by the .25/.75 average income shares of capital and labor.[6]

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[5] This can be shown mathematically as $\alpha G_\alpha + \beta G_\beta = 0$, where G_α and G_β are the percentage changes of the shares of capital and labor respectively. (See equation 13.)

[6] Using the definitions of G_l, G_k, α, and β, the following relation holds:

$$G_w = G_l + \frac{\alpha}{\beta}(G_k - G_r)$$

where G_w and G_r are the percentage changes of the wage rate and the return on capital. (See equation 8.)

E. *Factor Shares of Income*

The lack of pronounced trend in the rate of return on capital over the long run is another way of saying that the total real compensation of capital grew almost in proportion to the real stock of capital. This means that virtually all of the "productivity increment" accrued to labor—when the productivity increment is defined as the difference in a given period between real factor input and net product valued at base-period factor prices and net product prices respectively. If labor obtains a larger proportion of the productivity increment than its income share in the base period, then obviously the labor share of national income is rising.

The rates of change in the income shares of the factors can be quantified in a number of alternative ways. The first has already been introduced in the preceding section: the rates of change in the factor shares can be viewed as the rates of change in the unit real factor costs. Thus, the labor share of national income rose from 72 per cent in 1919 to 77.8 per cent in 1960—an average annual rate of increase of 0.2 per cent, equal to that of unit real labor cost; the drop of the capital share from 28 to 22.2 per cent represents an average annual rate of decline of 0.6 per cent, equal to the rate of decline in unit real capital cost (see Appendix B, Table 1, columns 8 and 9, and Appendix A, equation 13).

A more usual approach is to explain changes in income shares as the product of changes in the relative quantities of input and in the relative prices of the factors. Thus, the average annual rate of decline in labor input relative to total input (a weighted average of labor and capital inputs) was 0.3 per cent 1919-60, while the price of labor rose by 0.5 per cent a year, on average, in relation to the weighted average of the two factor prices. The sum of the two figures, 0.2 per cent, is the rate of increase in the labor share. The corresponding numbers with respect to rates of change in relative capital input and price are 0.8 and -1.4, which explain the 0.6 per cent a year decline in the capital share (see equations 16 and 17).

Since the rate of relative decline in labor input was more than offset by the rate of relative increase in the price of labor, it is apparent that the elasticity of substitution was less than 1. In fact, it was approximately 0.58 over the long period. This can be obtained from the rounded estimates cited in the previous paragraph,

$$\frac{0.3}{0.5} = 0.6 \quad \text{(see Appendix A, footnote 19),}$$

or as the difference between the growth rate of capital and labor inputs, divided by the difference between the growth rates of the real prices of labor and capital (Appendix A, equation 11a):

$$\frac{1.84 - 0.76}{2.56 - 0.71} = 0.58.^{7}$$

The rates of change in the factor shares can also be expressed in terms of the elasticity of substitution (Appendix A, equations 14 and 15). Thus, the growth of the labor share can be viewed as the difference between the rates of growth of real labor and capital inputs (0.76−1.84 = −1.08) times 1 minus the reciprocal of the elasticity of substitution

$$\left(1 - \frac{1}{0.58} = -0.72\right),$$

weighted by the share of capital in income, 0.25. The result is 0.2.[8] The rate of change in the capital share may be obtained as follows (equation 15): −0.6=0.75 (−0.72) (1.08).

F. *Saving and Capital Growth*

The growth of population, labor force, and labor input can be regarded as a largely exogenous factor. But the growth of the real tangible capital stock—which influences both relative factor prices, productivity, and thus factor shares—is a function of rates of change in saving ratios and real product, or real investment (see equation 20).

The ratio of real net saving and investment to real net product at factor cost showed a trend rate of decline of approximately 0.6 per cent a year over the long period. This decline is consistent with the declining capital coefficient (rising capital productivity) given the average trend rate of growth of real product. (See equation 18, and Appendix B, Table 1, columns 23, 25, and 5.)

The average annual rate of decline in the saving rate (0.59) falls short of the rate of increase in the output-capital ratio (1.37) by the difference between the rates of growth in real net investment and real capital stock (2.54 and 1.75 per cent, respectively).[9] The faster growth of real net investment than of capital stock was associated with the accelerated rate of growth of capital in the post-World War II period compared with the period as a whole, or even with the 1920's.

Since the growth of capital stock and input depend on the saving

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[7] If σ is the elasticity of substitution, and G_K and G_L the growth rates of capital and labor, then

$$\sigma = \frac{G_K - G_L}{G_w - G_r}.$$

[8] $G_\beta = \alpha\left(1 - \frac{1}{\sigma}\right)(G_L - G_K)$.

[9] $G_s = G_I - G_K - G_k$, where $G_s =$ the percentage change of the saving ratio and $G_I =$ the growth rate of investment. (See equation 18.)

ratio and consequent growth of real net investment, these variables can be used instead of real capital in some of the previous relationships we have examined. Alternative equations for rates of change in factor shares are developed in Appendix A (equations 24 and 25).

Despite the declining saving ratio, capital formation was large enough to cause the real stock of capital to grow substantially faster than the labor force, as already quantified. It should be further noted, however, that this continuing relative growth of capital was associated with a marked deceleration in the growth of labor input after World War I. If labor input had continued to grow at its pre-1919 rate of over 2 per cent a year, it is questionable if capital accumulation after 1919 would have been sufficient to provide a deepening of capital in relation to labor which explains part of the growth of real labor income.

III. *Interpretation*

These estimates of the trends and interrelationship of some of the chief macroeconomic variables in the course of recent U. S. economic development offer a fascinating scope for explanatory hypotheses.

A. *The Rate of Return*

In the first place, the long-run relative constancy of the rate of return on capital suggests the operation of an important servomechanism. In the absence of technological advance and labor force growth, the accumulation of capital would tend to push down the rate of return. This tendency was apparently offset by technological advance and growth of labor input, on balance. But in periods when the latter forces were strong enough to raise the rate of return, this apparently induced an increasing volume of saving and investment which eventually drove the return back down. Conversely, when the rate of return fell below a percentage adequate to induce a full-employment level of income (as appears to have happened by 1960), a slowdown in capital accumulation reduced the capital-output ratio at full-employment income below its trend level, and set the stage for an increase in the rate of return when demographic and technological forces became favorable enough to produce full-employment demand.

Some mechanism of this sort would seem to underlie the long swing under intensive study by Abramovitz and Kuznets although much further research is necessary. From the viewpoint of secular growth study, the important point is that relative constancy of the long-term rate of return means that most of the productivity increment accrues to labor. The social implications of this tendency with regard to the viability of capitalism are clear.

B. *The Noninflationary Wage Increase*

If a constant long-run rate of return were accompanied by a constant output-capital ratio, the shares of the factors would remain unchanged. The only sustainable means whereby labor may obtain an increase in the output-labor ratio (and thus an increase in the income share) is by technological advances that save capital as well as labor. Over shorter periods a decline in the rate of return—such as occurred during the 1948-60 period both absolutely and in relation to the output-capital ratio—also made possible a faster increase in real wages than in labor productivity. This possibility—and long-term tendency—is of interest particularly in connection with the attempt by the Council of Economic Advisers to formulate a productivity guidepost for noninflationary wage increases. The guidepost should, theoretically, be formulated with allowance for the anticipated divergence between the output-capital ratio and the rate of return on capital consistent with a full-employment inducement to invest (which was not the case prior to an implicit recognition in the 1962 Report). This divergence is not always positive, and thus an upward adjustment to the "labor productivity" guide is not always warranted. When, for example, the rate of return has been depressed to a level inconsistent with a fully employed economy, the required recovery of the rate of return may mean that the real wage rate must rise somewhat less than labor productivity.[10]

But over the long run (two or more long swings), the tendency of the rate of return to relative constancy means that labor tends to get *all* of the productivity increment, regardless of what happens to the output-capital ratio.[11] Rising "capital productivity," other things equal, means that "total productivity," the productivity increment, and real wages all increase more than would otherwise be the case.

The rising output-capital ratio, in conjunction with the constant rate of return, also means that the capital share of national income decreases. Only if the real price of a factor increases proportionately with the corresponding partial productivity ratio does its income share remain constant.

C. *Productivity Advance and Intangible Investment*

The decline in the net saving ratio is often taken at face value, and ascribed in large part to the increasing tax bite out of income. Undoubtedly, the increasing tax ratio reduced saving below the levels it would have reached with a constant tax ratio. Elsewhere, it has been suggested, however, that despite the larger tax bite, the declining saving ratio holds only if the definition of saving is limited to tangible capital.[12]

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[10] The practicality of a productivity guideline for noninflationary wage increases is a different issue; here, we merely discuss the formulation.

[11] See Kendrick [4, Ch. 5] in which it is estimated that 99 per cent of the productivity increment 1919–57 accrued to labor.

If the saving that is associated with intangible investments in research and development and in persons—particularly for health, education and training—are included with conventionally defined saving and investment, it is doubtful if this broader net saving ratio fell at all. Intangible investments appear to be the chief factor behind rising productivity, leaving aside scale economies. The acceleration in productivity advance after 1919 despite the decline in the net tangible investment ratio indirectly confirms other rough evidence that the intangible investment ratio grew substantially after 1919.

The associated increases in capital and labor productivity meant a faster increase in the relative real price of labor than in the period before 1919—enough so to produce the rising share of labor in national income, since the capital-labor ratio showed no acceleration.

If the tangible investment (saving) ratio declined to the point where capital grew less rapidly than labor, this would probably mean an increasing rate of return and at least a temporary reversal in the rising labor share of income (such as occurred in the 1941-46 period). This reverse tendency would be aggravated if intangible investment and saving also declined in relation to income, since this would tend to reduce the rate of growth of labor productivity, which would further dampen the relative increase in the real price of labor.

Once again, a rising rate of return on capital could be expected to set in motion forces that would tend to reverse the direction of the saving ratio. A more rapid growth of the real stocks of both tangible and intangible capital would tend to restore the underlying trends which we have described and attempted to rationalize.

D. *A Production Function for the U. S. Economy*

The following production function would seem to be appropriate for the U. S. economy in recent decades (equation 28):

$$Y = A_0 e^{0.021t} \frac{KL}{(aL^{2/3} + bK^{2/3})^{3/2}}$$

where a and b are the production elasticities in the Cobb-Douglas function (see equation 27).[13] This function differs from the traditional Cobb-Douglas type in two respects. First, the shares of capital and labor are no longer constant when capital and labor grow at different rates. Thus, the factor shares can vary depending upon which factor is growing faster. Secondly, the rate of change in the profit rate is no longer equal to the rate of change in the output-capital ratio. Depending upon the proportions of capital and labor, the profit rate can increase more or less than the output-capital ratio. In the same way, changes in the wage rate no longer offset the corresponding changes in the output-labor ratio. Wages can rise more than an increase in the average labor productivity. In short, these differences result from the measured elasticity of substitution being smaller than one, while in the Cobb-Douglas case the elasticity of substitution is well known to be unity. The production function presented above is based on our estimated elasticity of substitution equal to approximately 0.6.[14] Thus, when capital increases faster than labor as seen in the U. S. economy, the share of capital falls. The production function presented here would thus seem to be more realistic under current conditions of growth than the Cobb-Douglas function.

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[12] See Volume 9 of the background papers prepared for the Commission on Money and Credit [13].

[13] This is basically the Arrow-Chenery-Minhas-Solow "CES function" [1, pp. 225–50].

APPENDIX A—MATHEMATICAL RECAPITULATION

A. *Symbols*

Y: national income or net national product at factor cost in real terms
K: real stock of capital goods
L: labor force in man-hours worked by major industries, weighted by base-period average hourly earnings in each
π: rate of return on capital
w: wage rate
C: total capital compensation $= \pi K$
W: total labor compensation $= wL$
α: share of capital in the national income $= \pi(K/Y) = C/Y$
β: share of labor in the national income $= w(L/Y) = W/Y$
k: output-capital ratio $= Y/K$
l: output-labor ratio $= Y/L$
S: amount of saving
s: saving ratio as a percentage of the national income $= S/Y$
I: amount of investment $= dK/dt$
r: marginal rate of substitution $= \pi/w$
σ: elasticity of substitution between capital and labor $0 < \sigma < +\infty$
G_x: growth rate of a variable $x, d \log x/dt$. Thus G_Y is the growth rate of income $= d \log Y/dt$ and so on.
t: time as an independent variable. Whenever it is clear that the independent variable is time, it will be omitted. For example, income Y is a function of time $Y(t)$, but (t) will be omitted because it is obvious.

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[14] Our estimate of 0.58 compares with an estimate by I. B. Kravis [6] of 0.64 and with an estimate of 0.57 by Arrow, Chenery, Minhas, and Solow [1].

B. *Basic Assumptions*

We shall make the following assumptions:

1. All markets are highly competitive in the long run. Thus, actual output is closely related to potential output. It is not necessary to assume that actual or realized output is exactly the same as potential output. The above assumption implies that actual output is a constant fraction of potential output and thus the unemployment rate is constant. This assumption may be justifiable, since we are only concerned with the trend of economic growth, but not with cyclical fluctuations.[15]

2. The marginal productivities of capital and labor are approximately equal to the rate of return on capital and the wage rate respectively:

$$\pi = \frac{\partial Y}{\partial K} \quad \text{and} \quad w = \frac{\partial Y}{\partial L} \cdot$$

3. Let us assume that there is an aggregate production function in the economy. By assuming the production function we merely want to seek a good description of the relations among economic variables. The production function is stable in the long run and it may be expressed in the following equation:

$$Y = T(t)f(K, L) \tag{1}$$

where $T(t)$ is the technological improvement factor and $T(t) \geq 1$. Technical progress is on the average neutral: that is, the technological improvement factor does not enter in determining the shares of factors.[16] In the present study only a constant returns-to-scale function is considered. If the production function is an increasing returns-to-scale function, then it must be the subject of a separate and more ambitious study. We shall define an advance in technology as anything that increases the yield in terms of the real product of a given combination of original factors of production. This definition includes improvements in organization (including scale economies) as well as advances in technique as usually understood. In order to simplify the problem we may assume that a given advance in technology raises the yield of all combinations of production factors in the same proportion. We define technical progress as a process consisting of a series of advances in technique occurring successively in time. Thus, even if the supply of original factors of production were unchanged, the consequence of technical progress would be that the national income steadily increases. $f(K, L)$ represents what the national income would be at any given time if technology were entirely stationary. From equation (1) we derive:

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[15] See R. Sato [9] and R. M. Solow [12, pp. 76–86].

[16] It may be somewhat unrealistic to assume that technical progress is neutral. Some technical advance may be applicable to a large volume of production but not to a small one (or vice versa). Some may be applicable to an economy in which the capital-labor ratio is high. Attempts have been made to include "embodied" technology in the production function, but these attempts are still in the formative stage. Especially when one makes an empirical study such as described in this paper, it is not too unrealistic to assume that technical progress is spread over all modes of production and, thus, on the average it is neutral. For detailed discussions of technical progress and economic growth, see "Symposium on Production Functions and Economic Growth" [15].

$$T(t) = \frac{Y}{f(K, L)}\cdot \tag{2}$$

4. The production function (1) is linear and homogeneous. Thus:

$$Y(t) = \lambda^{-1}T(t)\cdot f(\lambda K, \lambda L), \quad \lambda > 0. \tag{3}$$

In other words, $T(t)$ is independent of the scale of production:

$$T(t) = \frac{\lambda Y}{f(\lambda K, \lambda L)}\cdot$$

From Euler's theorem,

$$T(t) = \frac{Y}{\dfrac{\partial f}{\partial k}K + \dfrac{\partial f}{\partial L}L} \tag{4}$$

where $\frac{\partial f}{\partial k}$ and $\frac{\partial f}{\partial L}$ are the rate of return on capital, and the wage rate respectively, which would have obtained if there were no technical progress for the relevant year. We assume that in the base year $T(0)$ is equal to one. "Total factor productivity" is estimated by equation (4).

5. $f(K, L)$ and $T(t)$ are continuously differentiable with respect to K, L, and t up to the desirable order.

6. The rate of return on capital π will fall if capital increases relatively more than labor does and, on the other hand, the wage rate w will fall if labor increases proportionately more than capital at a given level of technology. Thus:

$$\frac{\partial^2 Y}{\partial K^2} < 0 \quad \text{and} \quad \frac{\partial^2 Y}{\partial L^2} < 0.$$

Assumption 4 implies that the rate of return will rise when labor increases relatively and the wage rate will rise when capital increases relatively. Since Y is continuous,

$$\frac{\partial^2 Y}{\partial K \partial L} = \frac{\partial^2 Y}{\partial L \partial K} > 0.$$

C. *Technical Progress and Productivity Increase*

By differentiating equation (2) with respect to t, we obtain

$$G_T = G_Y - \alpha G_K - \beta G_L. \tag{5}$$

The above equation shows that the rate of increase in total productivity, i.e., the rate of technological improvement, equals the growth rate of the national income reduced by the sum of the growth rates of capital and labor weighted by their respective shares in the national income.

From the homogeneity assumption (assumption 4),

$$Y = \pi K + wL.$$

By taking the total differential of Y, we get:

$$dY = \pi dK + K d\pi + w dL + L dw.$$

Since all variables are functions of time,

$$\frac{dY}{dt} = \pi\frac{dK}{dt} + K\frac{d\pi}{dt} + w\frac{dL}{dt} + L\frac{dw}{dt}\cdot$$

From this we obtain:

$$G_Y = \alpha G_K + \alpha G_\pi + \beta G_L + \beta G_w\cdot$$

By substituting the above equation for equation (5) we obtain:

$$G_T = \alpha G_\pi + \beta G_w. \tag{6}$$

The above result states that the total productivity increase can also be regarded as the weighted mean of the growth rates of the return on capital and the wage rate. The weights to the return on capital and to the wage rate are the shares of capital and labor respectively.

Since $G_k = G_Y - G_K$ and $G_l = G_Y - G_L$, equation (5) may be alternatively written as:

$$G_T = \alpha G_k + \beta G_l. \tag{7}$$

Thus the rate of technical progress is the weighted mean of the rates of growth of the output-capital ratio and the output-labor ratio.

D. *Productivity and Wage Rate*

It is of value to formulate the relationship among variables connected with the growth rate of wages. From equations (6) and (7) the following relation can be readily derived:

$$G_w = G_l + \frac{\alpha}{\beta}(G_k - G_\pi). \tag{8}$$

Thus, average hourly labor compensation can rise percentagewise, by more (or less) than the increase in output per man-hour, to the degree that output per unit of capital input increases more (or less) than the rate of return on capital.[17]

An alternative formulation of the growth of the wage rate may be given in the following way. Since $w = \beta(Y/L)$, we get:

$$G_w = G_\beta + G_Y - G_L.$$

By combining the above equation and equation (5), we obtain:

$$G_w = G_\beta + G_T + \alpha(G_K - G_L). \tag{9}$$

We shall use the above equation in the next section.

E. *Factor Earnings*

From the homogeneity assumption the marginal rate of substitution r

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[17] This was first formulated (verbally) in Kendrick [5].

is a function of the capital-labor ratio alone. Since the capital-labor ratio equals output per labor input l divided by output per capital input k, the marginal rate of substitution r may be written as:

$$r = \frac{\pi}{w} = \phi\left(\frac{l}{k}\right).$$

On the other hand the ratio of the capital share to the labor share α/β is equal to the marginal rate of substitution r multiplied by the capital-labor ratio or l/k and thus,

$$\frac{\alpha}{\beta} = \frac{l}{k}\phi\left(\frac{l}{k}\right).$$

The share ratio α/β depends solely upon the capital-labor ratio l/k. Differentiating the above equation with respect to l/k, we derive,

$$\text{(10)} \qquad \frac{d\left(\frac{\alpha}{\beta}\right)}{d\left(\frac{l}{k}\right)} = r\left(1 - \frac{1}{\sigma}\right)$$

where σ is the elasticity of substitution between capital and labor. Following Hicks, σ is defined as:

$$\text{(11)} \qquad \sigma = \frac{\pi w}{Y\frac{\partial^2 Y}{\partial K \partial L}}.$$

It may also be written as[18]

$$\text{(11a)} \qquad \sigma = \frac{d\left(\frac{l}{k}\right)}{d\left(\frac{1}{r}\right)}\cdot\frac{\frac{1}{r}}{\frac{l}{k}} = \frac{d\left(\frac{K}{L}\right)}{d\left(\frac{w}{\pi}\right)}\cdot\frac{\frac{w}{\pi}}{\frac{K}{L}} = \frac{G_K - G_L}{G_w - G_\pi}.$$

It should be noted that equation (10) is an expression similar to the marginal and average relation in the price theory $[M = P(1 - 1/\xi)]$. Thus

$$-\sigma = \frac{d\left(\frac{l}{k}\right)}{dr}\cdot\frac{r}{\frac{l}{k}}.$$

Thus σ becomes always positive with $\pi > 0$, $w > 0$, and

$$\frac{\partial^2 Y}{\partial K \partial L} > 0.$$

See Hicks [3, pp. 117, 245] and Meade [7]. Meade obtains an expression similar to (11a).

$r=\pi/w$ is considered as an average change in the factor-share ratio and

$$\frac{d\left(\frac{\alpha}{\beta}\right)}{d\left(\frac{l}{k}\right)}$$

as a marginal change.

Expanding

$$\frac{d\left(\frac{\alpha}{\beta}\right)}{d\left(\frac{l}{k}\right)},$$

equation (10) may be written as:

$$\frac{\frac{\alpha}{\beta}(G_\alpha - G_\beta)}{\frac{K}{L}(G_K - G_L)} = \frac{\pi}{w}\left(1 - \frac{1}{\sigma}\right).$$

Since $\alpha=\pi(K/Y)$ and $\beta=w(L/Y)$ the above equation becomes:

$$\text{(12)} \qquad \frac{G_\alpha - G_\beta}{G_K - G_L} = 1 - \frac{1}{\sigma}\,.$$

From $\alpha+\beta=1$ we get:

$$\text{(13)} \qquad \alpha G_\alpha + \beta G_\beta = 0.$$

By combining equations (12) and (13) the rates of growth of the shares of capital and labor are readily expressed as

$$\text{(14)} \qquad G_\alpha = \beta\left(1 - \frac{1}{\sigma}\right)(G_K - G_L),$$

and

$$\text{(15)} \qquad G_\beta = \alpha\left(1 - \frac{1}{\sigma}\right)(G_L - G_K).$$

The above equations show that when $\sigma=1$, G_α and G_β would be zero and hence the shares of capital and labor are constant regardless of any combinations between capital and labor. When $\sigma>1$ and capital grows faster than labor, the share of capital increases but the share of labor decreases. If $\sigma<1$ and capital grows faster than labor, the share of capital decreases, but the share of labor increases.

Using equation (11a), σ is estimated to be approximately 0.58 for the

U. S. economy, 1919–60. Since σ is smaller than one, $G_\alpha<0$ but $G_\beta>0$, as $G_K>G_L$.

It is worth noting that G_α and G_β may be respectively considered as real unit capital cost and real unit labor cost, since α equals C/Y and β is equal to W/Y in real terms.

An alternative formulation of the growth rate of the labor share may be derived from equation (9),

$$G_\beta = [G_L - (\alpha G_K + \beta G_L)] + (G_w - G_T).$$

Since $\alpha G_K+\beta G_L$ equals the growth rate of total input or G_f where

$$G_f = \frac{d \log f(K, L)}{dt},$$

the above equation may be written as:

$$G_\beta = (G_L - G_f) + (G_w - G_T). \tag{16}$$

Thus, the growth rate of the labor share equals the annual rate of change in labor input relative to total input plus the growth rate of wages relative to total productivity change. In the same way the growth rate of the capital share may be alternatively written as[19]

$$G_\alpha = (G_K - G_f) + (G_r - G_T). \tag{17}$$

F. *Changes in the Saving Ratio*

We shall now consider the relationship between the saving ratio and the output-capital ratio. From $G_K=sk$, we obtain:

$$G(G_K) = G_s + G_k.$$

Since $G(G_K)$ is also equal to G_I-G_K, the above relation becomes $G_s+G_k=G_I-G_K$. Thus,

$$G_s = G_I - G_K - G_k. \tag{18}$$

When capital grows at a constant rate, G_I must be equal to G_K or $G(G_K)=0$. This implies that

$$G_s = - G_k. \tag{19}$$

Thus the growth rate of the saving ratio must have the opposite sign to the growth rate of the output-capital ratio but numerically must be the same.

When capital grows at an accelerated rate $[G(G_K)>0]$, G_I must be greater than G_K. The value of G_s may be either positive or negative or equal to zero, depending upon $G_I-G_K\gtreqless G_k$. If, on the other hand, capital grows at a decelerated rate, G_s must be always negative since $G_I<G_K$ and G_k is assumed to be positive.

Next, let us deal with the relations between the saving ratio and the

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[19] The elasticity of substitution σ may also be equal to:

$$\sigma = \frac{G_L - G_f}{G_T - G_w} = \frac{G_K - G_f}{G_T - G_r}.$$

other variables. Under assumptions 1 and 2, saving must be always equal to investment at any point of time.

Thus,

$$\frac{dK}{dt} = I = S = sY.$$

The growth rate of investment becomes $G_I = G_s + G_Y$.

By using equation (5) we eliminate G_Y in the above equation and obtain:

$$G_s = G_I - \alpha G_K - \beta G_L - G_T. \tag{20}$$

First we shall assume that the growth rate of capital G_K is constant. Then, since $G_I = G_K$, equation (20) can be simplified to:

$$G_s = \beta(G_K - G_L) - G_T. \tag{21}$$

In the above equation if G_T is zero, G_s depends solely on the difference between G_K and G_L. If $G_K > G_L$, G_s must be positive and vice versa. When G_T is positive, G_s will not be positive if G_T exceeds the difference between G_K and G_L.

The growth rate of the saving ratio can also be expressed in terms of the elasticity of substitution and other variables. Substituting $G_K - G_L = \sigma(G_w - G_\pi)$ in equation (21) we have:

$$G_s = \beta\sigma(G_w - G_\pi) - G_T.$$

Since $G_T = \alpha G_\pi + \beta G_w$, the equation may be written as

$$G_s = \beta(\sigma - 1)(G_w - G_\pi) - G_\pi. \tag{22}$$

There are three different cases influencing the value of G_s according to the value of σ; (a) $\sigma = 1$, (b) $0 < \sigma < 1$, and (c) $1 < \sigma < +\infty$. In case (a) G_s must be equal to $-G_\pi$ and thus the growth rate of return on capital must be numerically equal to the percentage change in the saving ratio with the sign of G_π being opposite. Since equation (19) must also be equal to equation (22), G_π must be exactly the same as G_k. In the second case, if σ is less than one, most likely G_s will be negative. In case (c), however, G_s may be positive unless G_π is extremely large.

Let us now assume that G_K is not constant. Since G_I is no longer equal to G_K, equation (20) becomes:

$$G_s = (G_I - G_L - G_w) + (1 - \sigma)(G_w - G_T). \tag{23}$$

The sign and magnitude of G_s depends on various factors. If $\sigma = 1$, G_s depends on G_I, G_L, and G_w. Most likely G_s is negative because the combined value of G_L and G_w is usually larger than G_I. If $\sigma \neq 1$, total productivity increase G_T must be taken into account. It all depends on how the economy changes in the long run. In the U. S. economy, since $\sigma = 0.58$, $G_I = 2.54$ per cent, $G_w = 2.56$ per cent, $G_L = 0.8$ per cent, and $G_T = 2.08$ per cent, G_s becomes -0.6 per cent.

Another interesting relation may also be found,

$$G_\alpha = \frac{\beta}{\alpha}\left(\frac{\sigma - 1}{\sigma}\right)(G_I - G_s - G_L - G_T) \tag{24}$$

$$G_\beta = \left(\frac{1 - \sigma}{\sigma}\right)(G_I - G_s - G_L - G_T). \tag{25}$$

The shares of capital and labor now depend partly on the behavior of the saving ratio. In the U. S. economy, as $\sigma < 1$ and as the value in the second parenthesis in (24) is positive, G_α is negative but G_β is positive.

G. *The Production Function of the U. S. Economy*

Since variables necessary to formulate the production function have been included in the previous sections, it might be well to speculate on the shape of the production function appropriate for the U. S. economy. The following production function,

$$Y = \frac{T(t)KL}{(aL^\theta + bK^\theta)^{1/\theta}} \tag{26}$$

has the elasticity of substitution $\sigma = 1/(1+\theta)$ as a parameter.

The production function expressed by equation (26) represents a whole family of linear homogeneous production functions containing a constant elasticity of substitution (CES function).[20] If θ approaches zero, σ becomes one. This can be seen by rearranging the above equation and taking the limit of the function as θ approaches zero. Thus,

$$\log Y = \log T(t) - \frac{\log(aK^{-\theta} + bL^{-\theta})}{\theta},$$

$$\lim_{\theta\to 0} \log Y = \log T(t) - \lim_{\theta\to 0} \frac{d}{d\theta}\left[\log(aK^{-\theta} + bL^{-\theta})\right],$$

$$\lim_{\theta\to 0} \log Y = \log T(t) + a \log K + b \log L,$$

$$Y = T(t)K^a L^b \tag{27}$$

where $a+b=1$.

The elasticity of substitution in the U. S. economy is approximately equal to 0.6. The production function most appropriate to the U. S. economy must now look like

$$Y = A_0 e^{.021t} \frac{KL}{[aL^{2/3} + bK^{2/3}]^{3/2}} \tag{28}$$

since $T(t) = A_0 e^{.01t}$ and $\theta = \frac{2}{3}$. Although we would not go so far as to say that equation (28) is the best representation of the production function of the U. S. economy for the period, the evidence presented in this study strongly suggests that it is a good description.[21] The purpose of this study was not to find the production function, but we arrived at the above conclusion as a by-product of the productivity study.

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[20] A similar function appears in Solow's article [11]. See also Arrow, Chenery, Minhas and Solow [1] and Frankel [2].

H. *A Diagrammatic View of U. S. Economic Growth*

This section gives a diagrammatic illustration of the U. S. economic growth for the past forty years. Obviously it is not practicable to include all the variables in the diagram and, hence, only the growth rates of capital, labor, and income are selected to show their relations with the changes in the saving ratio, the output-capital ratio, and productivity.

The neoclassical theory of growth asserts that in the long run the growth rates of capital and income tend towards equality.[22] In the U. S. economy, however, we cannot observe this tendency. This may be because the saving ratio is consistently declining and because productivity is increasing, which together disturb the long-run equilibrium adjustment.[23] Although capital has grown at an accelerated rate in the post-1948 period, the saving ratio in 1960 is lower than in 1919. A declining saving ratio and an accelerating growth rate of capital (which latter is not identical with the growth rate of income) are together consistent when technology is improving.

Let us demonstrate this in a diagram based upon the following expression for the growth rate of income:

$$G_Y = G_T + \alpha sk + \beta G_L. \tag{29}$$

Assume that G_L and s are independent of the output-capital ratio. For simplicity we use the average annual growth rates of labor and technology.

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[21] An interested reader may test some of the equations set forth in the previous sections. For example

$$\pi = \frac{a}{T_{(t)}{}^{2/3}} k^{5/3}$$

and

$$w = \frac{b}{(T_{(t)})^{2/3}} l^{5/3}$$

$$\alpha = \frac{a}{(T_{(t)})^{2/3}} k^{2/3}$$

and

$$\beta = \frac{b}{(T_{(t)})^{2/3}} l^{2/3}.$$

The growth rates of shares of capital and labor corresponding to equations (14) and (15) are:

$$G_\alpha = -\frac{2}{3}\frac{b}{(T_{(t)})^{2/3}} l^{2/3}(sk - G_L) = -\frac{2}{3}\beta(G_K - G_L)$$

and

$$G_\beta = \frac{2}{3}\frac{a}{(T_{(t)})^{2/3}} k^{2/3}(sk - G_L) = \frac{2}{3}\alpha(G_K - G_L).$$

[22] See, for example, R. M. Solow [11, pp. 70–73] and Sato [8].

[23] Smithies ponders this possibility [10, p. 201].

The diagram depicts how the U. S. economy moved during the period, 1919–60 (Figure 1).

The output-capital ratio k is measured on the abscissa, and the growth rates on the ordinate. The growth rate of capital with the saving ratio in 1919 ($t=0$) is shown by a line s_0k through the origin with a slope equal to s_0(8.5 per cent). The growth rate of labor G_L (shown by k_0C_0), which is independent of the output-capital ratio, equals 0.8 per cent. The line G_{Y0} is obtained by adding $\alpha_0 s_0 k$, β_0, G_L and G_T, where α_0 and β_0 are the initial share of capital and labor respectively ($\alpha_0=28$ per cent and $\beta_0=72$ per cent). G_Y is an increasing function of k because the growth rate of capital is an increasing function in the relevant range. G_Y is convex from below since σ is less than one.[24] The growth rate of income in 1919 is k_0A_0, which is equal to 3.15 per cent, and the growth rate of capital is k_0B_0 and is equal to 1.79 per cent ($s_0=8.5$ per cent and $k_0=0.21$). The output-capital ratio in 1919 is designated by $k_0(0.21)$. s_1k and G_{Y1} represent the growth rates in 1960 ($t=41$), and they were derived in the same way as s_0k_0 and G_{Y0} ($s_1=6.6$ per cent, $k_1=0.38$, $\alpha_1=22$ per cent and $\beta_1=78$ per cent). If the saving ratio and also technology had remained constant, the economy would have moved along the G_{Y0} line and the growth rates of capital and income would have approached together. But since the saving ratio has declined to s_1(6.6 per cent), concomitantly with improving technology (2.1 per cent), the economy moved in the directions of A_1 and B_1. The growth rate of income remained almost constant, while capital has grown at an accelerated rate from B_0 to B_1. During this period the output-capital ratio has increased from $k_0(0.21)$ to $k_1(0.38)$.

It must be pointed out that if the saving ratio had not declined, both the growth rates of capital and income would have been higher than they are

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[24] The diagram is drawn from the production function designated by equation (28). Thus,

$$G_Y = G_T + \frac{a}{(T_{(t)})^{2/3}} sk^{5/3} - \frac{aG_L}{(T_{(t)})^{2/3}} k^{2/3} + G_L.$$

Since G_Y is a function of both t and k, the line G_{Y0} is obtained when $t=0$ and G_{Y_1} when $t=41$.

$$\frac{dG_Y}{dk} = \frac{a}{3(T_{(t)})^{2/3}} k^{-1/3}(5sk - 2G_L)$$

which would be positive or negative, or zero depending upon

$$G_K = sk \gtreqless \frac{2}{5} G_L.$$

Since

$$G_K > \frac{2}{5} G_L,$$

G_Y must be an increasing function of k. The second derivative

$$\frac{d^2G_Y}{dk^2} = \frac{10}{9} \frac{a}{(T_{(t)})^{2/3}} sk^{-1/3} + \frac{2}{9} \frac{a}{(T_{(t)})^{2/3}} G_L k^{-4/3}$$

and, thus, G_Y is convex from below.

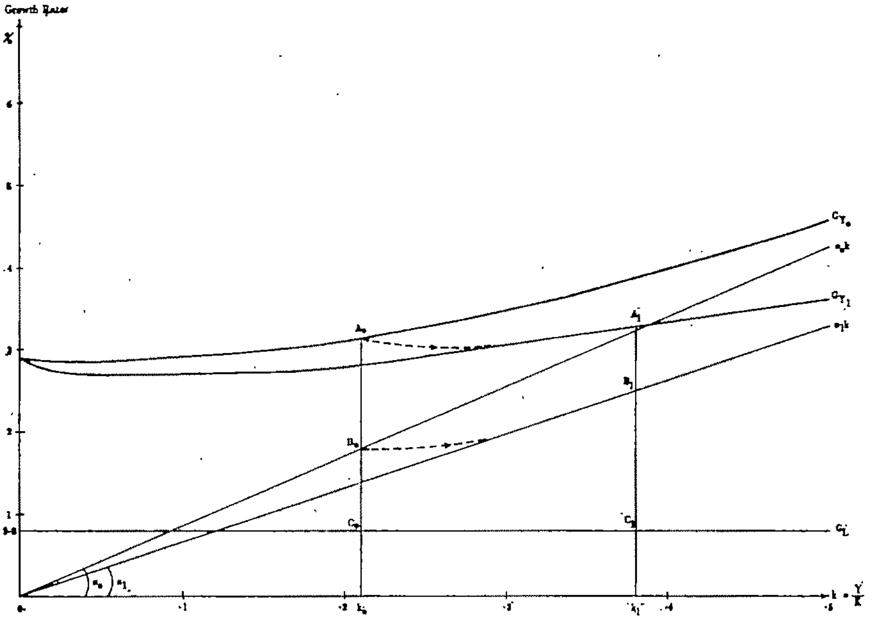

Figure 1. U. S. Economic Growth

now. Also wages would have risen higher, but the rate of return on capital would have fallen if the saving ratio had been constant.

Appendix B—Concepts and Estimates

The estimates on which the analysis was based are contained in the tables below. The basic series are those presented in John W. Kendrick [4], extended to 1960 by the same sources and methods described in that volume. Some of the 1960 estimates were published in the *42nd Annual Report* of the National Bureau of Economic Research [14]. In a few instances, minor revisions have been made in the estimates.

Since the concepts underlying the estimates are discussed in some detail in *Productivity Trends in the United States* [4], they will be summarized but briefly in the following notes. The descriptions of the series below will follow the same numbering as the basic tables. All estimates relate to the private domestic economy—the national economy exclusive of general government and the "rest of the world."

(1) *Total factor income* of the private domestic economy is the sum of the compensation accruing to owners of the services of labor and property employed by private industries and government enterprises of the United States. It is derived from the Commerce national income estimates (extrapolated to 1919 by Kuznets estimates), by substracting income originating in general government and in the rest of the world from the total.

TABLE 1—MAJOR MACROECONOMIC VARIABLES—COLUMNS 1–25 U.S. PRIVATE DOMESTIC ECONOMY, SELECTED YEARS, 1919–60

	(1)	(2) Factor Income	(3)	(4) Implicit Price Deflator (1)÷(5) Ratio	(5)	(6) Real Factor Income	(7)	(8) Distribution of Factor Income	(9)
	Total	Labor	Property		Total	Labor (2)÷(4)	Property (3)÷(4)	Labor (2)÷(1); (6)÷(5)	Property (3)÷(1); (7)÷(5)
	Current dollars (billions)				Constant, 1929 dollars (billions)			Percentages	
1919	60.85	43.82	17.03	1.075	56.60	40.76	15.84	72.0	28.0
1929	82.67	59.75	22.92	1.000	82.67	59.75	22.92	72.3	27.7
1937	66.43	52.40	14.03	0.789	84.19	66.41	17.78	78.9	21.1
1948	205.10	145.93	59.17	1.507	136.07	96.82	39.25	71.2	28.8
1960	365.92	284.60	81.32	1.814	201.71	156.89	44.82	77.8	22.2
Index numbers (1929=100)									
1919	73.6	73.3	74.3	107.5	68.5	68.2	69.1	99.6	101.0
1937	80.4	87.7	61.2	78.9	101.4	111.1	77.6	109.1	76.2
1948	248.1	244.2	258.1	150.7	164.6	162.0	171.3	98.4	104.1
1960	442.6	476.3	354.8	181.4	244.0	262.6	195.6	107.6	80.1
Average annual percentage rates of change									
1948–60	4.94	5.29	2.60	1.58	3.31	3.65	1.11	0.34	−2.21
1919–60	4.47	4.67	3.88	1.28	3.15	3.34	2.57	0.19	−0.57

TABLE 1—(*Continued*)

	(10)	(11) Real Factor Input	(12)	(13)	(14) Factor Price	(15)	(16)	(17) Real Factor Price	(18)
	Total	Labor	Property	Total (1) ÷ (10)	Labor (2) ÷ (11)	Property (3) ÷ (12)	Total (13) ÷ (4)	Labor (14) ÷ (4)	Property (15) ÷ (4)
Index numbers (1929=100)									
1919	84.9	86.7	80.3	86.7	84.6	92.6	80.7	78.7	86.1
1937	88.9	87.4	93.8	90.4	100.3	65.3	114.6	127.1	82.8
1948	112.3	111.9	113.3	220.9	218.2	227.8	146.6	144.8	151.2
1960	129.1	118.5	169.4	342.8	401.9	209.1	189.0	221.6	115.3
Average annual percentage rates of change									
1948–60	1.17	0.48	3.43	3.73	5.22	−2.28	2.14	3.61	−1.8
1919–60	1.03	0.76	1.84	3.41	3.87	2.01	2.08	2.56	0.71

TABLE 1—(*Continued*)

	(19)	(20)	(21)	(22)	(23)	(24)	(25)
	Factor Productivity			Real Net Investment		Real Capital Stock	
	Total (5)÷(10)	Labor (5)÷(11)	Property (5)÷(12)	Billions of 1929 dollars	Per cent of real income (22)÷(5)×100	Billions of 1929 dollars	Ratio to real income (24)÷(5)
1919				4.8	8.48	256.5	4.58
1929				7.0	8.47	331.1	4.04
1937				2.4	2.85	315.4	3.76
1948				13.3	9.78	357.5	2.68
1960				13.4	6.64	521.4	2.62
Index numbers (1929=100)							
1919	80.7	79.0	85.3	68.6	100.1	77.5	113.4
1937	114.6	116.5	108.5	34.3	33.6	95.3	93.1
1948	146.6	147.1	145.3	190.0	115.5	108.0	66.3
1960	189.0	205.9	143.8	191.4	78.4	157.5	64.9
Average annual percentage rates of change							
1948–60	2.14	2.84	−0.10	0.07	−3.28	3.19	−0.19
1919–60	2.08	2.36	1.28	2.54	−0.59	1.75	−1.37

(2) *Labor income* is the compensation of employees (wages and salaries plus "supplements") as estimated by the Commerce Department, plus an imputed compensation for the labor services of proprietors. The latter is obtained by multiplying the estimates of numbers of proprietors in each major industry group by the average compensation per full-time equivalent employee in each, based on Commerce estimates. This is only one of a number of possible ways of splitting the net income of proprietors between the labor and property components, but its general reasonableness is indicated by a comparison with corporate income distribution (see items 8 and 9 below).

(3) *Property compensation,* calculated as the difference between (1) and (2), consists of proprietors and corporation profits plus inventory-valuation adjustment, net interest, rents and royalties.

(4) *The implicit price deflator,* obtained as the quotient of (1) and (5), represents the average market prices of the final goods and services comprising net national product, adjusted to eliminate the effects of indirect business taxes less net subsidies. In other words, it is a weighted average of unit factor costs of the various final goods and services, combined by variable quantity weights. (See Kendrick [4, p. 115] for a comparison of market price and unit factor cost indexes. The trends are very similar.)

(5) *Real factor income* is estimated by extrapolating 1929 total factor income in the private domestic economy by an index of the real net national product originating (*cf. ibid.,* Table A-XXII, last column). In effect, this assumes that unit factor cost weights produce the same movements of aggregate real net product as do market price weights.

(6) and (7) *The real income of each factor* class is obtained as the quotient of the current-dollar income and the implicit price deflator (1929 = 100). This procedure is based on the implicit assumption that labor and property compensations are distributed in equal proportions among consumer goods, saving, and direct taxes. While it is probable that saving and taxes absorb a smaller proportion of labor than of property income, estimates do not exist to permit a precise weighting of price indexes of consumer, net investment, and government purchases for each. Moreover, the deflation of each type of income by the same general price index simplifies the analysis. Complications could be introduced by further deflating the real factor income by the ratio of the "appropriate" price index to the general index, if desired.

(8) and (9) *The distribution of factor income* is obtained by computing the percentages accruing to labor and capital, based on the income breakdown. Despite the imputation procedure required to allocate net proprietors income, the distribution of the total shows much the same relative shift as the distribution of factor income originating in the corporate sector where no imputations are necessary, as indicated in the following table. The lower level of the labor share in the total reflects chiefly the fact that agriculture, which is of negligible importance in the corporate sector, comprises almost half the total number of proprietors, while their imputed average compensation is well below the industry average.

	Labor Share of Factor Income	
	Private Domestic Economy	Corporate Sector
1929	72.3	74.6
1937	78.9	79.9
1948	71.2	74.8
1960	77.8	80.5

The factor shares of income are the same in constant as in current dollars. Thus, the index numbers of the percentage shares are equivalent to index numbers of unit factor costs, (6) ÷ (5) and (7) ÷ (5), and are thus equal to the quotients of real factor prices and factor productivities, (17) ÷ (20) and (18) ÷ (21).

(10) *Total real factor input* is the sum of real labor and capital inputs. It may be thought of as the service-hours made available by the owners of the various factor stocks, combined by the compensation per hour of each. Thus, factor input and factor compensation (cost) are equal in the base period.

(11) *Real labor input* is a weighted aggregate of the man-hours worked in the several industry groups (about 60), combined by the base-period average hourly labor compensation in each. This series rises somewhat faster than an unweighted man-hour aggregate due to the relative shift of workers to higher pay industries.

(12) *Real capital input* is an aggregate of the real capital stock existing in the various industry groups, weighted by the base-period rates of return in each.

(13) *Total factor price,* the quotient of factor income and real input, may also be thought of as an average of the prices of each factor combined by variable input quantity weights. It is the movements of the price ratios indicated by the index numbers that are significant, not the levels.

(14) *Price of labor,* the quotient of labor compensation and labor input, is the mean average hourly labor compensation in the various industries combined by variable man-hour weights.

(15) *Price of property,* the quotient of property (capital) compensation and capital input, in effect is the product of index numbers of capital goods prices and rates of return in the several industries, combined by variable real property input weights.

(16), (17), and (18) *Real factor price* is factor income (total, labor, and property) divided by the implicit price deflator for net national product at factor cost. It represents the purchasing power of factor income per input unit on the assumption that the income of the two major groups is distributed in the same way as the total. As indicated in the text, the real price of capital (property) is its rate of return if the average price of capital goods shows the same movement as the over-all price deflator.

(19) *Total factor productivity,* the quotient of real net product at factor cost (real income) and total real tangible factor input, in effect is a weighted average of the two "partial" productivity ratios. Changes in the total factor

productivity ratio reflect the net saving achieved in the use of factor inputs per unit of output as a result of advances in productive efficiency.

(20) and (21) *Labor and capital productivity* are the quotients of real income (product) and the respective real factor inputs, in index-number form. Each of these "partial" productivity ratios reflects the effects of factor substitutions as well as changing productive efficiency.

(22) and (23) *Real net investment* represents private outlays for new construction, producers' durable equipment and business inventory accumulation, in 1929 dollars, also shown as a percentage of real income or product in the private domestic economy. To ensure consistency, it is estimated as the annual change in the real capital stock (column 24 on a two-year moving average basis to convert from annual averages to end-of-year values).

(24) *The real capital stock* is the total average annual dollar volume, at 1929 prices, of the structures, land, equipment, and inventories in the private domestic economy. The estimates are based largely on those by Goldsmith, and by Kuznets and associates. See [4, Appendix A, and Table IX].

(25) The *"capital coefficient"* is calculated as the ratio of the average annual real capital stock to the annual total real national income or product in the private domestic economy.

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EQUITY YIELDS, GROWTH, AND THE STRUCTURE OF SHARE PRICES

By Burton G. Malkiel*

The first half of 1962 witnessed one of the most precipitous declines in stock market prices in recent history. In terms of the volume of trading and the magnitude of the daily erosion of stock values, one must look back to the crashes of 1929 and 1937 to find parallels. Moreover, the decline was not confined to U. S. stock prices, for a world-wide revaluation of equity values was transmitted from New York.

For most professional financial observers of these developments, two aspects of the decline served as the foci of their analysis. There was first the sharp drop in the *level* of share prices. Second, and perhaps more interesting, there was a marked change in the *structure* of share prices, i.e., the relationships among equities of different characteristics. We can illustrate the revaluation in the level of stock prices by noting that from December 12, 1961, the date of the pre-1962 peak level of stock prices, to June 26, 1962, the date of the 1962 trough, the Dow-Jones Industrial Average, an average of 25 very high quality shares, declined from 734 to 539, a drop of 26.6 per cent. During the same period the Standard and Poor 425 Industrial Stock Average, a broader and more heterogeneous grouping of stocks, declined from 76.69 to 54.80, a drop of 28.5 per cent. This decline of more than 25 per cent we take as illustrative of the drop in the level of stock prices.

Perhaps the most interesting and conspicuous change in the structure of share prices was what the financial press called "the revaluation of 'growth stocks.' " Much steeper price declines were suffered by those companies whose extraordinary earnings growth had gained them far higher price-earnings ratios than those applied to the standard list of industrial stocks. For example, an index of five high-quality growth

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* The author is a graduate student at Princeton University. He is indebted to Professors W. J. Baumol, L. V. Chandler, and R. E. Quandt who assisted through several formative drafts of this paper and made valuable suggestions which led to considerable improvements in the verbal and algebraic presentation of the argument. He has also profited from later useful criticism from Professor F. Machlup and Messrs. J. W. Land and P. A. Tinsley as well as the members of the Princeton University Seminar on Research in Progress. Finally, acknowledgment is made to the National Science Foundation whose grant to the study "Dynamics of the Firm" helped in the completion of this paper.

stocks[1] declined in the same period from 78.60 to 37.60, a drop of 52.2 per cent, almost double that suffered by the standard group. Even the *ne plus ultra* of growth stocks, International Business Machines Corporation, declined in the same period from 580 to 320, a loss of 44.8 per cent.

In the setting of the 1962 market adjustment, this paper will attempt to analyze the relationship of "growth stocks" to standard equities. First we will present a brief historical summary and critique of security analysis since the Great Depression. This is both necessary for explaining how the late 1961 structure of stock prices developed and useful for understanding the types of criteria long used by practical men to value common shares. Next we will construct a model designed to throw light on the "proper" relationships among equity groups that differ in their earnings and dividend growth. Several theorems from the model will be used to demonstrate the precise relationship between warranted share prices and the relevant determinants of valuation. But perhaps even more important than providing a valuation model, the analysis will serve as a conceptual scheme by which we can explain the change in the structure of share prices which occurred.

There is a remarkable similarity between the problems of building a theory of the term structure of interest rates and attempting to formulate the principles underlying the structure of equity prices with respect to issues differing in their growth characteristics. For example, we know that long-term bonds have wider price fluctuations than short issues for a given change in interest rates. Similarly, we shall find that non-dividend-paying growth stocks are inherently more volatile than standard issues in response to a change in the capitalization rate applied to equities in general. Moreover, this volatility is an increasing function of the duration of growth expected. As dividends are paid on the growth stocks, however, we will find that, in response to a change in the general level of share prices, the volatility of the shares is reduced. Thus, the ingredients will be furnished for a beginning to the type of analysis based on expectations and the mathematics of price movements which I have previously [11] attempted in a study of the term structure of interest rates. Finally, we shall note that several awkward difficulties arise peculiar to the valuation of shares, and thus the limitations as well as the applications of our model will be stressed.

I. *A Brief History of Security Analysis Since the Great Depression*

After its publication in 1934, Graham and Dodd's *Security Analysis* [7] became the classic training manual and reference work for the generation of security analysts who picked over the debris left by the 1929 crash in a search for hidden earning power and hidden assets. The work was a thorough and critical survey of the existing methods and techniques of analysis and was of uniformly high quality. Many of the methods of analysis described in the text remained unexceptionable even throughout the 1950's. The central idea of the work, however, was increasingly criticized in the postwar period, and it is this, its philosophy of valuation, with which we are chiefly concerned.

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[1] The construction of the index, as well as the criteria for selection of the shares included, will be explained later.

The fundamental proposition of security analysis advanced by Graham and Dodd is that any common share may be valued by capitalizing its indicated "earning power" by some suitable multiple. The concept of earning power combines a statement of actual earnings shown over a period of years with the expectation that these past earnings will be approximated in the future, unless extraordinary conditions supervene [7, pp. 418-19]. It is both in the earning-power concept and in the problem of selecting an appropriate multiple of these earnings that security analysts of the 'fifties broke sharply with Graham and Dodd. In the first place Graham and Dodd severely admonished the analyst against unduly emphasizing the past trend in earnings of any company, particularly when the trend is upward. "It must be remembered that the automatic or normal economic forces militate against the indefinite continuance of a given trend. Competition, regulation, the law of diminishing returns, etc., are powerful foes to unlimited expansion . . ." [7, p. 422]. But, and here is the crucial distinction, even if after qualitative study the analyst is convinced that the favorable trend will continue, "the analyst's philosophy must still impel him to base his investment valuation on an assumed earning power *no larger than the company has already achieved* in some year of normal business. Investment values can be related only to demonstrated performance . . ." [7, pp. 422-23, italics mine].

Later Graham and Dodd admit that the earnings of companies which have enjoyed above-average growth may be capitalized more liberally, but the absolute maximum suggested was a multiple 20 per cent higher than that accorded the securities in the Dow-Jones group [7, p. 458]. *Security Analysis* repeatedly warns against the special dangers inherent in overvaluing the earnings of growth stocks [7, pp. 388-89, 396-400, 458]. To buttress their argument they adduce the thesis of Mead and Grodinsky that all industries after a period of growth face an inexorable decline which once begun is rarely reversed [12, pp. 708-09]. Thus the security analyst who strictly followed the tenets of Graham and Dodd would view indulgently no more than a very limited premium for the earnings of a growth stock, and the entire flavor of the Graham and Dodd valuation approach would make him uneasy about even this compromise.

The opposing school of security analysis, which we shall describe as the growth-stock school, also had its beginnings in the Great Depression. It is difficult to ascribe to any one individual the credit for originating the application of the "present value" technique to growth stocks. S. Eliot Guild [8] is often given this distinction, although the classic development of the technique and particularly the nuances associated with it were worked out by John B. Williams [15] in 1938.

This theory argues that it is nonsense to claim that there must exist some arbitrarily defined maximum earnings multiple which should apply to a security no matter what its growth prospects. Surely, the warranted earnings multiple must be related to these prospects if any sense is to be made of the structure of share prices. The growth-stock approach to valuation can be illustrated by a recent variant of the approach suggested by Clendenin and Van Cleave [3]. Utilizing the basic Williams valuation formula (i.e., the present worth of a share is the summation of all dividends expected to be received from it, discounted to present value at an appropriate rate of interest) [15, p. 55], the authors have constructed tables showing the present value of a stock now paying $1.00 per share with varying assumptions concerning the growth rate and the growth period. The important point to be gained from the analysis is that, depending on the growth assumptions made, the investor may be justified in paying double or triple the price for growth shares compared with those which enjoy little or no growth [3, Table 4, p. 371]. Or to put the proposition somewhat differently, if the market values of shares corresponded roughly to the Graham and Dodd measure of central value, then an investor who purchased the shares of "growth companies" would receive an extraordinarily large effective return. Therefore, the search was on for those shares expected to enjoy above-average growth. This search and the concomitant bidding-up of the prices of such shares roughly characterized the major change in the structure of share prices in the 1950's.

While the basic idea of the growth-stock school was both correct and a necessary addition to the Graham and Dodd techniques of security analysis, there were several difficulties with the approach. In the first place, much of the theoretical valuation work focused entirely on dividend payments as the exclusive source of value. It has been argued recently [10] [13] that dividend payments may under certain assumptions be totally irrelevant. Furthermore, the search for an appropriate discount rate had led, at best, to the arbitrary choice of a single rate or, at worst, to the choice of several discount rates where dividends more distant in time are discounted at progressively higher discount rates [3, Table 5, p. 373] [6, pp. 41-43]. This type of technique is not only capricious, but it also suffers from what Miller and Modigliani call the "bird-in-the-hand fallacy."[3] In addition, an extremely long (60-year) horizon is utilized. This has the advantage of seemingly avoiding estimates of future dividend-price relationships. Unfortunately, it has the great disadvantage of resting on the unrealistic assumption that investors are actually able to make estimates for that long a period; if, indeed, it is meaningful even to speak of growth rates compounded over 60 years. Thus we must conclude that these techniques really offer no satisfactory solution to the growth-stock valuation problem.

Another difficulty is inherent in the proposition that the long-term growth rate of dividends may exceed the discount rate. This leads to a very awkward situation. Depending upon how many years one wishes to include in his horizon, one may be justified in paying any amount one wishes for the securities of those companies. Indeed, if perpetual growth at a rate greater that the discount rate is expected, the present value of the shares is infinity [3, pp. 368-69]. David Durand [5] has pointed out the remarkable analogy between this problem and the famous St. Petersburg Paradox of Bernoulli. But the common-stock buyers of the late 1950's were not troubled by the subtleties of these difficulties. Instead, it seemed logical that the investor should not worry about the price he paid for the shares of growth stocks—all that mattered was to find those securities which were experiencing the largest rates of growth. As a corollary to this "new investment maxim," the growth-stock philosophy believed that it had finally solved the age-old investment problem of timing. "The time to buy a growth stock is now. The whole purpose in such an investment is to participate in future larger earnings, so *ipso facto* any delay in making the commitment is defeating" [2, p. 122].

Graham and Dodd's *Security Analysis*, the erstwhile bible of the security analyst, had lost touch with the realities of the new "new era" of common-stock valuation. The search for growth became the main preoccupation of the security analyst [16]. "Growth" took on an almost mystical significance, and to question the propriety of such analysis became, as in a generation past, heretical. The structure of share prices existing in late 1961 is understandable only against this background of the recrudescence of these "new-era concepts" in security analysis. This structure of prices represented the illogical extension of a procedure that was designed by Guild and Williams as simply an ancillary technique to the main body of security analysis, not its replacement.

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[3] Miller and Modigliani [13, p. 424 n.] argue that the attempt to use a purely subjective discount rate to derive uncertainty-valuation formulas is to commit an error analogous to deriving certainty-valuation formulas for shares through the use of marginal rates of time preference.

II. *The Level of Common-Stock Prices*

How then should one decide the appropriate prices for those securities which enjoy a superior rate of growth in earnings per common share? To attempt to answer this question we next begin the construction of a valuation model which, initially, will be presented in terms of perfect certainty. Later we shall introduce some elements of uncertainty into the analysis. In theory, the valuation of any common stock can be reduced to a very simple relationship. Assuming that the investor has a finite horizon, the value of any common stock can be expressed simply as the sum of the present values of both the dividends to be received over the years included in the horizon and the terminal price of the common shares at the end of the horizon period. But immediately a whole host of problems arise: how can one determine the future stream of dividends to be received? How can one solve the valuation formula without first dealing with the problem of how the common shares will be valued at the end of the horizon period? What is the appropriate discount rate to use? And so forth. We might first note that we shall set ourself a task that will be somewhat simplified by the nature of our investigation. Our initial problem will be to deal solely with the structure of stock prices in a certainty setting where we abstract from taxes and market imperfections. The question we shall ask is what is the proper relationship between a given price level for a representative or standard security and the price of a security whose prospects for growth are above average. Then we shall seek to determine the effect of a *ceteris paribus* change in the standard price level on the price of a growth stock. But throughout the discussion we shall abstract from investor expectations concerning future movements of the prices of standard issues. Thus our first job will be to define what is meant by the level of stock prices.

When we refer to the level of stock prices, we have in mind whether equities in general seem to be high or low. In practice, financial people usually take the level of stock prices simply to be the index number associated with the popular market averages of standard securities. The difficulty with such a measure, wholly apart from the index-number problems involved, is that its information content is unsuitable for our purposes. A general price-level index can not distinguish between a change in its value that results only from a change in the earnings and dividends of standard issues and a change that reflects a difference in the capitalization rate applied to constant earnings and dividends. In this paper, which is addressed to the problem of the determination of the "real value" of a share, the appropriate measure is clearly one that deals with capitalization rates, not with absolute prices. Consequently, we shall find it useful to define the level of share prices as the price commanded by a dollar of earnings for a representative share. This "normalized" price is identical with the price-earnings-ratio concept used by financial men to describe the ratio of the price paid per share of stock (P) to the earnings per share (E). The price-earnings multiple (ratio) for any share (m) is simply the inverse of the rate of capitalization of earnings, i.e.,

$$m \equiv \frac{P}{E} \cdot \tag{1}$$

Our choice of this normalized price m instead of the absolute price P is also necessitated because we will want to make comparisons between the values of different securities. Clearly, the ratio of the absolute price of two stocks is a matter of the denominations in which they are issued. It is only the ratio between their price-earnings multiples which can indicate the way in which the two securities are regarded by the market.

We may now briefly outline the development of the model that follows. We shall first use the theoretical construct of a representative standard share in order to find the objective market-determined discount rate that will be employed in valuing other issues. Next we will find the relationship between the discount rate and the earnings multiple of the standard share. It will be seen that a given standard earnings multiple uniquely determines this rate. The growth-stock valuation model will then be introduced. This model will evaluate for any standard earnings multiple (and the discount rate that it implies) the warranted earnings multiple for a growth stock. Since the earnings multiples for both growth and standard shares can be considered normalized prices, we will then have determined a structure of comparable share prices for issues that differ in their earnings and dividend growth rates.

Assuming the investor could buy a share of a representative standard security, what yield would he receive if he knew with certainty the future dividends and the future valuation of the composite share? Assuming the dividend, earnings, and price per share grow at the same percentage rate, it is easy to make a marginal-efficiency-of-investment calculation and solve for the effective yield. We shall find that it equals the current dividend rate per share (i.e., the dividend payment to be received for the year expressed as a percentage of existing market price)[8] plus the percentage annual growth in earnings per share, which, by assumption, is equal to the annual growth in the dividend and the price of each share as well.

The above proposition may be demonstrated most easily if we begin with the simplest possible case and utilize the Williams valuation formula. We define the value of the representative standard share as the present value of the dividends to be received by each share in all future periods. Since, by assumption, last year's dividend payment per representative standard share (D_s) is conveniently expected to grow at a constant growth rate (g_s) over time, the price per share (P_s) will equal:

$$P_s = \sum_{i=1}^{\infty} D_s \left(\frac{1 + g_s}{1 + r}\right)^i. \tag{2}$$

Summing the geometric progression and solving for r we obtain:

$$r = \frac{D_s(1 + g_s)}{P_s} + g_s, \tag{3}$$

i.e., the marginal efficiency of the investment is equal to the current year's dividend rate (the dividend to be paid at the end of the current year as a percentage of the existing market price) plus the growth rate. It can also easily be shown that the same result follows if, instead of discounting the future stream of dividends in perpetuity, we utilize a finite time period and discount back for the dividends and the price of the standard share as of any future date. Assuming that investors continue to be willing to capitalize the earnings of the share at the same rate, the price of the representative share also grows at the rate g_s, and the valuation formula becomes:

$$P_s = \sum_{i=1}^{N} D_s \left(\frac{1 + g_s}{1 + r}\right)^i + P_s \left(\frac{1 + g_s}{1 + r}\right)^N. \tag{4}$$

Summing the progression and rearranging terms we get:

$$P_s\left[1 - \left(\frac{1 + g_s}{1 + r}\right)^N\right] = \frac{D_s(1 + g_s)}{r - g_s}\left[1 - \left(\frac{1 + g_s}{1 + r}\right)^N\right]$$

which (after solving for r) reduces to (3). Thus we see that the time horizon chosen for valuing the representative security is irrelevant under the assumptions postulated.

We shall call r the "apparent marginal efficiency" of the representative standard share. Now we will demonstrate how r uniquely can be determined once the standard earnings multiple, m_s, is known. First, we will find the general relationship between the r and m_s. Let the dividend rate, d_s, be defined as:

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[8] In financial circles this expression is often called the dividend yield. We shall use the term "rate" to avoid confusion with the total yield or marginal efficiency, which we will presently compute.

$$d_s \equiv \frac{D_s(1 + g_s)}{P_s}. \tag{5}$$

d_s is related to r by (3), $r - g_s = d_s$. By (1) $P_s \equiv m_s E_s$. Substituting this expression into (5) and solving for m_s, we have:

$$m_s = \frac{D_s(1 + g_s)}{E_s} \Big/ d_s. \tag{6}$$

In this expression,

$$\frac{D_s(1 + g_s)}{E_s}$$

is simply the payout ratio (based on next year's dividend), which we assume to be known and constant through time, as is implied by our prior assumption that dividends and earnings would grow at the same rate. Therefore, we are able to determine the price-earnings ratio, m_s, from (6). Alternatively, we can view the price-earnings multiple as determining r. Given a fixed payout ratio for standard shares, then any m_s will uniquely determine the dividend rate (d_s). Since g_s is also taken to be a constant, we can then immediately find the apparent marginal efficiency.[4] Thus our construct describing the determination of the level of normalized share prices (i.e., the earnings multiple for a representative standard share) can provide us with a necessary building block for the construction of our valuation model. It uniquely determines an objective market rate of discount which we may then apply to the valuation of growth stocks. Moreover, we shall see that it provides both a method of capitalizing the earnings of these stocks when their period of extraordinary growth is concluded and a standard of comparison of value.

III. *A Model for the Valuation of Growth Stocks*

The term "growth stock" as used in the financial community is a term of approbation to describe those common shares which have enjoyed (or are expected to enjoy) in some sense above-average earnings growth. We shall use the term to describe any equity security that meets both of the following conditions: first,

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[4] But note that if the dividend payout ratio for the standard share is zero (i.e., no dividends are paid) then $r = g_s$, which is, of course, independent of m_s. Alternatively, we might say that the price-earnings ratio is indeterminate. This is so because, given the assumption that investors will continue to be willing to capitalize the earnings of the share at the same rate, any price-earnings ratio is consistent with (3). A, say, 5 per cent rate of growth in earnings will produce a 5 per cent capital appreciation per year (and hence a 5 per cent internal rate of return) regardless of what earnings multiple the standard share commands. Once any dividend is paid, however, this problem disappears.

$$d + g > d_s + g_s, \tag{7}$$

the dividend rate plus the growth rate of the growth stock (the absence of the subscript s always indicates a growth stock) must exceed the dividend plus growth rate for the representative standard share or, what is the same thing, its apparent marginal efficiency. Second, we require that

$$g > g_s, \tag{8}$$

the growth rate of the growth stock must exceed the standard growth rate. This avoids considering as growth stocks securities which merely pay unusually high dividends. In the event that the growth security pays no dividends, then the growth-stock definition simply requires:

$$g > r. \tag{9}$$

Moreover, we expect that the above conditions must have been satisfied on the average, say, for the past three years and are expected to be satisfied for at least the next two years. However, we reject the possibility that a growth rate will exceed r in perpetuity.[5] This particular definition of a growth stock has been chosen to permit applicability of the model to the maximum number of shares. If these conditions are not met, the following model is not appropriate.[6]

We begin with the fundamental proposition that the "market" will tend to equalize the net yield obtainable from all securities. Since the stream of receipts from the growth stock and the representative standard issue are both expected with certainty, then we can use the apparent marginal efficiency of the standard issue as the interest rate appropriate for discounting the future stream of receipts. Let us now recapitulate the symbols to be used:

E = earnings per common share of the growth stock in the fiscal year just past. Again we remind the reader that when the subscript s appears with E, we refer to the earnings of the standard share.

D = dividends per share in the past fiscal year. Dividends are assumed to be paid out annually at the end of each fiscal year.

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[5] This of course would lead us into the "growth stock paradox" again. But as Modigliani and Miller point out [14, p. 664 n.] this possibility must be rejected as a curiosum devoid of economic significance. Assuming that there must be a finite value for all shares, then r (a variable in a general equilibrium setting) would have to rise to restore equilibrium to the capital markets should the rate of earnings growth of some shares be perpetually greater than r. Nevertheless, it is both possible and plausible that the rate of growth of many firms over some finite period may exceed r by a wide margin.

[6] See footnote 9.

g = the expected growth rate of earnings per share and dividends per share over the next N years.[7]

$m \equiv P/E$ = the price-earnings ratio of the share today.

$d \equiv D(1+g)/Em$ = the dividend rate based on next year's dividend payment.

P = the present value of the future stream of receipts, i.e., the value of the common share today.

r = the apparent marginal efficiency of the representative standard share.

In our valuation formula we shall assume that both the dividends and earnings per share of the growth stock grow at a rate g for a period of N years. After that period, the security enjoys only the average growth expected of the standard list of securities (and therefore is able to pay the average dividend of the standard composite security).[8] Accordingly, we postulate that the price-earnings multiple of the shares to be valued falls to that multiple which the market is applying to standard issues. This acknowledges that above-average earnings growth can continue only for a limited period. Furthermore, the price-earnings ratio is expected to recede to the standard level thus avoiding the mistake of "counting the same trick twice," i.e., continuing to value growth stocks at extraordinarily high price-earnings ratios after the extraordinary rate of earnings growth has already occurred [7, pp. 459–60].[9]

Assuming that each year's dividend is paid at the end of the year and that valuation is to be made at the beginning of the year, our valuation formula becomes:

$$(10) \quad P = \frac{D(1+g)}{(1+\bar{r})} + \frac{D(1+g)^2}{(1+\bar{r})^2} + \cdots + \frac{D(1+g)^N}{(1+\bar{r})^N} + \frac{\bar{m}_s E(1+g)^N}{(1+\bar{r})^N} \cdot$$

The numerator of the last term of (10) represents the market price of the security at the end of year N. The earnings in year N, $E(1+g)^N$, are capitalized by applying the standard earnings multiple, m_s. This as-

[7] Underlying the projection of a particular growth rate are, of course, specific assumptions concerning the investment opportunities of the firm and its investment and financial policies. In my view these assumptions are better left implicit in a model which for this discussion addresses itself only to the problem of share valuation. However, in much of the recent work in this area (e.g., Gordon, and Miller and Modigliani) these assumptions are integrated explicitly into the models.

[8] Alternatively, the shares could grow at a rate r and retain all earnings. In fact, any combination of $g+d$ which equals r is permissible. See (A3) below.

[9] This assumption renders the model inapplicable for securities not meeting our conditions for growth. It is one thing to argue that, after a period of abnormal growth, the multiple applied to the shares will fall back to the standard ratio. It is quite another matter to invoke this condition for shares where the historical record has been substandard. In the latter case there is no reason to assume that the issue could, for example, grow at the standard rate and have the same dividend payout ratio as does the standard issue.

sumes that the growth stock, in effect, takes on the characteristics of a standard issue after the period of extraordinary growth is completed. Moreover, we assume that no change occurs in the standard multiple at the end of the growth period (i.e., the level of share prices remains constant). This terminal value we discount to the present and add to the sum of the present values of the stream of dividend receipts.[10] Summing (10) and dividing through by E, we can solve for the price-earnings ratio, m, of the share:

$$m = \frac{D(1+g)}{E(\bar{r}-g)} - \frac{D(1+g)^{N+1}}{E(\bar{r}-g)(1+\bar{r})^N} + \frac{\bar{m}_s(1+g)^N}{(1+\bar{r})^N}\,. \tag{11}$$

It will now be useful to review three well-known theorems concerning growth stocks.[11] First, we should note that the warranted price-earnings ratio, m, that can be paid for a growth stock will always exceed the standard multiple, $\bar{m}_s$. Second, it is clear that the premium over the standard earnings multiple is an increasing function of the growth rate, g. Finally, the valuation premium is also an increasing function of the number of years that the issue continues to meet our criteria of growth. While the form of valuation equation (10) would also seem to indicate that $\partial m/\partial D > 0$, such a conclusion is not justified. Our formula omits the relationship between dividend payments per share and the ability to achieve future earnings growth per share. It could well be that $\partial m/\partial D = 0$ because, while the effect of a large dividend per share would tend to increase m, the lower terminal value *per share* might reduce m by an exactly equal amount.[12]

Let us turn now to the special case of the growth stock that pays no dividends at all. This case is an important one since this condition characterizes a number of securities normally considered growth stocks.

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[10] It should be noted that the investor need not hold his security for N periods to obtain the yield r. Assuming that the standard security continues to be valued at the same rate (i.e., the level of share prices remains constant), then the investor may sell his security at any intermediate time and realize the holding-period yield r.

[11] Formal proof of these theorems is offered in the Appendix.

[12] Miller and Modigliani [13, p. 414], for example, have argued that when a company pays a cash dividend it simply puts in the share-owner's left-hand pocket what it has taken from his right. Given the investment decision, dividend payments to current shareholders merely reduce the value of their claim in the enterprise. This is so because additional equity shares in the business (representing an amount equal to the dividend payment) must be sold in order to finance the predetermined level of capital expenditures. The worth of the enterprise is accordingly affected, not by "how the fruits of the earning power are 'packaged' for distribution," but only by the "real" variables of the company's earning power and investment opportunities. Looked at in this way, a cash dividend is just as irrelevant as a stock dividend. Of course, this argument abstracts, as does ours, from tax considerations, market imperfections, etc. However, for uncertainty counterarguments to the Miller-Modigliani position, see W. J. Baumol [1] and John Lintner [10].

Here the valuation formula reduces to:

$$P = \bar{m}_s E \frac{(1+g)^N}{(1+\bar{r})^N}, \quad \text{or} \tag{12}$$

$$m' = \bar{m}_s \frac{(1+g)^N}{(1+\bar{r})^N}. \tag{13}$$

We are thus able to express the normalized price of a nondividend-paying growth stock as some multiple of the price-earnings ratio for the representative standard share.

We shall now examine the effect of a *ceteris paribus* change in the standard price level on the normalized prices of growth stocks. But, while we allow for such a change to occur, we shall posit that investors have formed no expectations concerning the likelihood of the general level of equity prices moving either up or down. Thus, whatever the level of standard share prices is, the model asserts that a structure of share prices is formed which assumes the perpetuation of the present level. Given this assumption, it follows that nondividend-paying growth stocks are inherently more volatile than standard issues. Moreover, the greater is the number of years for which the extraordinary growth is expected, the more widely will the shares fluctuate in price as the level of share prices changes. However, the volatility of the price-earnings ratio is invariant with respect to the *rate* of growth expected.

While formal proof is offered in the Appendix, these findings can be understood easily if one simply examines equations (12) and (13). When the level of share prices falls, the price-earnings ratio, m_s, of the standard list falls, and therefore the warranted price-earnings ratio of the shares to be valued tends to fall *pari passu* with m_s. But a falling level of share prices also implies that the dividend yield on standard shares tends to rise. Therefore, the discount rate used to convert the future value of the growth stock to present worth increases. The result is that m falls *a fortiori*. Hence, *ceteris paribus*, the price-earnings ratios of nondividend-paying growth stocks may be expected to fall by a greater percentage rate than those of ordinary shares. Furthermore, since the discount factor $(1+r)$ is raised to the Nth power, the decline will be greater, the larger N is, i.e., the longer the period is over which the shares are expected to continue their superior growth record.

Returning to the case of dividend-paying growth stocks, the above results do not, in general, hold. The relevant elasticity expression for dividend-paying shares is, unfortunately, rather unpleasant, and I was unable to deal with it analytically. Instead I was forced to examine ranges of representative values of the function for different values of the variables to determine their effects. Specifically, the value of ϵ_m (the elasticity of the price-earnings ratio) was calculated for all the relevant ranges of m_s, D/E, g, and for N.[18] The following propositions are suggested by these calculations:

$$\frac{\partial \epsilon_m}{\partial (D/E)} < 0. \tag{14}$$

The larger is the dividend paid on the growth stock, then, *ceteris paribus*, the less volatile will it be. The computations also suggest that a necessary, but not a sufficient, condition for this elasticity to be less than unity is for the growth stock to have a payout ratio $(D/E) > (D_s/E_s)$. It also appears that

$$\frac{\partial \epsilon_m}{\partial g} > 0 \quad \text{for} \quad N \geqq 2 \quad \text{and} \quad \frac{\partial \epsilon_m}{\partial g} = 0 \quad \text{for} \quad N = 1. \tag{15}$$

Thus for all horizons greater than one year, the volatility of dividend-paying growth stocks increases with the growth rate. Finally, we conjecture that

$$\frac{\partial \epsilon_m}{\partial N} > 0 \quad \text{for} \quad \epsilon_m > 1 \quad \text{when} \quad N = 1. \tag{16}$$

The volatility increases with N whenever the stock is more volatile than the standard issue to begin with. Thus in the dividend-paying case the following generalization appears to be valid: a dividend payment tends *ceteris paribus* to decrease the volatility of a growth stock. Nevertheless, unless the dividend payout ratio is larger than that for standard issues, these shares will still be more volatile than standard shares and an increase in the growth horizon will, as before, increase their volatility. Finally, the volatility of dividend-paying growth stocks (in contrast to nondividend-paying shares) increases with the growth rate for all horizons longer than one year.

We conclude, then, that in one important respect growth stocks are intrinsically different from standard issues. When the level of share prices changes, the prices of growth stocks must fluctuate more than proportionately if the structure of share prices is to remain unchanged. Thus, growth stocks are inherently more volatile than standard issues. When the level of share prices drops sharply, as was the case during the first six months of 1962, a significantly larger fluctuation in the prices of growth stocks is to be expected as a normal concomitant.

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[18] The calculations were performed on the Institute for Defense Analysis CDC 1604 computer. m_s was allowed to vary from 8 to 25 in intervals of 1, D_s/E_s was set at 0.6, and g_s was held constant at 2 per cent. These values were chosen as being historically relevant in accordance with the empirical analysis which follows. For each m_s (and the r it uniquely determines) a set of warranted growth-stock price-earnings ratios and corresponding elasticities was calculated. Over 17,000 hypothetical growth stocks representing all combinations of payout ratios from 0 to 100 per cent, growth rates up to 40 per cent, and growth horizons up to 15 years were considered in each set. I am greatly indebted to R. E. Quandt for programming the calculation.

Thus, at least a part of the "revaluation of growth stocks" experienced in early 1962 may not have been revaluation at all, but rather a predictable result of the relevant functional relationship.

IV. *Application of the Model*

Before turning to the application of our model we must find some operational definition for the level of share prices and establish the conditions under which the certainty analysis may be used to approximate the world of uncertainty.[14] As the empirical counterpart to our construct of the representative standard share, we shall choose the index number associated with the Standard and Poor 425 Industrial Stock Average (425ISA).[15] This index number can be considered to be the price of a standard composite industrial share. The price-earnings ratio applicable to the composite share we will take to be the normalized price, i.e., the level of share prices.

Recall that we found by (3) that the apparent marginal efficiency of the representative standard share was composed of the sum of its dividend rate and its growth rate. If we assume that the recent rate of growth of earnings of the S & P composite share will continue at a constant rate into the future, we can, at any time, actually assign an internal rate of return to the stock average. Over the past five years (1957 through 1962) the 425ISA has achieved a compounded earnings growth of 1.9 per cent per annum. By taking different periods it is possible to find both higher and lower growth rates. If, for example, 1929 is taken as a base, the annual growth rate rises to 3.8 per cent. However, if the recent high-profit year of 1955 is used, the growth rate of earnings is negligible. The growth of dividends has exceeded the growth rate of earnings in recent years, but the two have been roughly similar for longer periods. We shall use the rate 2 per cent as an empirical approximation to what, in the certainty analysis, would be the anticipated (with certainty) annual percentage increase in earnings and dividends per share. This rate roughly conforms to past experience as adjusted for the less favorable recent earnings performance.[16] The price-earnings ratio of the composite share and the dividend yield that it determines are, of course, matters of record. At the December 12, 1961 market peak, the 425ISA sold at a price-earnings multiple m_s of 20.3 and had an apparent marginal efficiency, r, of approximately 4 $\frac{3}{4}$ per cent. [17] The relevant figures for the June 26, 1962 trough are $m_s = 15.5$ and $r = 5.9$ per cent.

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[14] We shall make no attempt in this paper to develop a full uncertainty model of share valuation. However, we shall indicate in several places during the following discussion the kinds of considerations that are relevant for such an extension to uncertainty.

[15] The problems associated with any index number are well known in the economic literature. The peculiar difficulties associated with the popular market averages are particularly discouraging. Nevertheless, we know of no alternative to attempting to describe the level of share prices in terms of one of the standard lists of equities. We do assert, however, that the S & P 425ISA gives the broadest and fairest available representation of standard industrial shares.

[16] Some support for our choice of 2 per cent as the rate of anticipated earnings growth can be found by examining long-term data. Using the Cowles Commission All-Stock

It is important to note here that in the operational counterpart to our theoretical construct, we have strayed from the protection of perfect certainty. Our empirical internal rate of return is not a "pure" yield, for it includes an allowance for the risk inherent in holding a standard industrial share. This discount rate can be applied appropriately only to growth issues for which the risk is in some sense equal to that of standard securities, for all stocks will not tend to have the same yield regardless of risk. However, we do postulate that there should be no clear risk-premium difference between growth and ordinary shares. That is, for two securities which are equally risky in all other respects, we will ignore any risk differences resulting solely from the fact that the expected yield from one may come largely from capital appreciation, whereas the yield from the second is expected to be derived largely from dividend payments.[18]

There is, however, one respect in which we can say unambiguously that most growth stocks are riskier than standard securities. The former have been shown to be inherently more volatile than repre-

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Index [4, p. 59], annual gains of 1.8 per cent were experienced from just after the Civil War to the middle of the 1930's.

[17] The dividend yield, based on the anticipated next year's dividend rate, was 2.75 per cent, and 2 per cent was the projected growth rate of earnings per share.

[18] For support of this position in the context of an argument concerning the relevancy or irrelevancy of dividend payments, see Lintner [10, p. 255] and Miller and Modigliani [13, pp. 426-32]. Their demonstration, however, is consistent with a view that growth stocks may still be more risky than standard issues. While there is undoubtedly some justification for this frequently voiced position, its importance can be overstated. It is easy to imagine cases where the risk for a company continuously investing large amounts may actually be less than the normal risk of a business doing little investing. Moreover, one can point to a large class of growth companies which derive a substantial share of their earnings from rental and service revenues. In these cases, the companies not only enjoy a stable source of revenue and profit but also grow as the result of each new sale.

Thus, it is not self-evident, when comparing issues of roughly the same quality, why the security analyst would necessarily be less confident of his forecast of earnings growth than his appraisal of the viability of an established yearly dividend. Indeed, any number of counterexamples may be found. A case in point is the unreliability of the dividend payment of so august a security as United States Steel. Financial analysts in 1962 had good reason to regard the continuance of an extraordinary rate of growth by International Business Machines Corporation as a much less risky proposition. Moreover, if we introduce tax considerations, most investors should be expected, if anything, to prefer growth stocks because their ultimate yield would be taxed at substantially lower rates than the shares of standard issues paying higher dividends. This is not to deny, however, that many growth companies, particularly those which invest in completely new areas of endeavor, may indeed be riskier than standard issues.

sentative standard issues. It could be maintained that because of this greater volatility, a higher discount rate should be applied than the apparent marginal efficiency of the standard list. The investor, according to this argument, should be given a risk premium to compensate him for assuming the risks of larger price fluctuations. Thus, by a process akin to "normal backwardation" in the futures markets, growth stocks would have to sell at a discount from their certainty value, which is calculated by abstracting from changes in the level of share prices. But if one subjects this argument to careful examination, one finds a Pandora box of difficulties. Surely, if investors expect that the level of share prices will rise, then growth stocks should be expected to sell at a premium vis-à-vis ordinary shares since they would offer greater possibilities for capital appreciation. Thus a thorough uncertainty model of share valuation would contain precisely the same considerations as make up the expectational theory of the term structure of interest rates. At times, more volatile issues should sell at a premium and at other times at a discount from their certainty valuation precisely because of their inherent mercurial qualities. While we shall not extend this discussion further, these implications may indicate a few of the considerations which need to be included in a full-uncertainty model of share valuation.

We may now proceed to apply the model to recent periods of stock market history. In particular, we shall examine the recent market "adjustment" in the United States, where both the level and the structure of share prices underwent an unusually extensive metamorphosis. Using the formulas developed above, we shall first compute the theoretically justifiable price-earnings ratios for securities with differing growth characteristics. We then may compare the "warranted" normalized prices at recent peak and trough dates with those which actually existed. Table 1 treats only nondividend-paying shares, and, consequently valuation formula (13) is used. The numbers enclosed in the boxes are the warranted price-earnings multiples appropriate for two different levels of share prices. They represent normative valuations, assuming that the listed rate of growth of earnings is to occur for N years.[19] After this, the security loses its extraordinary growth characteristics and becomes valued as a typical standard issue.[20] The greater volatility of the shares compared with standard issues is immediately revealed.

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[19] Of course, the model could easily be modified to allow for a diminution in the growth rate in later years of the growth horizon or the continuation of a more moderate growth rate after the end of N years.

[20] A similar and independent analysis has recently been performed by Charles C. Holt [9]. However, Holt neglects the dependence of the standard yield on the earnings multiple applied to standard issues. Therefore, his analysis suggests (incorrectly) that relative earnings ratios are invariant with respect to the general level of share prices.

TABLE 1—WARRANTED PRICE-EARNINGS RATIOS AT PEAK AND TROUGH LEVELS OF SHARE PRICES

Rate of Growth g / No. of Years Ahead Discounted N	7.5		15		20		25		35		Per Cent Decline Peak to Trough for all g
	Peak	Trough	Peak	Trough	Peak	Trough	Peak	Trough	Peak	Trough	
2	21.38	15.97	24.47	18.28	26.64	19.90	28.91	21.60	33.72	25.19	25.3
3	21.94	16.21	26.86	19.85	30.52	22.55	34.49	25.49	43.45	32.11	26.1
4	22.52	16.46	29.49	21.55	34.96	25.55	41.16	30.09	56.00	40.93	26.9
5	23.11	16.71	32.37	23.41	40.05	28.96	49.12	35.52	72.17	52.18	27.7
7	24.34	17.22	39.02	27.60	52.56	37.18	69.95	49.48	119.88	84.80	29.3

Peak (December 12, 1961) Equation:

$$m = 20.3 \frac{(1+g)^N}{(1.0475)^N}$$

Trough (June 26, 1962) Equation:

$$m = 15.5 \frac{(1+g)^N}{(1.059)^N}$$

Per Cent Decline in Standard Multiple (m_s): 23.6%.

We shall now look at the actual price-earnings multiples that existed for certain well-known nondividend-paying growth shares. We can then reverse the analysis. Instead of asking what the warranted earnings multiple should be, we can instead determine from our model what growth is implicitly being anticipated and is already discounted in the market prices of the shares. Then we can ask whether the past history of earnings growth of these companies suggests that the rate of growth required to justify their current prices is reasonable. What follows is perhaps a very rough and, in large measure, an impressionistic attempt to select a sample of growth stocks that are no riskier than those issues included in the standard list. We wish to insure that the apparent marginal efficiency of the standard list is the rate of return relevant for discounting. It would, of course, be impossible to formulate rigorous criteria for equal riskiness without developing a full-uncertainty valuation model. Nevertheless, we would argue that our method of choosing securities insures that our certainty model gives a good approximation to what might be considered warranted valuations.[21]

Our procedure will be to compute a mean (compounded) growth rate for each company. As has become abundantly clear in much of the recent work in uncertainty analysis, it may very well be that higher moments are of very significant importance to decision-making investors. Thus our insistence on relatively consistent growth will insure that the standard deviation of the mean growth rate is very low. It is necessary that higher moments of the total expected yield for any growth security be essentially similar to those applicable for standard issues. Similarly, we require an investment grade status comparable to standard issues in order to screen out those securities whose past records would suggest that a discount rate higher than r be applied. While a full-uncertainty analysis would include several modifications of this procedure, I do not believe they would materially affect the valuation results we have derived.

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[21] Our five sample nondividend-paying growth stocks have been selected on the basis of the following criteria: (1) that the company has been established in business for at least ten years (we want to exclude speculative new companies without sufficient past history to indicate future prospects); (2) that the company has been profitable in each of the past five years; (3) that the company has met the requirements of the definition of a growth stock in at least four of its last five fiscal years preceding the date of valuation; and (4) that the shares be considered of "investment grade" as evidenced by ratings applied by the investment advisory services and by widespread inclusion in the portfolios of the leading investment companies specializating in common-stock investments.

We now reach the denouement of our story. In Table 2 the price-earnings ratios existing at the recent market peak and trough are compared with the growth rates and growth periods that were implicitly assumed by their prices. We find that to justify the market prices for these issues at the market peak, an investor would have to be certain of extraordinary rates of growth (typically larger than had ever been achieved in the past) for a very long period of years.

To appraise this finding, it is necessary to make two observations, one bordering on the obvious, the other on the metaphysical. Our first comment is simply a reminder that the maintenance of a long-run high level of compounded earnings growth implies an ever increasing arithmatic growth of earnings. If a company which has enjoyed a 25 per cent annually compounded growth rate over the past five years is to continue its same rate of growth in the next five years, then it will have to triple its absolute *increase* of earnings.[22] When we consider that we are discussing earnings *per share,* it is obvious that the difficulties are increased significantly. The actual growth rate of total earnings must in most cases be even larger to overcome the dilution effects of new shares issued. Finally, we should note that most "growth companies" were able to achieve the spectacular growth rates of Table 2 only because of the very low earnings base from which they started. Thus we must conclude on a priori grounds that it is highly unlikely that these companies will continue such rapid rates of growth for more than perhaps one year or, at best, a very few years.

Our second comment concerns the horizon over which the valuation is based. A strong a priori case can be made for the use of a relatively short horizon. I believe that ordinarily five years should be considered the absolute maximum for the investment horizon and, for the reasons given above, I believe that growth rates of 20 per cent or more should

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[22] If the company earned a dollar a share at the beginning of the first period, it must earn over three dollars at the end of the fifth year (a two-dollar increase) and over nine dollars at the end of the tenth year (a six-dollar increase over year five).

TABLE 2—GROWTH-STOCK VALUATIONS
(Growth Rates and Periods Implicitly Assumed)

Growth Stock	Market Peak December 12, 1961					Historic Growth Rate g (Based on Past 5 Years)
	Price P	Earnings Multiple[b] m	Years[a] of Growth at Historic Rate Needed to Justify Current Price N	Growth Rate (g) Needed to Justify Current Price		
				3-Year Horizon $N=3$	4-Year Horizon $N=4$	
	(1)	(2)	(3)	(4)	(5)	(11)
Automatic Retailers	64	98.5	8	77%	55%	28%
Howard Johnson	60	33.5	4	24%	19%	18%
Litton Industries	161	63.9	6	53%	40%	28%
Perkin Elmer	61	54.0	4	45%	34%	37%
Varian Associates	47	64.4	5	53%	40%	31%
Average of Five Growth Stocks	79	62.9	6	53%	39%	28%

Growth Stock	Market Trough June 26, 1962					Historic Growth Rate g (Based on Past 5 Years)
	Price P	Earnings Multiple[b] m	Years[a] of Growth at Historic Rate Needed to Justify Current Price N	Growth Rate (g) Needed to Justify Current Price		
				3-Year Horizon $N=3$	4-Year Horizon $N=4$	
	(6)	(7)	(8)	(9)	(10)	(11)
Automatic Retailers	29	25.2	2½	24%	20%	28%
Howard Johnson	32	16.4	½	8%	7%	18%
Litton Industries	78	24.8	2½	24%	19%	28%
Perkin Elmer	26	24.5	2	23%	19%	37%
Varian Associates	23	33.8	3½	37%	29%	31%
Average of Five Growth Stocks	38	24.9	2½	24%	19%	28%

[a] Rounded to nearest half-year. Thus there are minor rounding inconsistencies in the table.
[b] Based on latest 12-months' earnings to each period.

be viewed very suspiciously if projected for more than three years. Usually a corporation's investment prospects can be forecast intelligently only for the shorter three-year period. The five-year period represents, to my knowledge, the maximum budgeting horizon used by corporations themselves and by financial analysts in attempting an analytical forecast of earnings. An attempt to project growth rates further (except in the most unusual circumstances) seems completely unjustified. But no matter how questionable, precisely these kinds of projections seem to have been made and indeed, as Table 2 indicates, the structure of share prices in late 1961 can be rationalized only on that basis. Thus we would argue that in the world of uncertainty, the time horizon for discounting is one of the most important variables for determining equity values. The difficulty is that while there may be a priori reasons for setting a maximum to the discounting period (and for concluding that these issues were overvalued), there is no *one* reasonable horizon. I believe there is only a ***putatively reasonable horizon,*** a variable characteristic of market sentiment at any time. That the putatively reasonable horizon may undergo rapid shifts is obvious when we compare columns 3 and 8 of Table 2. At the trough, the wheel had turned full circle. Market prices implied more moderate rates of growth (usually well below the past performance of the companies) for periods well under the five-year "maximum" horizon. Even if one argues that part of the revaluation of growth stocks represented a growing distrust of projecting the high growth rates of the past into the future, this explanation is not complete. There was also a definite reduction in the putatively reasonable horizon used by investors in the valuation process. We might also note that to the extent that one could rely on continued good rates of growth for these companies (even if more moderate than in the past), the existing structure of share prices tended to undervalue growth stocks during June 1962, just as it had overvalued them in December 1961.

Our discussion of the use of the model would be incomplete without reference to the valuation of International Business Machines' shares. No other security is so synonymous with growth in the minds of investors as is IBM. Perhaps the most salient feature of IBM's past growth is its remarkable steadiness. Computing compounded growth rates, using each of the past ten years as a basis, one finds that the growth rate in earnings per share was 20 per cent, or slightly above, for each year. Despite IBM's deserved reputation as the ideal growth stock, we should nevertheless be very suspicious if the growth rate implied by the price of the shares should exceed that rate which has been achieved in the past decade. Indeed, as we remarked earlier, we would be surprised to see it continue, particularly considering the size of the company.[23]

Table 3 presents a series of valuation estimates for IBM under differing assumptions as to future growth rates and growth periods. Since IBM pays dividends, formula (11) has been used for the calculation. In addition, we have introduced one innovation into our valuation principle. We may well be accused of being too conservative in assuming that after the period of abnormal growth is completed the shares will sell at the same price-earnings ratio as do standard shares. Indeed, would it not be plausible to assume that the shares of a company such as IBM would still be able to command a premium over ordinary shares both because of its extraordinarily high quality and because the company's growth prospects may still appear to be above average? To be as liberal as possible, we have made a separate calculation making the (arbitrary) assumption that the residual price-earnings multiple for the shares will be 50 per cent higher than the multiple applied to standard securities. The calculations are presented below.

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[23] IBM's total revenues are now running at an annual rate of approximately $2 billion. To keep up a long-term 20 per cent growth rate (we assume earnings and revenues must expand at the same rate) then the revenue growth from year seven to eight would equal $1.4 billion, the revenue volume of 1960. To push the argument to the absurd, if the growth rate continued for an additional 22 years, IBM's revenues would exceed the U.S. national income in 1962.

Table 3—Valuation of IBM Common Stock

Horizon N	Market Peak December 12, 1961				Market Trough June 26, 1962			
	$g=20\%$		$g=15\%$		$g=20\%$		$g=15\%$	
	Residual Earnings Multiple		Residual Earnings Multiple		Residual Earnings Multiple		Residual Earnings Multiple	
	20.3	30	20.3	30	15.5	23	15.5	23
3	31.5	46.1	27.5	40.3	23.6	34.5	20.5	30.1
5	42.0	61.1	33.5	49.0	31.0	45.0	24.5	35.8
7	55.7	80.8	40.7	59.4	40.4	58.4	29.3	42.7

At the market peak, IBM shares sold at 580, a multiple of 64 times the most recent 12-months' earnings.[24] Particularly in light of our previous discussion on the implications of extending a growth rate of 20 per cent for a period of seven years and beyond, we can assert unequivocally that IBM shares were overvalued relative to the standard list of securities. In the words of a favorite Wall Street aphorism "share prices were discounting not only the future but also the hereafter." At the trough, IBM shares sold at 320, a multiple of 32 times the latest 12-months' earnings. While the case for considering IBM shares undervalued is not as clear-cut as the overvaluation at the December peak, it is easy to find justification for believing that the shares of IBM were probably slightly on the low side of a reasonable valuation vis-à-vis the level of share prices.[25]

[24] As estimated by the author. Reported earnings in all cases have been adjusted to include unremitted foreign income.

[25] Assuming a growth rate of 15 per cent could be achieved for three years and that the residual multiple of earnings be a modest 23, the price-earnings multiple is (approximately) justified.

V. *Summary and Conclusion*

It will be useful now to review the ground we have covered and to appraise the results we have achieved. We attempted to explain the December, 1961 peak structure of share prices as a phenomenon understandable only against a background of the history of security analysis since the Great Depression. We described the shifts in the central interest of security analysis from an analysis of balance sheet values through an emphasis on earning power and finally to the elevation of growth analysis as the *sine qua non* of profitable investment. Concomitantly, we observed that fashions of equity investment determine a structure of share prices which, at various times, has put varying degrees of premium on these values.

Next we introduced and developed a simple model by which we might determine a warranted structure of share prices on a certainty basis. In addition to demonstrating its potential usefulness in detecting cases of over- and undervaluation of growth shares, the model could serve as a conceptual scheme for analyzing the change in the structure of share prices which took place. In the first place, we showed that a part of the much larger percentage decline in growth stocks was to be expected from a stable functional relationship, just as when interest rates rise, long-term bonds can be expected to suffer a sharper decline in price. Next, we analyzed the revaluation in terms of investors' reluctance to continue to accept the plausibility of projections of past rates of growth into the future and their uneasiness with the very long horizons implicit in share prices. Moreover, we showed that precisely because some stocks were being valued on the basis of a long growth horizon, their volatility, given a change in the level of share prices, was increased. Finally, in our modification to the IBM valuation model, we suggested that the expected residual price-earnings ratio applied to the shares at the termination of their period of extraordinary growth could also be an important variable. In a world of uncertainty, where company prospects beyond the very immediate future become increasingly fuzzy, there may be room for a considerable range of residual valuations for the same security.

But perhaps even more important than these considerations, our analysis has at once demonstrated what an ephemeral process valuation is and underlined the very severe limitations by which any model of security valuation must necessarily be beset. There is no self-evident appropriate horizon for the projection of growth rates, only a putatively reasonable one. The history of share-price behavior demonstrates ineluctably that investors are at some times willing to take a much longer view than at others. At the beginning of the present decade of the "soaring 'sixties" it did not seem unreasonable to anticipate with some degree of confidence a decade of substantial growth. At the 1962 market trough investors were unwilling to pay a substantial premium for any growth that was not expected to occur over the fairly immediate future. We have demonstrated the very large magnitude of difference in warranted prices that can result from such a change in the growth horizon. Moreover, it is enormously difficult to determine the growth rates that should be used in the valuation formulas, if, indeed, it is even admissible (except for simplification of analysis) to project compounded growth rates for earnings per share. It is very difficult to be neutral about things so uncertain as future growth rates. At times of great optimism it is easy to accept the idea that even some acceleration of past growth rates is possible, given the exorbitant claims that were made for the economy during the decade of the 'sixties. At times of great pessimism it seems implausible to believe that any but the most modest rates of growth are sustainable for companies which now must grow on a far larger base of sales and earnings than was the case in years past, when their historic growth records were made. The problem, of course, is that it is easy for investors to convince themselves of the credibility of either of these positions.

The Keynesian analogy of the newspaper contest is by now a classic statement of the problems which we face. It is immaterial to the contestant what his personal criteria of beauty may be. What is important is rather what the average opinion considers the relevant criteria to be, or rather, what the average opinion believes the popular criteria to be, etc. Similarly, it is less important in determining market values to pick, let us say, four years as the objectively proper period over which growth can be discounted, when the relevant period for discounting is determined by that period which the average opinion believes to be proper. This I believe to be at the heart of the problem of valuation. However, this is not to be construed as implying that market values are necessarily determined by the irrationality of some sort of perpetual tulip-bulb craze. On the contrary, I am convinced that actual market values oscillate around a structure of prices that does a reasonably good job at attempting to equalize net yields, given the often very limited amount of information available to the investing community. But styles and fashions have a habit of changing drastically for considerable periods of time, so that the Keynesian admonition about the long run must always be kept in mind. Therefore, we must always accept an uncomfortably large degree of indeterminacy in economic models that attempt to determine objectively a structure of warranted stock prices. Nevertheless, it seems to me that both economists and those closest to the financial markets can benefit from this kind of conceptual scheme which, if nothing more, can describe quantitatively the market valuations that exist at any time.

APPENDIX

Theorem 1: The normalized price, m, that can be paid for a growth stock will always exceed the standard multiple, m_s.

Proof: Substituting d for $D(1+g)/mE$ in (11) and solving for $\bar{m}_s/m$ we obtain:

$$\frac{\bar{m}_s}{m} = \frac{(1+\bar{r})^N}{(1+g)^N} - \frac{(1+\bar{r})^N}{(1+g)^N}\left(\frac{d}{\bar{r}-g}\right) + \left(\frac{d}{\bar{r}-g}\right). \tag{A1}$$

Now it can be shown that the quantity $\bar{m}_s/m < 1$ for a growth stock. Assume $g < r$. Then

$$\frac{(1+\bar{r})^N}{(1+g)^N} > 1 \quad \text{and} \quad \frac{d}{\bar{r}-g} > 1$$

from (7), the definition of a growth stock. It can then be seen that

$$\frac{(1+\bar{r})^N}{(1+g)^N}\left[1-\left(\frac{d}{\bar{r}-g}\right)\right] < \left[1-\left(\frac{d}{\bar{r}-g}\right)\right] \quad \text{or} \quad \frac{\bar{m}_s}{m} < 1, \tag{A2}$$

the price-earnings multiple of the growth stock is greater than the standard multiple. A similar demonstration applies to the case of $g > r$. If $g = r$, then utilizing (10) we have:

$$m - \bar{m}_s = N\left(\frac{D}{E}\right) > 0. \tag{A3}$$

But note when

$$d = r - g, \quad m = \bar{m}_s.$$

Theorem 2: The warranted price-earnings multiple for a growth stock is an increasing function of the growth rate expected.

Proof: Substituting mE for P in (10) and differentiating we obtain:

$$\frac{\partial m}{\partial g} = \frac{D}{E(1+\bar{r})} + \frac{2D(1+g)}{E(1+\bar{r})^2} + \cdots + \frac{ND(1+g)^{N-1}}{E(1+\bar{r})^N} + \frac{N\bar{m}_s(1+g)^{N-1}}{(1+\bar{r})^N} > 0. \tag{A4}$$

Of course, this proposition holds even in the absence of the convenient assumption of constant growth, as the formulation of (A4) makes abundantly clear.

Theorem 3: The warranted earnings multiple is an increasing function of the number of years for which the security is expected to continue its extraordinary rate of growth.

Proof: Differentiating (11) and simplifying we obtain:

$$\text{(A5)}\qquad \frac{\partial m}{\partial N} = \ln\left[\frac{(1+g)}{(1+\bar{r})}\right]\left[\frac{(1+g)}{(1+\bar{r})}\right]^N\left[\bar{m}_s - \frac{D}{E}\,\frac{(1+g)}{(\bar{r}-g)}\right].$$

If

$$g > \bar{r},\ \ln\left[\frac{1+g}{1+\bar{r}}\right] > 0 \text{ and } -\frac{D}{E}\,\frac{(1+g)}{(\bar{r}-g)} > 0 \text{ so that } \frac{\partial m}{\partial N} > 0.$$

If

$$g < \bar{r},\ \ln\left[\frac{1+g}{1+\bar{r}}\right] < 0, \text{ but } \left[\bar{m}_s - \frac{D}{E}\,\frac{(1+g)}{(\bar{r}-g)}\right] < 0 \text{ also.}$$

To show

$$\bar{m}_s < \frac{D}{E}\,\frac{(1+g)}{(\bar{r}-g)}$$

we divide both sides by $m > 0$ and obtain

$$\frac{\bar{m}_s}{m} < \frac{d}{(\bar{r}-g)}.$$

This holds since

$$\frac{\bar{m}_s}{m} < 1 \text{ by (A2) and } \frac{d}{(\bar{r}-g)} > 1 \text{ by (7).}$$

Theorem 4: Nondividend-paying growth stocks are more volatile than standard issues. For a given (percentage) change in the level of share prices, m_s, m will vary more than in proportion.

Proof: Utilizing expression (3) for r and substituting $m_s\bar{E}_s$ for P_s, we can restate (13) as

$$\text{(A6)}\qquad m = m_s\frac{(1+g)^N}{\left[1+\bar{g}_s+\dfrac{\bar{D}_s(1+\bar{g}_s)}{m_s\bar{E}_s}\right]^N}.$$

Now letting m_s vary (i.e., allowing the general level of share prices to change) and differentiating m with respect to m_s, we have:

$$\text{(A7)}\quad \frac{\partial m}{\partial m_s} =$$

$$\frac{\left[1+\bar{g}_s+\dfrac{\bar{D}_s(1+\bar{g}_s)}{m_s\,\bar{E}_s}\right]^N[1+g]^N + m_s[1+g]^N N\left[1+\bar{g}_s+\dfrac{\bar{D}_s(1+\bar{g}_s)}{m_s\bar{E}_s}\right]^{N-1}\left[\dfrac{\bar{D}_s(1+\bar{g}_s)}{\bar{E}_s m_s^2}\right]}{\left[1+\bar{g}_s+\dfrac{\bar{D}_s(1+\bar{g}_s)}{m_s\bar{E}_s}\right]^{2N}}.$$

Multiplying through by m_s/m to convert (A7) into an elasticity formulation (where we use expression (A6) for m), we have:

$$\frac{\partial m}{\partial m_s}\cdot\frac{m_s}{m} = 1 + \frac{Nd_s}{1+r} > 1. \tag{A8}$$

Theorem 5: The volatility of nondividend-paying growth stocks increases with the number of years included in the time horizon but is invariant with respect to the rate of growth expected.

Proof: Letting $\epsilon_m \equiv \partial m/\partial m_s \cdot m_s/m$ represent the elasticity of the price-earnings ratio and differentiating (A8), we have:

$$\frac{\partial \epsilon_m}{\partial N} = \frac{d_s}{1+r} > 0 \tag{A9}$$

and

$$\frac{\partial \epsilon_m}{\partial g} = 0. \tag{A10}$$

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MANAGERIAL DISCRETION AND BUSINESS BEHAVIOR

By Oliver E. Williamson*

The importance of managerial discretion in the operations of the large corporation has been widely recognized. Carl Kaysen has recently characterized the large corporation as one "in which the constraints imposed by market forces are loose, and the scope for managerial choice is considerable" [14, p. 90], and R. A. Gordon holds that the development of the large corporation has led "to a greater emphasis on the non-profit goals of interest groups other that the stockholders. Almost certainly, the personal and group goals of . . . executives are a part of the total value system—the desires for security, power, prestige, advancement within the organization, and so on. . . . Profits are viewed as the basic constraint subject to which other goals can be followed" [12, p. xii].

Although there is substantial support for these views, it is by no means universal,[1] and before general acceptance can be attained a number of questions need to be answered. Can operational significance be provided to these managerial goals? Can such a translation of managerial objectives be integrated into a theory of the firm from which meaningful theorems can be derived? What is the evidence that discretion has an important and systematic impact on business behavior? Specifically, what influence does competition in the product market, managerial tastes, and the diffusion of stockholder control have on the allocation of resources in the business firm? How do regulatory or other constraints influence nonprofit behavior?

My purpose in this paper is to show that the first two of these questions can be answered in the affirmative, that the preliminary evidence tends to support the proposition that the opportunity for discretion does have a systematic effect on resource-allocation decisions, and that regulatory constraints are apt to produce particularly strong manifestations of nonprofit behavior. Part I introduces the notion of "expense preference" for translating managerial goals to an operational form, develops the implications of a model constructed around these objectives, and contrasts these with those obtained from the profits-maximization hypothesis. Part II examines some of the preliminary evidence on discretionary behavior. The effects of regulatory constraints are considered in Part III.

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* The author is assistant professor of economics at the University of California, Berkeley. He acknowledges with thanks the helpful comments of R. M. Cyert, J. G. March, A. H. Meltzer, E. S. Mills, J. F. Muth, L. A. Rapping, and H. A. Simon. The study resulting in the paper was made in part under a fellowship granted by the Ford Foundation. However, the conclusions, opinions, and other statements in the paper are the author's and are not necessarily those of the Ford Foundation.

[1] For an argument that monopoly distortions are not great, see A. C. Harberger [13] and David Schwartzman [23].

I. *Some Models of Business Behavior*[2]

My purpose in this section will be to show how managerial objectives can be introduced into a theory of the firm, to develop the implications of a model responsive to what appears to be the salient motives of managers, and to contrast these with those obtained from short-period and multiperiod versions of the profits-maximization hypothesis.

A. *A Managerial Discretion Model*

1. *Managerial Objectives.* The following list represents a (largely overlapping) composite of the managerial motives identified as the result of the experiences and insights of the organization theorists, Chester I. Barnard [4] and Herbert A. Simon [26], and the study of business leadership of R. A. Gordon [12]: salary, security, power, status, prestige, professional excellence.[3] That they are neither equally significant nor entirely independent should be obvious. Rather than attempt a finer discrimination, however, it seems more fruitful to inquire into the behavior such motives produce.

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[2] The material appearing in this and the subsequent sections is developed in more detail in my unpublished doctoral dissertation, "The Economics of Discretionary Behavior: Managerial Objectives in a Theory of the Firm" (Carnegie Institute of Technology, 1963). The initial version of the model, which has since been modified, appears in [34].

[3] In addition to the factors listed, Barnard and Simon also include expansion as one of the firm's objectives. Indeed, William J. Baumol has recently suggested that the firm is operated so as to maximize the rate of growth of sales [6, pp. 1085-87], which is the dynamic counterpart of his static sales-maximization hypothesis [5] and preserves the main theorems of his earlier model [6, p. 1087]. Although "expansion" objectives also enter into the model that we propose, it is a somewhat selective variety of expansion that will occur—namely, expansion of those types of activities that most contribute to the satisfaction of the management—rather than a generalized expansion of the entire scale of the firm. Whether this leads to a more fruitful construction is an empirical question and can scarcely be determined on a priori grounds. The preliminary evidence that is examined in Part II of this paper appears to support the proposition that preferences towards specific varieties of expenses exist (rather than a generalized preference for expansion per se), but this is essentially an open question.

The usual objection to introducing nonpecuniary elements into the theory of the firm is that such considerations, if not unimportant, are analytically evasive. Since their importance is an empirical question, it can hardly be dismissed so easily. In order, however, to assess their influence, an analytical basis for examining them must be devised. Shifting attention from the motives to the *means* by which the motives are realized provides the necessary connection. That is, rather than attempt to introduce security, power, prestige, and so forth into the theory directly, we ask instead: to what activities do these motives give rise? These activities, rather than the motives, are then made a part of the model.

The essential notion that we propose in order to connect motives with behavior is that of *expense preference.* That is, the management does not have a neutral attitude towards costs. Directly or indirectly, certain classes of expenditure have positive values associated with them. In particular, staff expense, expenditures for emoluments, and funds available for discretionary investment have value additional to that which derives from their productivity.

Expansion of staff is an activity that offers positive rewards, the benefits of which can be enjoyed quite generally [18, pp. 321-22]. Indeed, since promotional opportunities within a fixed-size firm are limited, while increased jurisdiction has the same general effect as promotion but simultaneously produces the chance of advance for all, the incentive to expand staff may be difficult to resist. Not only is it an indirect means to the attainment of salary [25], but it is a source of security, power, status, prestige, and professional achievement as well.[4]

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[4] As has been observed among organization theorists, "the modern organization is a prolific generator of anxiety and insecurity" [30, p. 24]. This insecurity is partly due to uncertainty with respect to the survival of the organization as a whole and, more important (and more immediately relevant to its individual members), of the parts with which the individuals identify. Attempts to reduce this condition can be expected; indeed, the direction these efforts will take can be anticipated. If the surest guarantee of the survival of the individual parts appears to be size, efforts to expand the separate staff functions can safely be predicted.

That staff contributes to power, status, and prestige should be self-evident. This is true within the organization as well as in the manager's business and social relationships outside the firm. The vast influence that executives in large industrial organizations enjoy arises much more from the perceived control over resources that they possess than from the personal wealth which they have attained.

The "professional" inducement to expand staff arises from the typical view that a progressive staff is one that is continuously providing more and better service. An aggressive staff will therefore be looking for ways to expand. Although in choosing directions for expansion the relative contribution to productivity will be considered, the absolute effect on profits may be neglected. As long as the organization is able to satisfy its performance requirements, there is a predisposition to extend programs beyond the point where marginal costs equal marginal benefits. The incentive to increase staff, having both natural and legitimate elements, is exceptionally difficult to resist.

We use the term "emoluments" in a somewhat special sense. They refer to that fraction of managerial salaries and perquisites that are discretionary. That is, emoluments represent rewards which, if removed, would not cause the managers to seek other employment. They are economic rents and have associated with them zero productivities. Thus they are not a return to entrepreneurial capacity but rather result from the strategic advantage that the management possesses in the distribution of the returns to monopoly power. Being a source of material satisfaction and an indirect source of status and prestige, they are desirable as a means for satisfying goals in each of these respects.

The management would normally prefer to take these emoluments as salary rather than as perquisites of office since, taken as salary, there are no restrictions on the way in which they are spent, while, if withdrawn as corporate personal consumption (such as expense accounts, executive services, office suites, etc.), there are specific limitations on the ways these can be enjoyed. However, there are two considerations that make perquisites attractive. First, for tax purposes it may be advantageous to withdraw some part of discretionary funds as perquisites rather than salary. Second, perquisites are much less visible rewards to the management than salary and hence are less likely to provoke stockholder or labor dissatisfaction.[5] Hence a division of emoluments between salary and perquisites is to be expected.

Although it is difficult to specify what fraction of salary and perquisites is discretionary in the sense defined, it is possible, as we show in Part II, to test for the relation of these rewards to competition in the product market and managerial tastes. Thus, they can be identified *ex post* even if not *ex ante.*

The existence of satisfactory profits is necessary to assure the interference-free operation of the firm to the management. Precisely what this level will be involves a complicated interaction of the relative performance of rivals, the historical performance of the firm, and special current conditions that affect the firm's performance. Management, however, will find it desirable to earn profits that exceed the acceptable level. For one thing, managers derive satisfaction from self-fulfillment and organizational achievement, and profits are one measure of this success. In addition, profits are a source of discretion (indeed, we define "discretionary profits" as the difference between actual profits and minimum profits demanded). Discretionary profits represent a source of funds whose allocation may be importantly determined by managerial, in addition to economic, considerations. As with the expansion of staff, the expansion of physical plant and equipment provides general opportunities for managerial satisfaction and for much the same reasons.

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[5] Historically, whenever stockholder discontent has been rampant, management compensation has been a favorite target [12, p. 164]. Likewise in wage negotiations, unions often make a point of executive salary levels. Emoluments, being much less visible, are less readily attacked.

2. *The Model.* Since these notions will be introduced explicitly into a mathematical model, it will be useful to define them more precisely. The relationships that we shall use are:

R = revenue = $P \cdot X$; $\partial^2 R/\partial X \partial S \geq 0$
P = price = $P(X, S; \epsilon)$; $\partial P/\partial X < 0$; $\partial P/\partial S \geq 0$; $\partial P/\partial \epsilon > 0$
X = output
S = staff (in money terms) or (approximately) general administrative and selling expense
ϵ = the condition of the environment (a demand-shift parameter)
C = production cost = $C(X)$
M = managerial emoluments
π = actual profits = R-C-S
π_R = reported profits = $\pi - M$
π_O = minimum (after tax) profits demanded
T = taxes where t = tax rate and $\overline{T}$ = lump-sum tax
π_R-π_O-T = discretionary profits
U = the utility function

From our statement of the firm's objectives, the firm is operated so as to

maximize: $U = U[S, M, \pi_R - \pi_O - T]$
subject to: $\pi_R \geq \pi_O + T$.

As formulated, the constraint is of the same form as the last term in the utility function. Hence, assuming that second order conditions are satisfied and disallowing corner solutions, the constraint becomes redundant so that we can treat the problem as one of straightforward maximization.[6] Substituting the functional relationships for profits into the expression we have:

maximize: $U = U[S, M, (1-t)(R-C-S-M)-\pi^0]$.

The following first-order results are obtained by setting the partial derivatives of U with respect to X, S, and M equal to zero:[7]

$$\frac{\partial R}{\partial X} = \frac{\partial C}{\partial X} \tag{1}$$

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[6] Although this is a convenience, it is by no means a necessity. An inequality-constrained maximization could be handled by making use of the Kuhn-Tucker theorem [16].

[7] In these expressions, U_1 is the first partial of the utility function with respect to S, U_2 is the first partial with respect to M, and U_3 is the first partial with respect to π_R-π_O-T.

$$\frac{\partial R}{\partial S} = \frac{-U_1 + (1-t)U_3}{(1-t)U_3} \tag{2}$$

$$U_2 = (1-t)U_3. \tag{3}$$

From equation (1) we observe that the firm makes its production decision in the conventional fashion by equating marginal gross revenue to the marginal costs of production. However, equation (2) reveals that the firm will employ staff in the region where the marginal value product of staff is less than its marginal cost. This equation can be rewritten as:

$$\frac{\partial R}{\partial S} = 1 - \frac{1}{(1-t)} \frac{U_1}{U_3}, \text{ where } \frac{U_1}{U_3}$$

is the marginal rate of substitution between profits and staff. In the profit-maximizing organization, staff has no value other than that associated with its productivity, so that this exchange rate is zero, and the equality of marginal costs and value products obtains. Equation (3) discloses that the firm will absorb some amount of actual profits as emoluments—the amount being dependent on the tax rate.

Having established the equilibrium conditions, the comparative statics properties of the model remain to be developed.[8] That is, we want to find how the system adjusts to a change in the condition of the environment (the demand-shift parameter ϵ), a change in the profit tax rate (t), and a lump-sum tax ($\bar{T}$).

The results for a displacement by each of the parameters are shown in Table 1. The direction of adjustment of any particular decision variable to a displacement from its equilibrium value by an increase in a particular parameter is found by referring to the row and column entry corresponding to this pair.

TABLE 1—RESPONSES TO DISPLACEMENTS FROM EQUILIBRIUM FOR THE MANAGERIAL MODEL

Variable	Parameter		
	ϵ	t	$\bar{T}$
X^0	+	+	−
S^0	+	+	−
M^0	+	+	−

Actually the response to a change in the profits tax rate is not unambiguous. It can be shown that this response is separable into a net substitution effect and the equivalent of an income effect, where the net substitution effect is always positive and the income effect is always negative. The gross substitution effect is the combination of these two separate effects and hence depends on their relative magnitudes. Under reasonable assumptions, the gross substitution effect will be positive as shown in the table.[9]

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[8] The procedure we use for obtaining the comparative statics responses is described in Paul A. Samuelson [22, pp. 12-14].

B. *Entrepreneurial Models*

The significance of these responses can best be discussed by comparing them to the corresponding results obtained from profit-maximizing models. Consider first the usual or single-period profit-maximizing model. As is well known, the equilibrium relations for this model require that the firm be operated so as to equate marginal gross revenue with the marginal costs of production and the marginal value product of staff with its marginal cost. The comparative statics responses are shown in Table 2.

TABLE 2.—RESPONSES TO DISPLACEMENTS FROM EQUILIBRIUM FOR THE SHORT-RUN PROFITS-MAXIMIZING MODEL

Variable	Parameter		
	ϵ	t	$\bar{T}$
X^0	+	0	0
S^0	+	0	0

The differences between the models are more numerous than their similarities. Indeed, it is only with respect to the demand-shift parameter (ϵ) that the two return the same results, and even here the result is not identical. In addition to the increases in staff and output that the profit-maximization model shows, the managerial model also indicates that spending for emoluments will increase as ϵ increases. Moreover, while the qualitative differences with respect to ϵ are not great, quantitative differences may produce sharper discriminations. In general, a profits-maximizing firm will adjust to changes in business conditions within narrower bounds than the utility-maximizing firm. The absence of slack in its operations, as contrasted with the calculated accumulation (and decumulation) of slack by the utility-maximizing firm, is responsible for these quantitative differences.

A more general entrepreneurial model can be obtained by devising a multiperiod or discounted version of the profits-maximization hypothesis. The variables are subscripted by time periods by i, where $i=1, 2, \cdots n$, and n is the planning horizon. Letting r be the discount rate, profits in year i will be discounted by $1/(1+r)^{i-1}$. Let this be represented by α^{i-1}. We make the assumption that production decisions in

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[9] Only when the firm is pressed very hard to satisfy its minimum-profits constraint is a reversal apt to occur.

TABLE 3—COMPARATIVE STATICS RESPONSES FOR THE DISCOUNTED PROFITS-MAXIMIZING MODEL

Variable	Parameter			
	ϵ	t'	t''	$\bar{T}$
X_1^0	+	+	0	0
S_1^0	+	+	0	0

period k affect costs in no other period or, if there are effects, that these are offsetting. However, staff expenditures in period k are assumed to have a positive influence on future-period revenues over the entire planning horizon. Indeed, the length of the "period" can be defined as the interval beyond which current production decisions have no effect and the length of the planning horizon as the number of such periods for which current staff expenditures have a positive effect.

Letting π represent the discounted value of profits, the objective is to maximize:

$$\pi = \sum_{i=1}^{n} (1 - t)(R_i - C_i - S_i - \bar{T}_i)\alpha^{i-1}.$$

First-order conditions for a maximum are obtained by setting the partial derivatives of π with respect to X_1 and S_1 equal to zero. Thus we have:

$$\frac{\partial R_1}{\partial X_1} = \frac{\partial C_1}{\partial X_1} \tag{4}$$

$$\frac{\partial R_1}{\partial S_1} = 1 - \sum_{i=2}^{n} \frac{\partial R_i}{\partial S_1}\alpha^{i-1}. \tag{5}$$

Inspection of equation (4) reveals that the firm chooses that value of output for which the marginal gross revenue is equal to the marginal costs of production. Equation (5), however, shows that the current marginal value product of staff is less than its current marginal cost.[10] These equilibrium conditions are thus similar to those obtained from the managerial model.

Since the effects when the tax is levied for a period less than the planning horizon are different from those when the tax covers the entire horizon, the tax-rate effect is split into "temporary" (designated tax rate t') and "permanent" (designated tax rate t'') types. The comparative statics responses for this model are displayed in Table 3.

Whereas the qualitative responses to a "temporary" change in the profits tax rate are identical to those obtained from the managerial model, a change in the "permanent" profits tax or the levy of a lump-sum tax (or bounty) produces no effect in the profits-maximizing organization. A response to both is predicted by the managerial model. Hence a discrimination between the hypotheses on the basis of comparative statics properties is potentially achievable.

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[10] Over the entire horizon, however, the marginal value product of staff equals its marginal cost.

II. *Some Evidence*

Changes in either the profits tax or a lump-sum tax provide the most direct basis for distinguishing between the utility and profits-maximization theories. Testing the effects of a profits tax, however, requires that a rather advanced type of simultaneous-equations model be devised, while lump-sum taxes are hard to come by. The first of these carries us beyond the range of the present analysis, and only preliminary evidence on the effects of the lump-sum tax is available. (See Section B below.)

Fortunately other tests of a less direct but nonetheless meaningful sort can be devised. For one thing, the comparative statics implications are limited to qualitative responses; quantitative differences are neglected. If, therefore, significant quantitative differences between the two theories can be shown to exist, these can be used for making a discrimination where qualitative properties are identical.

Secondly, tests of particular behavior are available. Thus, the utility-maximizing theory is based on the proposition that opportunities for discretion and managerial tastes will have a decided impact on the expenditures of the firm. More precisely, those expenditures that promote managerial satisfactions should show a positive correlation with opportunities for discretion and tastes. The profit-maximizing theory is somewhat ambiguous on this question. Interpreted as a theory which attends entirely to the stockholders' best interests, it clearly implies that expenditures which, under the utility-maximizing hypothesis, will be positively correlated with measures of discretion and tastes, will instead be uncorrelated with these relationships. Interpreted somewhat more loosely, closer agreement with the utility-maximizing hypothesis can be obtained. Thus, it is possible that the management first selects that physical combination of factors that maximizes profits and then absorbs some amount of actual profits as cost. These absorptions may be correlated with the same measures of discretion and taste as would be expected under the utility-maximizing theory. Hence, evidence that managers respond to opportunities for discretion is not inconsistent with the profit-maximizing theory, but neither is evidence to the contrary; the theory is simply silent on this question. However, the failure of firms to respond to opportunities for discretion constitutes a contradiction of the utility-maximizing hypothesis, while observations that firms do display expense-preference behavior supports it.

The executive compensation and retained-earnings analyses reported in Section A are designed to test for the effects of discretion and taste in management expenditure decisions. The summary of the field studies in Section B is concerned with the question of physical magnitudes of adjustment to adversity and provides some indication of what criteria are involved in making expense adjustments as well as what effects a lump-sum tax has on business behavior.

A. *Principal-Firm Analysis*

If the firm is operated so as to attend to managerial interests, then the classes of expenditures for which expense preference was indicated should be expanded beyond the levels called for by strictly profit considerations. The amount by which such expansions occur should be positively related to the opportunity for discretion and the tastes of the management. More precisely, if X is an expenditure for which a positive expense preference exists, I_1 is an index of the absence of competition, I_2 is an index of management taste, I_3 is an index of stockholder diffusion, and $f(\pi)$ is the level of X which would be supported solely by profit considerations, then under the utility-maximization hypothesis:

$$X = f(\pi)g(I_1, I_2, I_3)$$

where

$$\frac{\partial X}{\partial I_i} > 0.$$

Under the stockholder version of the profits-maximization hypothesis, the partial derivative of X with respect to each of the I_i will be zero.

Since it is in the large corporation that manifestations of discretionary behavior are alleged to be important, and as complete data are most readily available among larger industrial firms than their smaller counterparts, the tests are restricted to those firms that clearly qualified as "principal firms." Among the 26 industries included in the analysis, selection was limited to the two largest firms, ranked according to sales, in each.[11] The tests performed are cross-section tests for the years 1953, 1957, and 1961.

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[11] Although the sample is a purposive rather than a random sample, the results probably generalize to a somewhat larger population. Based on the *1962 Moody's* listing of major industries (pages a-16 and a-17) 24 additional industries would be added to our list of 26 for a total of 50 major industries and, with two "principal firms" in each, 100 principal firms.

1. *Executive Compensation*. George Stigler has observed that the estimation of the effect of monopoly on profit may be complicated by the absorption of some fraction of "true" monopoly profit as cost. In particular, "the magnitude of monopoly elements in wages, executive compensation, royalties, and rents is possibly quite large" [28, p. 35]. Our interest here is limited to testing only a part of this hypothesis. Specifically, we examine the effects of discretion on compensating the top executive.

Focusing on a single representative of management might appear to restrict severely the relevance of our results. If the compensation of the rest of the management group were determined independently of that of the chief executive, this would certainly be the case. However, payments between executive levels are carefully scaled [3, p. 181] [15, p. 320] [25]. Hence, the factors that influence compensation to the top executive can be presumed to affect the level of staff compensation generally.

Under the utility-maximizing hypothesis, a positive expense preference towards emoluments exists. In particular, executive salaries should be correlated with the opportunities for discretion. Letting W_a be the actual salary of the management and W_c be the competitive salary, we have: $W_a = W_c + (W_a - W_c)$, where $W_a - W_c$ is a measure of the monopoly returns withdrawn by the management (by virtue of its advantageous position) as economic rent.

As indicated above, the hypothesis that discretion influences expenses takes the form $X = f(\pi)g(I_1, I_2, I_3)$ where $f(\pi)$ is the expense incurred strictly on a profit-maximizing basis, and I_1, I_2, and I_3 are indices of the absence of competition, the tastes of the management, and the diffusion of the stockholders respectively. Specifying $f(\pi)$ for purposes of studying executive compensation is somewhat difficult. A measure of hierarchical activity over which the executive in question has responsibility, together with the special abilities required for the position, probably measures this approximately. For the top executive, the level of hierarchical activity is effectively the entire staff structure. Thus let $f(\pi) = f'(S, Z)$ where S is the level of staff (general administrative and selling expenses)[12] and Z is an index of special ability.

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[12] Previous studies of executive compensation have used the total revenue of the firm for this purpose [17] [20]. This has the advantage of minimizing errors of measurement that arise from differences in accounting practice but is probably not as good a proxy for "staff" as is general administrative and selling expense. Sales are defective for two reasons. For one thing, they reflect activity at levels below the management hierarchy whereas the size effect would be expected to act largely within the hierarchy [25]. Secondly, interfirm comparisons are complicated by differences in vertical integration policies. As a matter of curiosity, regressions replacing general administrative and selling expense by sales were run—with uniformly adverse results. The objections to using gen-

We assume that the index of competitive pressure (I_1) is reflected by the concentration ratio and the entry barrier in each industry. The concentration ratio reflects the influence of realized interdependencies between rivals. Where concentration ratios are high, interdependencies will generally be intimate, and behavior between rivals will at least be circumspect and may involve explicit agreements. In either case, the influence of competition will be consciously controlled. Hence, an increase in the concentration ratio will tend to widen the opportunities for managerial discretion. Obviously, this measure is defective and there will be exceptions. However we are content merely to account for average rather than exceptional behavior.

The barrier to entry measure, as developed by Joe S. Bain [2], is explicitly designed to estimate the extent to which firms are insulated from the effects of competition. Although concentration and entry conditions are correlated, they are by no means identical. In combination they provide a particularly good measure of the opportunities for discretion. High concentration together with a high barrier to entry will tend to produce substantial discretion, for not only is potential competition limited, but existing rivals are few enough to appreciate their conditions of interdependence. Low values for each of these measures will tend to produce the reverse effect, while mixed values, presumably, give rise to mixed effects.

To allay any suspicion that the concentration ratio and entry barriers are merely another measure of size, it might be noted that the product moment correlations between the logarithm of sales and the logarithms of "staff," concentration, and barriers (for the firms included in the sample) are about .75, — .13, and — .14 respectively. Quite clearly these latter two correlations are small enough that if concentration and barriers have an effect on compensation, it is not primarily due to their relationship to size.

A sharp measure of managerial tastes (I_2) is not available. However, the composition of the board may act as a proxy measure of the extent to which management desires to operate the firm free from outside interference. Although low proportional representation of the management on the board of directors need not reflect a "taste" for active outside participation in the affairs of the firm, clearly a high internal representation does reflect the intent of the management to conduct the

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eral administrative and selling expense as a proxy for heirarchical expense are largely related to differences in accounting practice among firms. However, the components of general administrative and selling expense very nearly give us a measure of the level of staff activity in the firm (for a list of the functions usually included see [31, Chs. 17 and 18]). Amounts charged to these accounts are, for the most part, current costs, and hence ambiguities arising from the use of historical costs are reduced.

affairs of the firm free from such outside influence. We hypothesize that, as the management representation on the board increases, there tends to be a subordination of stockholder for managerial interests. In this sense, the composition of the board reflects management's attitude toward discretionary resource allocations and a voluntary change in composition reflects a change in these "tastes."

An estimate of stockholder diffusion (I_3) was not obtained. Such a measure would probably be correlated with the composition of the board variable. However the association may not be great. Where substantial concentration of ownership exists, there is frequently a tendency towards nepotism. This in turn may produce high internal representation rather than the high outside representation that would otherwise be predicted. If in fact the correlation were zero (and there were no other neglected variable to consider), our estimate of the composition effect would be unbiased. As it is, some bias may result from the lack of a diffusion measure.[13]

The effects of each of the independent variables on executive compensation should be positive. In addition, they are assumed to be multiplicative. Thus we assume that:

$$X_i = \alpha_0 S_i^{\alpha_1} C_i^{\alpha_2} H_i^{\alpha_3} B_i^{\alpha_4} U_i \tag{6}$$

where

X_i = compensation of the top executive
S_i = administrative, general, and selling expense (i.e., "staff")
C_i = concentration ratio in the industry
H_i = height of the barrier to entry in the industry
B_i = composition of the board
U_i = a random error term[14]

and the subscript i refers to the i^{th} firm in the sample.[15]

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[13] Alternatively, the composition of the board variable might be interpreted as reflecting the *joint* effects of management tastes and stockholder diffusion. What is really needed, however, are sharper measures that reflect each of these effects separately.

[14] U_i includes the effects of special abilities (the Z variable mentioned above), the omitted stockholder-diffusion variable, numbers of years the top executive has held that position, and other neglected factors.

[15] The number of observations for each of the years was 26 in 1953, 30 in 1957, and 25 in 1961. Inability to use all 52 of the firms studied (as in the second set of tests reported here) was largely due to the lack of estimates on the condition of entry for many of the industries. The sources of the data were as follows:

Executive compensation: as a matter of law, publicly held corporations are required to report executive compensation to the Securities and Exchange Commission. Although these data are a matter of public record, they are not readily available. *Business Week,* however, annually publishes executive compensation figures for a group of principal firms. It was from this source that the data on compensation were obtained.

General administrative and selling expense and composition of the board: both were obtained from *Moody's Industrials* (supplemented occasionally by other sources when the listing of officers in *Moody's* was incomplete).

Taking logarithms of both sides of the equation and using these data to obtain least-squares estimates of the net regression coefficients, we obtain the results shown in Table 4.

The signs for each of the parameters in all three years are as predicted by the expense-preference hypothesis. Moreover, with the exception of the composition of the board coefficient, which is significant at the 10 per cent level only in 1957, all of the regression coefficients are highly significant—two-thirds being significant at the 2.5 per cent level.[16] Whereas the relation of executive compensation to general administrative and selling expense (i.e., "staff") is almost certain to be positive and significant, there is no reason to believe that the measures of taste and discretion that we introduce should have the effects shown (unless one endorses the view that management responds to opportunities for discretion in the ways indicated). Since the compensation of the chief executive generalizes to the entire staff structure, these results have broad significance for the resource-allocation process within the business firm. Furthermore, we would expect that these same measures of discretion would produce similar effects over the entire range of expenditures on emoluments.

Concentration ratios: data on concentration for 1953 were developed from the 1954 concentration ratios for the four largest firms reported in *Concentration in American Industry* (Washington, D.C., 1957). Concentration data for 1957 and 1961 were developed from the 1958 concentration ratios for the four largest firms reported in *Concentration Ratios in Manufacturing Industry 1958,* Pt. I (Washington, D.C., 1962). A weighted average of several of the SIC industry groups was sometimes used to arrive at a concentration ratio for the industries in question. Although such weighting procedures can produce distortions, this is probably not too serious in reasonably narrowly defined industry groups.

Barrier to entry: Bain's study [2] provided the estimates of the height of the barrier to entry. In addition, I took the liberty of classifying textiles as an industry with a low entry barrier since Bain did not include textiles in his analysis, but there is general agreement that the industry has a low entry barrier. A dummy variable which took on the value 1 ($\ln 1 = 0$) when the barrier to entry was low and e ($\ln e = 1$) when the barrier was high or substantial was used in the regressions. Two dummy variables to represent the substantial and high entry conditions separately were also tried. Although one might suppose that the parameter for the high-barrier dummy would exceed that of the substantial-barrier dummy, the results were somewhat mixed. As I have suggested elsewhere, however, the principal difference between a substantial and high barrier may be that in the former case the firm expands selling expense beyond its optimal level in order to discourage entry, with the result that the effective condition of entry is the same in substantial- and high-barrier industries [35]. The question requires additional empirical investigation.

A list of the firms and industries included in the executive compensation and retained earnings analyses can be obtained from the author upon request.

[16] The tests are one-tailed tests, which are appropriate since the hypothesis specifies that the signs should be positive (which they are). The standard errors shown are corrected for the finite population correction $\left(\frac{N-n}{N}\right)^{1/2}$, where N is 100 in all years and n is 26, 30, and 25 for 1953, 1957, and 1961 respectively.

Whether the results apply to a larger group than these 100 principal firms remains a subject for subsequent investigation.

Table 4—Regression of Executive Compensation on "Staff," Concentration Ratio, Composition of the Board, and Barriers to Entry

	Year		
	1953	1957	1961
"Staff"			
Coeff.	.228[a]	.240[a]	.218[a]
S.E.	.061	.052	.054
Partial	(.564)	(.610)	(.614)
Concentration			
Coeff.	.503[a]	.513[a]	.422[b]
S.E.	.157	.143	.152
Partial	(.517)	(.517)	(.470)
Composition			
Coeff.	.137	.139	.053
S.E.	.118	.101	.120
Partial	(.213)	(.224)	(.084)
Entry Barriers			
Coeff.	.446[a]	.221[b]	.200
S.E.	.110	.114	.126
Partial	(.606)	(.307)	(.290)
Coeff. of Correl. (adjusted)	.786	.724	.687

[a] Significant at the 0.1 per cent level

[b] Significant at the 2.5 per cent level

Of course it could be argued that the concentration ratio and entry-barrier variables have positive regression coefficients because they are correlated with the profit rate—that this profitability effect is responsible for the results obtained. But obviously the causality runs from concentration and entry barriers to profits rather than the reverse. Thus, by focusing on the market structure, the model directs attention to the ultimate determinants of discretionary behavior (competition in the product market) rather than the apparent determinant (the profit rate). Although these market variables might not perform as well as the profit rate among the smaller firms in the industry, it does not seem inappropriate to use them for studying the behavior of the two largest firms where the relationship between market structure and behavior is probably reasonably direct. Indeed, it is of interest to note that: (1) if the profit rate on the stockholders equity is substituted for the concentration ratio and entry-barrier variables, the coefficient of determination (R^2) falls to two-thirds of the value obtained using these market variables in 1953 and 1961, and yields less than a ten per cent increase in R^2 in 1957; (2) if the profit rate, concentration ratio, and entry-barrier variables are all included, the profit rate is significant only in 1957 and has the wrong sign in 1961, while the concentration ratio and entry-barrier variables remain significant at the ten per cent level or better in every year.

Although the profit rate might perform better if a weighted average were used instead of current values, the argument offered above that this is an apparent rather than the ultimate determinant of behavior still applies. Moreover, the appropriate estimate of the profit rate is the actual rather than the reported rate. But the actual rate is unknown if, as the evidence above suggests, some fraction of actual profits is absorbed as salary and perquisites.

Some feeling for the responsiveness of salary to the independent variables in the regression equation can be obtained by taking the median of the estimates for each parameter and finding the effect on salary of increasing each individual independent variable by a factor of two. In some gross sense we can expect that executive salaries will possibly increase on the order of 17 per cent if the level of staff activity were to double, on the order of 41 per cent if the concentration ratio in the industry were to double, on the order of 10 per cent if the internal representation on the board were to double, and on the order of 25 per cent if the industry of which the firm was a part had a substantial or high barrier to entry rather than a low one. Thus, not only are the signs as predicted by the theory, but the magnitudes are sufficiently large to render somewhat doubtful the contention that discretionary effects are unimportant.

2. *Earnings Retention.* The composition of the board variable was used in the executive compensation model to reflect the "tastes" of the management for discretion. Internal representation on the board acts as a proxy for the attitude of the management towards outside influence. As the proportional representation of management on the board increases, it is assumed that stockholder interests tend to be subordinated to managerial objectives. This was manifested in the executive compensation regression by the positive regression coefficient associated with the composition of board variable.

A second test for this effect is to examine the relationship between composition of the board and earnings-retention policy. Consistency with our model requires that the earnings-retention ratio be directly related to the composition of the board. This follows since retained earnings are a source of discretion and a high internal representation provides the opportunity for management to shift the dividend policy to its advantage.

Alternative theories of the firm that regard managerial objectives as unimportant implicitly predict that there will be no association between the composition of the board and retention policy. Thus, our hypothesis of a direct association is tested against the null hypothesis of no association.

Earnings retention will, of course, be responsive to a number of considerations other than that of the composition of the board. Most important, investment opportunities will differ between industries and these could easily be overriding. If it can be assumed that the firms in the same industry have identical opportunities, however, these effects can be neutralized.

A paired-comparison technique was used to neutralize the industry effects. That is, between the two principal firms in each of the 26 industries we compare the composition of the board and earnings-retention ratio. The random variable can take on either of two values: 1 if the higher internal representation is paired with the higher earnings-retention ratio, and 0 otherwise. Hence it is distributed as a binomial. Under the hypothesis that no association exists, the expected number of times the positive association will occur, divided by the total number of observations, is one-half. Thus the null hypothesis is that the binomial parameter p is .50. Our model, however, predicts that the positive association will occur more than one-half of the time—i.e., that p exceeds .50.

TABLE 5—BINOMIAL TEST FOR ASSOCIATION BETWEEN COMPOSITION OF BOARD AND EARNINGS-RETENTION POLICY

	1953	1957	1961	All Years
Number of observations	25	26	26	77
Expected number of positive occurrences under the null hypothesis	12.5 ($p=.50$)	13 ($p=.50$)	13 ($p=.50$)	38.5 ($p=.50$)
Actual number of positive occurrences	13.5 ($\hat{p}=.54$)	16 ($\hat{p}=.62$)	18 ($\hat{p}=.69$)	47.5 ($\hat{p}=.62$)
Probability that a value as high as observed would occur if the null hypothesis were true[a]	.34	.13	.02	.02

[a] Normal approximation to the binomial was used to obtain the probabilities that the null hypothesis would produce the results observed.

The results for each of the three years as well as the pooled results for all three years are shown in Table 5. The proposition that internal representation has no effect on the earnings-retention policy between pairs of firms in the same industry is unsupported by the data. In every year the proportion of positive observations exceeds .50. In 1953 and 1957 the probability that a value as high as that observed if the null hypothesis were true is .34 and .13 respectively, and in 1961 this drops to .02. Clearly we are inclined to reject the hypothesis in favor of the alternative suggested. That is, due to the discretion associated with the retention of earnings and the opportunity to influence the retention policy which arises from representation on the board, the relation that we suggested (namely, that between pairs of firms in the same industry, the higher the internal representation, the higher the earnings retention rate) is supported by the data. Although it is possible that the composition of the board is acting only as an intervening variable and that the real explanation for this association lies elsewhere, no simple connection suggests itself.

The strongest evidence in favor of our hypothesis is provided by the pooled results for all three years. Here the observed number of positive occurrences would appear by chance under the null hypothesis with a probability of only two times in a hundred. Before the pooling of the observations can be justified, however, it is first necessary to establish that the observations are independent and that the association observed in one period is simply not carried over to the following period. Since the composition of the board and earnings-retention decisions reflect policy considerations that exhibit continuation in consecutive years, lack of independence between consecutive years would be expected. On the other hand, our observations are separated by a period of four years. The association between consecutive years may well be eliminated over this interval. Since the issue can scarcely be resolved on a priori grounds, we submit the hypothesis that the observations are independent to test.

A chi-square test for association was used. A low value of χ^2 is consistent with the hypothesis that the observations between successive four-year intervals are independent. The value of χ^2 between 1953 and 1957 is .0065, and between 1957 and 1961 is .62. Sampling randomly from independent populations, values as high or higher than this would occur 95 per cent and 45 per cent of the time respectively. Hence the hypothesis of independence is supported, the pooling of the observations is justified, and the best test for the composition of the board effect is that of all three years combined. Here the possibility that the positive association observed has occurred by chance is only .02. Indeed, among pairs of principal firms we can expect that the firm with the higher internal representation on the board of directors will have a higher earnings-retention ratio about three-fifths of the time.

The above results are limited to directional effects only and say nothing about the magnitudes involved. This is probably all that the data justify. However, a crude estimate of the quantitative effect is available by an application of the general model suggested above for studying discretionary expenditures. Thus let

R_{ik} = the retained-earnings ratio
ρ_k = the rate of return on investment available to principal firms
C_k = the concentration ratio
H_k = the entry barrier
B_{ik} = the composition of the board of directors
V_{ik} = a random-error term[17]

The subscript i refers to the firm, and the subscript k refers to the industry of which the firm is a part. Then, assuming the relation is multiplicative, we have:

$$R_{ik} = \beta_0[f(\rho_k)]^{\beta_1} C_k^{\beta_2} H_k^{\beta_3} B_{ik}^{\beta_4} V_{ik}. \tag{7}$$

Taking the ratio of retained earnings between the ith and jth principal firms in the same industry yields:

$$\frac{R_{ik}}{R_{jk}} = \left(\frac{B_{ik}}{B_{jk}}\right)^{\beta_4} V'. \tag{8}$$

Taking logarithms of both sides of the equation, the value of β_4 can be estimated by least squares. The resulting estimates for 1953, 1957, and 1961 are .17, .17, and .16 respectively, but only the 1957 estimate is significant at the 10 per cent level.[18] These estimates suggest that the retained-earnings ratio would increase by about 12 per cent if the internal representation on the board of directors were to double.

A tenuous connection between the composition of the board and the investment policy of the firm can be obtained by noting the results obtained by Myron Gordon and M. Fg. Scott in their recent studies of investment financing. Gordon remarks that "The really surprising result is produced by return on investment. . . . In both industries there is a statistically significant tendency for the retention rate to fall as the corporation's rate of return increases. We must conclude that either

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[17] Neglected variables that may influence the retained-earnings policy include liquidity measures (such as the current ratio) times interest earned and other financial variables. Among principal firms in the same industry, such measures tend to display substantial stability.

[18] Since the estimates are sensitive to extreme values of retained earnings (values of R_{ik} greater than .95 or less than .05), and since such extreme values ordinarily represent a transitory condition, these extreme values were removed in making the estimates. Thus the estimated values of β_4 apply to the range of retained earnings between 5 and 95 per cent.

Actually, there is little theoretical reason for including the product market variables in the retained-earnings regression. They are included primarily for the purpose of indicating how variables common to both firms can be eliminated by using the ratio device. It is of some interest to note, however, that the estimate of β_4 obtained using only those industries with a high concentration ratio (> 50), exceeds that obtained from industries with a low concentration ratio (≤ 50) in all three years.

[our estimate] is a poor measure of rate of return on investment or that corporations are not primarily influenced by the price of their stock in setting dividend rates" [11, pp. 231-32]. And Scott, in a somewhat more broadly based study of dividend policy, observes that the "negative correlation of −.30 between undistributed profits . . . and the subsequent growth of earnings . . . is somewhat surprising. It suggests that stockholders . . . might benefit from more generous dividend distributions" [24, p. 244]. For a theory that makes the firm's objectives identical with those of the stockholders, such a result is somewhat disquieting. For an approach such as ours, however, which allows for the subordination of stockholder to managerial objectives, a possible explanation for these results based on the composition of the board analysis can be easily provided.

As was suggested above, high internal representation on the board of directors favors attention to managerial objectives, and this is manifested in a high earnings-retention rate. The funds thus provided are available to the management for the pursuit of expansionary objectives, and the resulting investment, being based on a combination of profit and expansionary goals, will exceed the amount dictated by profit considerations alone. As a result, the average rate of return in firms whose management is inclined to subordinate stockholder objectives can be expected to fall below that in firms where management interests are more nearly those of the stockholders.[19] Thus the tastes of the management, as revealed originally in the composition of the board, make their influence felt through the earnings-retention policy and thence on the return on investment. Where these tastes favor expansion, there is an adverse effect on the rate of return on investment. This indirect implication of our theory is precisely the result that Gordon and Scott report. Although conjectural, it suggests the value of including a "taste" variable, of which the composition of the board is a somewhat imperfect proxy, in future studies of the investment decision.

B. *The Evidence from the Field Studies*

Simon has pointed out that "neither the classical theory of the firm nor any of the amendments to it or substitutes for it . . . have had any substantial amount of empirical testing" [27, p. 8]. To remedy this he offers several proposals, one of which is the intensive interview. This has the advantage of permitting detailed observations that are unavailable in the ordinary survey, and these may provide insights into the ways in which the firm perceives its problems and the processes it employs in responding to them.

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[19] In addition to the quantity of funds invested and diminishing-rate-of-return effect, there may also be political influences to consider [9]. As the amount of available resources increases, the importance of political relative to economic criteria will tend to increase. Any such a shift toward political considerations naturally has an adverse effect on the rate of return on investment.

Unfortunately, field study observations are difficult to summarize. Their relevance derives largely from their detail, and, since the observations are few in number, statistical tests are often inappropriate. The field studies reported here are precisely of this kind. They nonetheless produce insights that would be difficult to obtain by other means. Of principal interest from our studies of the response of firms to adversity are the following:

1. In the face of a sharp drop in profitability, hierarchical expenses typically undergo extensive curtailment. One firm, after a long period of operating in a seller's market, responded to a sharp fall in profits with the following adjustments [33, pp. 5-11]: (a) salaried employment over the entire organization was reduced by 32 per cent; (b) headquarters employment was reduced by 41 per cent; (c) the research and development staff was reduced from 165 personnel to 52 and much of its work was redirected to commercial R and D organizations; (d) the personnel and public relations staff was streamlined from 57 to 7; (e) a general reduction in emoluments of all kinds was realized. All this occurred with production unchanged. Return on investment over the interval was increased from the 4 per cent level to which it had fallen to 9 per cent. Further cutbacks in some areas are expected; additions are contingent on changes in volume and are tied to a new set of long-range plans.

Both the type and magnitude of these reductions suggest that the managers were operating the firm so as to attend to other than merely profitability goals in the period preceding the earnings decline. Invoking the notions of expense preference and discretionary spending makes it possible to provide an uncomplicated explanation for the adjustments observed.

2. The philosophy of management in instituting cutbacks is of particular interest. The chief budgeting officer in one organization made this observation [33, p. 13]:

> In any large organization, certain plants or departments will have found ways to habitually operate more efficiently than others. This may be due to *competitive pressure* which has historically been felt in some products to a greater extent than others. It may be due to differences in *individual management philosophy*. . . . It follows . . . that any approach toward an arbitrary management dictate for an across-the-board slash in all cost areas will inevitably damage necessary functions in some areas, and leave remaining inefficiencies in others.

As a result, cost reductions were tailored to the individual divisions —taking their competitive history and management philosophies into account. Whereas such behavior is consistent with the managerial model, it is less clear that it should occur in a profit-maximizing organization.

Related observations of interest were the way in which headquarters overhead was allocated to achieve the effects of a lump-sum tax [33, pp. 53-58] and the discretion that the division management was permitted in the allocation of any earnings in excess of the assigned profit goal [33, pp. 26-28]. In both respects, the behavior observed is readily accommodated by the managerial model but is not easily explained by the profit-maximizing hypothesis. In addition, the business literature abounds with descriptions of behavior that generally conform to those cited above.[20]

The detail revealed by the field studies, like the relationships found from the principal-firm analyses, suggests that, in order to explain and predict what appears to be a nontrivial range of business behavior, it may be necessary to make managerial objectives an integral part of the analysis. To treat them otherwise is to require *ad hoc* explanations for behavior which, broadly conceived, may be entirely rational and hence subject to systematic analysis and routine explanation.

III. Application to Regulated Industries

Armen Alchian and Ruben Kessel have recently argued that the presence of a regulatory constraint in the form of a maximum allowable rate of return tends to encourage expenditures on emoluments and other items that yield managerial satisfactions [1]. That is, if above-normal profits cannot be long continued and if supernormal profits will almost certainly invite the early attention of the regulatory commissions, the management of a regulated firm has an incentive to hold profits at or below some "safe" level by absorbing profits through expanding satisfaction-producing expenses. Alchian and Kessel argue that the behavior of these firms is best analyzed by substituting a general preference function for profits.

Although they do not formalize their argument, the effects they describe appear to be largely in accord with those obtained from the model that we have proposed. Thus, if the utility-maximization model is augmented to include a maximum-profit constraint, it is easily shown that, when the firm encounters the region bounded by the constraint, the profit component in the utility function becomes fixed at this allowable maximum value, and expenditures on staff and emoluments will be increased to assure that this condition is not violated [34]. This is precisely the behavior they describe. Rather than generalize their argument, however, they restrict the application of their analysis to regulated industries (or others similarly confronted by a maximum-profit constraint—such as firms facing potential antitrust action). In all other circumstances, they claim, competition in the product market or competition in the capital market will remove any opportunities for such nonprofit behavior [1, p. 160].

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[20] For recent surveys on such behavior, see [10] [29] [32].

The position that competition in the product market will render impotent any tendencies to promote nonprofit expenditures is quite unassailable. The mechanism of natural selection enforces conformance to the profit-maximizing norm. However, their belief that, in the absence of competition in the product market, the capital market will assign monopoly powers "to those who can use them most profitably" [1, p. 160] lacks an equally efficacious enforcement mechanism. It requires that effective control of monopoly power reside with the stockholders and that this be transferable through financial (capital market) rather than by political (managerial) processes. It has been widely recognized, however, and it has been the force of our argument and evidence above, that the management, and not the stockholders, is in effective control of the monopoly power in the business firm,[21] and the transference mechanism is one of executive ascension rather than financial exchange. Subject to loose performance constraints imposed by the capital market (both the stockholders and the firm's creditors), the management is largely free to exercise the monopoly power that the firm possesses at its own discretion. Thus, while we fully agree with the Alchian-Kessel discussion on nonpecuniary motives and their suggestion that profits be replaced by a general preference function, we would suggest that regulated industries are merely a special case of the general case where competition in the product market—for reasons of concentration, conglomerate bigness, or barriers to entry—is weak.

IV. *Conclusions*

Based on the twin assumptions of self-interest and rational behavior, a general approach for introducing managerial objectives into a theory of the firm has been suggested. The notion of expense preference constitutes a critical part of the argument. It provides the essential connection for relating managerial objectives to operating behavior.

In addition to the comparative statics properties that were investigated, the managerial model also provides identical qualitative responses to those of the profits-maximizing model with respect to a sales tax (of either the specific or ad valorem variety). Thus the utility-maximization hypothesis preserves the main theorems of the profits-maximization hypothesis with respect to shifts in demand and application of a sales tax. Indeed, since there is little dispute concerning the general validity of these implications of the classical theory, it would be distressing to have the managerial model predict differently. However, when it comes to matters where the qualitative implications of the profits-maximizing model have been somewhat suspect, namely the effects of a profits tax and a lump-sum tax, the managerial model registers responses that contradict the classical theory.

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[21] Edward S. Mason, for example, takes the position that "almost everyone now agrees that in the large corporation the owner is, in general, a passive recipient; that typically control is in the hands of management; and the management normally selects its own replacements" [19, p. 4]. See also [12, pp. vi-x] and [21].

The evidence presented is clearly suggestive rather than definitive. Such as it is, it generally supports the implications of the utility-maximization approach. Although it is not strong enough to provide a discrimination between the utility- and profits-maximizing theories, it does suggest that either firms are operated as indicated by the managerial model or, if "actual" profits are maximized, that reported profits are reduced by absorbing some fraction of actual profits in executive salaries and possibly in perquisites of a variety of sorts. This raises a serious question whether studies of monopoly power based on reported profits provide an accurate estimate of the effects of monopoly. It is possible that a nonnegligible part of true monopoly profits is absorbed internally.[22]

If subsequent results confirm the present findings concerning the effects of internal representation on the board of directors on executive compensation and dividend policy, the case for an independent board becomes much more compelling. Although Gordon has already argued this position persuasively [12, pp. 343-51], the reasoning has lacked empirical support and there is little indication that his views have been heeded.

A continuing investigation of the effects of discretion on managerial behavior would appear to be warranted. Indeed, we could not agree more with Becker's view that the economist *can* provide nonpecuniary motives with economic content and that "progress in this field has been hindered not so much by an intractable concept as by the economists' reluctance to take the concept seriously" [8, p. 179].

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[22] The results obtained by Gary S. Becker in his study of the effects of monopoly on discrimination [7] also support this proposition.

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SHORT CYCLES IN CORPORATE DEMAND FOR GOVERNMENT SECURITIES AND CASH

By ERNEST BLOCH*

It is curious, to say the least, that most empirical studies of money-holding behavior are based on annual data, and that the conclusions drawn are for the long run. One would suppose that the most liquid of all assets—namely money—would be sensitive to sharp short-run cycles in economic activity, or that at least it would be interesting to find out if it were. Yet only recently have there appeared studies of monetary velocity using quarterly rather than annual data [15, p. 123] [12, pp. 1557-70].

Another relatively unexplored area in the literature—until recently—is a sector-by-sector examination of monetary behavior, rather than an over-all, economy-wide approach.[1] The economy-wide approach would perhaps be appropriate in a situation in which the several sectors have available to them only one asset serving the several monetary functions. However, in view of the postwar growth in volume and in variety of near-money assets that can be used as money substitutes, a sectoral approach has probably become essential for short-term or long-term analysis. For example, the upsurge in holdings of savings and loan shares is essential to an understanding of consumer behavior and of little worth in a study of corporate cash management. For corporations, the rapid growth in volume of short-term government securities (and of privately issued money-market instruments) has induced far greater

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* The author is associate professor of finance at New York University. The major part of this study was written while the writer was on leave from the Federal Reserve Bank of New York under the Bank's program for advanced education. The author would like to acknowledge the helpful discussions and comments of George Garvy, Robert Lindsay, and Ralph Schindler of the Federal Reserve Bank of New York; of Robert Bethke of the New York Discount Corporation; of Lawrence S. Ritter of New York University; of Jack M. Guttentag of the University of Pennsylvania; and of Professors Adolf Lowe, Alfred Kahler and Hans Neisser of The New School for Social Research. The customary rules governing acknowledgments apply: those named above contributed ideas of their own that may or may not represent the views of their institutions; and whatever errors of fact or interpretation remain are the sole responsibility of the author. The author also wishes to acknowledge the help of Howard Hendricksen and Philip Krackow in the design and execution of the charts.

[1] For recent exceptions see Selden's study [18], which uses annual data, and McGouldrick [12] for a quarterly treatment of corporate and individuals' sectors.

changes in their money-holding patterns than in the behavior of consumers.[2]

The following is an attempt to fill the gap among the available studies of monetary behavior by studying the short cycles in liquid-asset holdings of one major sector, namely, the nonfinancial corporations. The corporate sector was chosen partly because nonfinancial corporations as a group have become the largest holders of demand deposits in the economy.[3] In addition, holdings of government securities also qualify corporations as important money-market participants: at the end of 1961, their holdings constituted about one-fifth of total marketable issues in the hands of the nonbank public. Further, the volume of corporate trading activity in government securities has been so high that corporate turnover in 1958 was ranked immediately behind commercial banks and other financial institutions but ahead of the Federal Reserve and U.S. government securities dealers [22, p. 51].

In the postwar period, government securities have become the major credit instrument through which corporations have "economized cash" by holding a minimum of liquid assets in the form of currency and deposits. With cash balances being compressed as much as possible, the asset function of money has been taken over by short-term securities that bear minimal risk and are so readily convertible into cash as to properly deserve the name "near-money." Indeed, this study will indicate that short cycles in corporate holdings of cash and near-money securities were reflected primarily in the variations of the latter.

I. *Corporate Liquidity: Major Holders*

Virtually any analysis of the corporate sector involves close study of the activities of large corporations. As Mason [11, p. 5] has written, "the one-hundred-and-thirty-odd largest manufacturing corporations account for half of manufacturing output in the United States. The five hundred largest business corporations in this country embrace nearly two thirds of all nonagricultural economic activity. . . . By now we are all aware that we live not only in a corporate society but a society of large corporations." The concentration of corporate activity in the largest units applies even more to financial than to "real" activities, and most particularly to the holding of liquid assets.

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[2] While a number of studies now available have given some attention to holdings of near-money assets, notably Heston [5, and sources cited there], most prior work has followed the example of Keynes' *General Theory* in neglecting all liquid assets but cash. Keynes, however, explicitly stated that his analysis would *exclude* holdings of money-market assets, such as Treasury bills [7, pp. 166-67]. Keynes' simplified model was appropriate for the 'thirties, when the bulk of short-term money-market instruments had been paid off. It is not appropriate for the postwar period.

[3] According to the most recent survey of ownership of demand deposits compiled in early 1961, these corporations held $41 billion of deposits out of a grand total of $109 billion. See [20, p. 405].

Consider, for example, the distribution of holdings of government securities (hereafter called simply "governments") between manufacturing corporations[4] with assets of less than $100 million—let us call these the "smaller corporations"—and those with assets of $100 million or more, called "large" below.[5] As the solid lines in Chart 1 indicate, the holdings of the "smaller" units, shown in the lower panel, have been virtually stable, and what little shifts they undergo may be regarded as having a primarily seasonal association. On the other hand, the governments held by the large corporations (upper panel) are much larger and fluctuate substantially. More specifically, in the early postwar period, holdings of both groups were in the neighborhood of $3 billion, but holdings of the small corporations have generally remained at that level, while governments in the hands of the large firms have exceeded $8 billion most of the time, with occasional peaks reaching $10 to $12 billion. There is a sharp contrast between stability of holdings by smaller units and substantial fluctuations in the holdings of large firms.

Next consider the relation between tax liabilities, shown as a broken line on the chart, and holdings of governments, for both groups. The smaller corporations managed to boost their holdings of governments to parity with their tax liabilities only by 1959, while in previous periods a 50 per cent "coverage" had been typical. But throughout the period a nearly perfect intra-year coordination of changes in tax liability and of government holdings of the smaller corporations also imparted to their holdings of governments a substantial degree of seasonality.

For the large manufacturing corporations, on the other hand, the direction of changes of holdings of governments and of tax liabilities is not always the same. And what is perhaps most important, the governments held by the large corporations have always exceeded tax liabilities, although by varying margins. These margins between holdings and tax liabilities have been as high as $4 billion and at times have dropped virtually to zero.

The acquisition of government securities by the smaller and the large corporations as a counterpart to their federal tax accruals is readily

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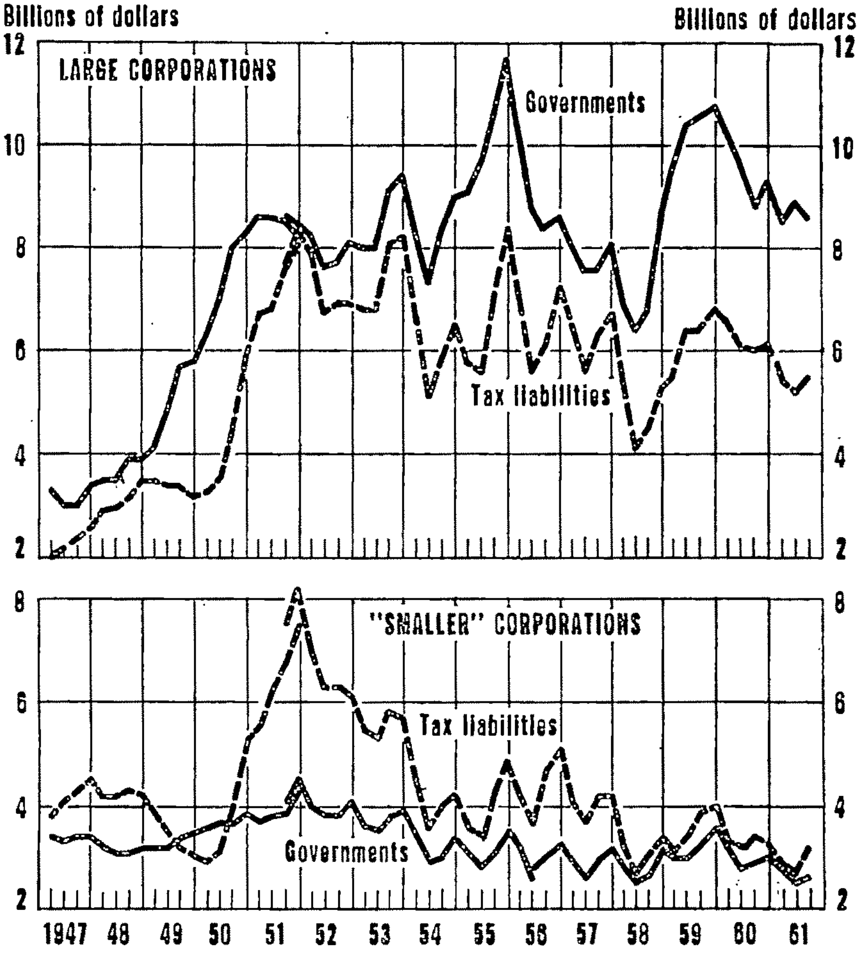
[4] All data on manufacturing corporations used below have been taken from [23, 1947-61].

[5] The large preponderance of corporate holdings carry short maturities of one year or less. A number of studies of postwar holdings have indicated this, and the Treasury's *Survey of Ownership* in [26] of corporate holdings, available since early 1960, bears this out on a month-by-month basis as well.

CHART 1. QUARTERLY HOLDINGS OF GOVERNMENTS AND TAX LIABILITIES OF LARGE AND "SMALLER" MANUFACTURING CORPORATIONS*



* Large corporations are defined as having assets of $100 million or more; "smaller" corporations are those with assets below $100 million.

Source: U.S. Federal Trade Commission and Securities and Exchange Commission, *Quarterly Financial Reports for Manufacturing Corporations.*

explained by the earnings potential of these securities. For $100 of tax accruals placed at 3 per cent will, even after corporate taxes on the interest, earn about $1.50 per year, and reduce a $100 tax-payment owed by the firm to $98.50.[6] The secular rise in Treasury bill rates in the postwar period helps to explain the improving tax-liabilities "coverage" in holdings of government securities of even the smaller firms shown in Chart 1. Further, since the interest cost of bank borrowing for tax payments also has risen secularly, it has become general practice for a growing number of firms, whether large or small, to accrue taxes "in advance" through holdings of government securities rather than pay taxes with proceeds of costlier bank loans, the repayment of which means in fact that taxes are accrued in retrospect. A fairly simple set of interest-rate relationships can explain the "economization of cash" for tax accruals or even the choice between such tax accruals and tax borrowing for large and smaller corporations.

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[6] In fact, the phenomenon of interest rates moving in phase with the business cycle tends to produce a softening of cyclical tax pressures on corporations as they move into the boom. Let us assume that the average and marginal corporate tax rates are 50 per cent (which is close enough to reality for the large firms), and that Treasury bill rates

Changes in portfolios of government securities held above and beyond tax liabilities by the large corporations are not subject to such simple interest-rate relationships, however, as the following section will indicate.

II. *Short Cycles in "Free" Governments*

The holdings of governments above and beyond tax liabilities will be called "free" governments. It will be argued that these governments serve as a kind of "secondary liquidity reserve" for the large manufacturing corporations in a manner analogous to the role played by the holdings of short-term governments in the portfolios of commercial banks.[7]

Holdings of free governments should be kept conceptually and functionally distinct from the holdings of "tax governments" or other types of liquid assets held for similar payments—e.g., bond interest or ground rents—that are substantial and predictable. For holdings of liquid assets to meet such lumpy and foreseen purposes tend to fluc-

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move between 2 per cent and 4 per cent from recession to boom. Then $100 of corporate taxes accrued (for a year) in bills provides the Treasury with a net tax yield of $99 in recession and of $98 in the boom. For the corporations, effective tax rates thus *drop* from about 49½ per cent in recession to 49 per cent in the boom. If bill rates rise by more than the 2 per cent spread used above, the effective tax-rate reduction in the boom is greater. But by the same token, if bill rates tend to drop more in recession, the effective corporate tax bite tends to rise! On the other hand, the reverse tends to be the case for corporations that pay taxes through bank borrowing. Their "total effective" tax rate tends to rise in the boom as interest rates rise on bank loans, and fall in recession.

[7] The term "free" governments follows usage in prior work by Kisselgoff [8] and Lutz [10]. It should be noted that some free governments are also held by nonmanufacturing corporations. An analysis of the over-all corporate sector on the basis of SEC data given in its release [25] shows that for the postwar period the free governments held by 300 large manufacturing firms were about on a par with those held by the remaining 750,000 nonfinancial corporations put together. But while the free governments held by the large *manufacturing* firms underwent readily identifiable short cycles, those held by "all other" firms underwent basically seasonal shifts.

tuate, by and large, with the volume of that specific set of payments and the efficiency with which a corporation can identify them in advance. The holdings of free governments, by contrast, are not associated with the rate of flow of such a specific type of transactions. Changes in holdings of free governments are likely to be related instead to the less foreseeable interaction of major corporate sources and uses of funds. As a result, the time lag between accumulation and decumulation of free governments tends to be greater and more variable than that for lumpy and foreseen payments. Finally, because free governments carry short maturities almost exclusively, the term is therefore meant to suggest the free availability (for purchase or sale) of these near-money assets.[8]

The characteristic pattern of the free governments held by large manufacturing firms is a cyclical one. This pattern can be demonstrated by considering such holdings (in Chart 2) during periods of economic decline and periods of advance. In each recession, save the last, an upsurge in holdings began during the first downturn quarter. The build-up period continued through the recession and for a period of about one year in the recoveries that followed. Thus the accumulation periods were about completed in late 1950, mid-1955, and early 1959, respectively. Similarly, sales of free governments were about as rapid as their accumulation in the first two cycles and took about six quarters in each boom period. The business upturn of 1959-60 was unusual in that there were few sales of free governments after the $4 billion level was reached. While we shall indicate below the changed character of the corporate financing cycle and of liquid-asset holdings during the 1959-61 period, it may be observed at this point that the patterns of corporate financing and of holdings of free governments are interlinked: because the 1959-60 recovery in the rate of business activity was modest, holdings of free governments displayed stability in contrast with sharp declines in the two prior cycles.

Why did holdings of free governments fluctuate in these cycles? Were corporations induced to "economize cash" because of interest yields and did they *change* their holdings in response to the substantial cyclical *changes* in Treasury bill rates shown in the chart? More precisely, do free governments rise as interest rates rise over the cycle, and are they at their highs when the peaks of interest cycles are reached? And do they drop when rates drop, reaching their lows in interest-rate troughs?

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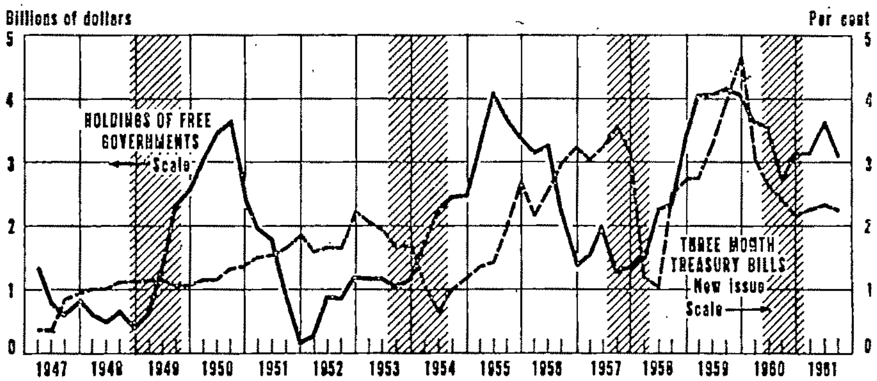
[8] Although the purpose of accumulating near-money assets to pay later for a known amount of taxes or bond interest may be distinguished from the more general purpose of accruals of free governments, the actual assets held for both purposes could be the same credit instruments (say, Treasury bills). This is, of course, in line with Keynes' liquidity-preference notion [7, p. 195] that "money held for each of the three purposes forms, nevertheless, a single pool, which the holder is under no necessity to segregate into three watertight compartments. . . ."

CHART 2. QUARTERLY HOLDINGS OF FREE GOVERNMENTS OF LARGE MANUFACTURING CORPORATIONS AND TREASURY BILL RATES 1947-61



Note: Shaded areas represent reference cycle contractions according to the chronology of the National Bureau of Economic Research.

Sources: U.S. Federal Trade Commission and Securities and Exchange Commission; Board of Governors of the Federal Reserve System.

There is no interest-rate relationship that can consistently explain either the level of holdings or changes in holdings. The first upsurge in free governments started in the first recession quarter (IV '48), and holdings rose by $3 billion, reaching a peak at the beginning of the Korean boom (III '50); during that upsurge, Treasury bill rates drifted both up and down in a narrow range between 1 and 1⅜ per cent. Indeed, the chart shows that the first short cycle, with the sharpest movement in free governments, took place in a setting of nearly stable Treasury bill rates.

It might be objected that Treasury bill rates did not undergo sharp cyclical swings until the period following the Treasury-Federal Reserve Accord, and that a true test of interest responsiveness would be meaningful only in that period. But in the two periods of sharply rising holdings following the Accord (III '53-II '55 and III '57-I '59), rates both dropped and rose. Similarly, in the periods of declining holdings little consistent relationship with Treasury bill rates was indicated: during the second largest postwar drop in holdings (II '55-IV '56) rates were generally rising, but in the cycle during which holdings declined the least (III '59-III '60) bill rates underwent their widest gyrations. Indeed, while bill rates surged to their postwar peak, rising from 2¾ per cent at the end of the first quarter of 1959 to reach 4½ per cent at the close of 1959, holdings of free governments remained virtually unchanged. It might be argued that the very sharp rate-rise of 1959 arrested what would otherwise have been a sharp drop in holdings. This point loses most of its force, however, if we examine the experience of the first quarter of 1960: in that quarter rates dropped from 4½ to 3 per cent, while holdings fell by less than $0.5 billion.

To be sure, as more firms have become rate-conscious, corporations as a group have responded to the secular upward drift in postwar interest rates. This secular interest-rate relationship provides a reasonable explanation for the secular decline in cash holdings (see Section V below) but cyclical changes in rates do not explain the short cycles in free governments. Rather, these cycles are explained by the exigencies of corporate financing requirements for real investment. While corporations hold a substantial volume and various types of financial assets carrying primarily short maturities, their financial problem is only in part, and only at times, a problem of short-term portfolio administration pure and simple. For while nonfinancial corporations have added to their holdings of financial assets, their major incentives and goals, even in the administration of purely financial assets, still lie on the "real" side of their activities. This does not mean that the portfolio problem goes unrecognized, but that conflicts between the "real" side and the "financial" tend to be resolved in favor of the real.

III. *Free Governments and Corporate Financing 1948-61*

To arrive at an appropriate evaluation of the predominantly real incentives for accumulation and sell-off of free governments, the relevant pattern of corporate financing must be defined, and estimates of the amounts involved must be set forth. Our specific hypothesis, to be tested below, is that free governments serve as a financial reserve pool for unknown future investment outlays. We will attempt to show, therefore, that when major corporate uses of funds (i.e., total spending on net inventories and on plant and equipment) exceed major sources of funds (i.e., both internal and outside funds), free governments will be sold. Conversely, when major sources exceed major uses, free governments will be accumulated. The major sources of funds include bank loans (both short-term and long-term), capital-market flotations, and internal sources (retained earnings plus additions to depreciation reserves).[9]

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[9] As indicated above, the major data source for this study is FTC/SEC [23, *passim.*]. That source had to be supplemented by additional data to give a consistent set of numbers to analyze because the raw FTC/SEC data on corporate "outside" financing are not too helpful in finding the gross volume of funds raised in each quarter. The author computed an estimate of proceeds of public and privately placed security sales based on

Chart 3 has combined the major sources of funds into an area chart, and the total uses of funds have been delineated as a heavy line superimposed upon the area chart. The chart reveals the two major developments of corporate finance in the postwar period: (1) internal financing has become by far the major source of funds for the large manufacturing firms; and (2) total uses of funds for real investment (inventory change plus plant and equipment outlays) underwent substantial cyclical surges through 1957 but have remained virtually stationary since that year. To place the growing importance of internal financing into perspective, it should be recalled that the corporate subsector under review includes the largest companies with the best credit ratings and the widest opportunities to finance through bank and nonbank sources. To be sure, the slowdown of spending for real investment over the last four years shown has reduced the need for outside financing. And the liberalized tax treatment of depreciation as well as the continued growth of the stock of depreciable assets has provided these firms with a rising cash flow in spite of the leveling-off of retained earnings.[10]

As a first approximation, the heavy reliance on internal financing by these firms may be viewed as a function of the large scale of their outside financing operations, each of which may present a difficult capital-marketing problem. But beyond this, the needs for outside funds of the large firms are concentrated during investment booms. At such times, large firms approach bank and nonbank lenders en masse with an over-all demand for funds that is likely to strain the capacity of the capital and money markets. According to Securities and Exchange Commission [24], the large firms typically tend to float intermediate or long-term security issues of at least $20 million each, and a sudden surge of such issues onto the capital markets carries with it the threat of market congestion or even of "disorderly conditions." Because such a threat could easily lead to the failure of large-scale financings of even the best credit risks, there have been periods—notably in 1956—when a number of reductions in size or postponements of financings took

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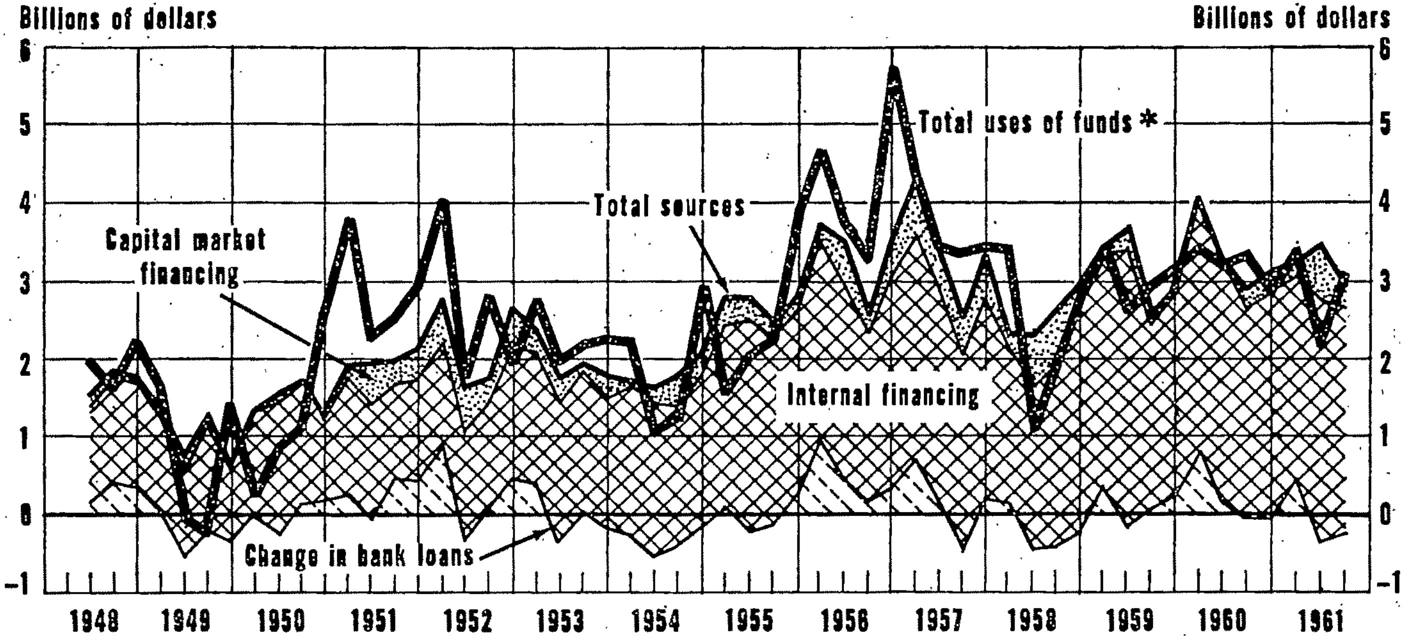
SEC worksheets; adjustments for refundings of earlier issues were made whenever possible. Balance-sheet data on plant and equipment spending taken from the same FTC/SEC sources had to be adjusted as follows: Corporations report each quarter on the value of their physical properties *after depreciation*. Mere first differences between one quarter of the year and the subsequent quarter thus reflect additions to plant value from construction *and substractions* owing to depreciation. To arrive at gross spending on real investment, depreciation was added back.

[10] For the period 1948-61 there were two major tax changes leading to accelerated depreciation: starting with 1951, there was the "Korean" five-year accelerated amortization program, and when that program was phased out in the late 'fifties, the methods available under the 1954 revisions of the tax code became important.

CHART 3. INTERNAL AND EXTERNAL SOURCES OF FUNDS AND USES OF FUNDS OF LARGE MANUFACTURING CORPORATIONS, 1948–61



* Composed of outlays on inventory change and plant and equipment.
Sources: See Chart 2.

place, or when some large financings in the planning stage were delayed indefinitely [21, p. 346]. In sum, there are diseconomies of scale for outside financings of large firms in periods of tight financial markets—but these are the periods when the need for such financings by large firms is greatest.[11] Attempts to achieve a greater degree of self-financing by these firms may be considered a response to this condition. In addition, short cycles of accumulation and decumulation of free governments may now be interpreted as an important adjunct to the self-financing process of the large corporations.

The various sources of funds shown in Chart 3 were generally insufficient to finance major uses (the line chart) during the investment booms of the 1950's. In those periods (notably III '50-II '54; III '55-I '58) the large corporations experienced deficits on a current-account basis: available sources of funds in each quarter were less than the realized uses. Conversely, there were quarters with surpluses, when sources exceeded uses (such as, for example, several recession periods when real-investment outlays tended to drop).

The major source of funds making up these deficits and the major use of funds employing the proceeds from the surpluses were changes in holdings of free governments. To demonstrate this we shall compare each quarter's surplus or deficit with the change in free governments in the same quarter. The upper panel of Chart 4A indicates that corporate surpluses and deficits were closely associated with changes in free governments in a manner consistent with our hypothesis, at least through 1957. Thus when business activity and real-investment spending declined and corporate surpluses began to appear, free governments were accumulated by nearly equivalent amounts. Conversely, when corporate activity accelerated and an investment boom began, holdings of free governments tended to decline. Finally, in the period since 1957, when deficits and surpluses were small, changes in holdings of free governments were small also, and the relationship of changes in free governments to variations in corporate surpluses and deficits became substantially weaker.[12]

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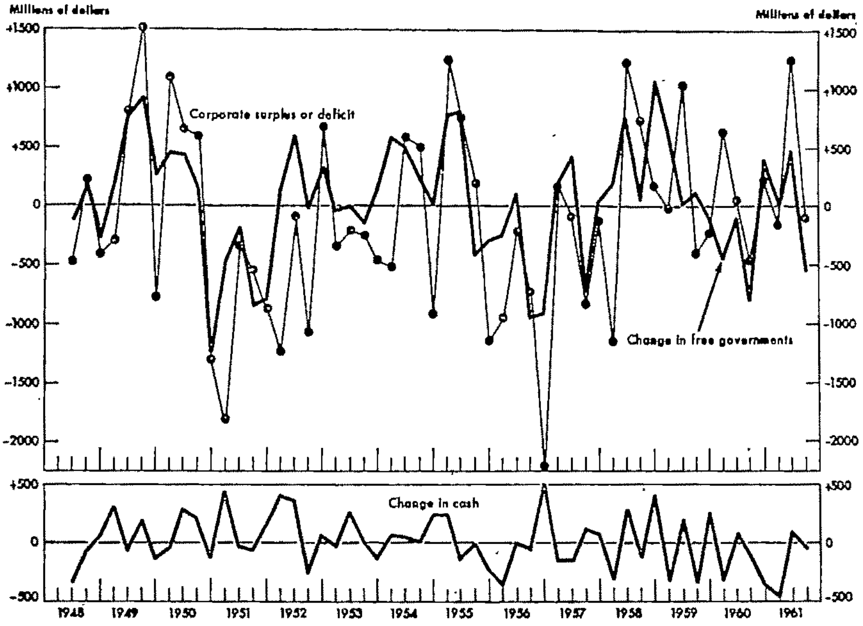
[11] For a discussion of the problems involved in marketing a large corporate issue, see [1, pp. 172-76].

[12] A small digression is in order at this point to discuss the interest-rate relationship—if any—according to which portfolios of free governments are managed. It is clear that variations in, or levels of, Treasury bill rates do not explain changes in such holdings. But since holdings are managed in a countercycle to the financing of "real" investment, "the" interest rate used should be one that expresses the relative advantage of switching corporate funds between "real" and "financial" assets. Such a rate, in an environment of heavy corporate self-financing of real investment, is a difficult one even to define. Modigliani and Miller [13, pp. 261-97] made attempts to do so, in part by relating the "cost of capital" to some measure of the imputed cost of internal funds for the individual firm. Even if it is assumed that the controversy generated by their discussion could be

CHART 4A. FINANCING "DEFICITS" AND "SURPLUSES," AND CHANGES IN HOLDINGS OF FREE GOVERNMENTS AND CASH OF LARGE MANUFACTURING CORPORATIONS, 1948-61



Sources: See Chart 2.

From the lower panel of Chart 4A and from Chart 4B it can be seen that changes in holding of cash and net trade credit, the other major holdings of corporate liquid assets, have a far smaller degree of association with corporate surpluses and deficits than free governments do.[13] In effect the small variations in holdings of cash suggest that for

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resolved, we would still have to solve a formidable empirical problem in working out a composite "rate" for all corporations. Finally, only an appropriately weighted "rate" could, at any one time, suggest whether or not the corporations ought to finance the acquisition of real assets out of the proceeds of financial holdings.

[13] Correlating these quarterly data of *changes* in free governments, cash, and net trade credit with corporate surpluses and deficits, the following results were obtained (on the basis of simple correlation runs in each case):

		Period 1948-57	Period 1948-61
r^2	Free governments	.54	.41
r^2	Cash	.005	.0002
r^2	Net trade credit	.03	.03

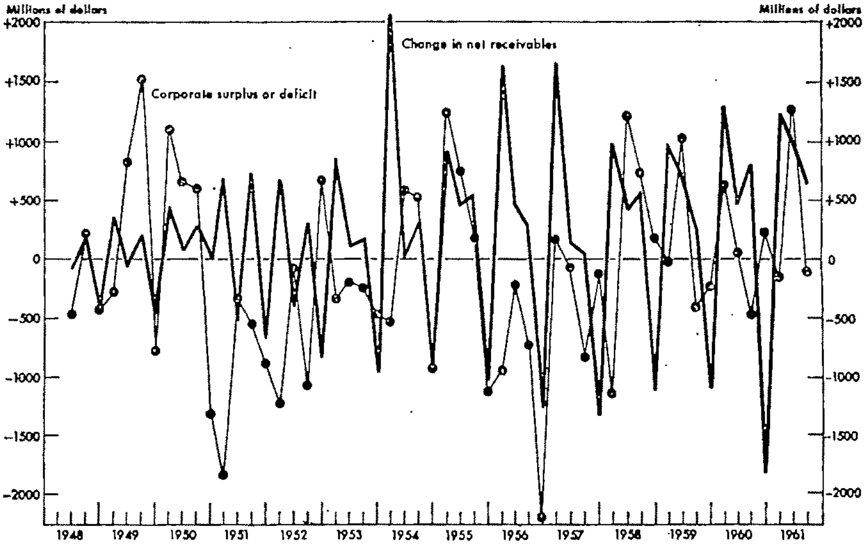
(Results given on basis of r^2 adjusted for sample size)

From these and other correlations undertaken by the author, it is clear that statistical significance attaches only to the relationship with free governments. A multiple-correlation analysis of the relationships of corporate net surpluses and deficits, and of changes in the large firms the economization of cash had proceeded to the point where holdings were too low to finance substantial rises in outlays in the boom. On the other hand, Chart 4B indicates that swings in extension of net trade credit have tended to widen. The chart also indicates, however, that these swings reflect seasonal rather than cyclical variations in trade credit.

IV. *Cycles in Free Governments and Use of Bank Loans*

If free governments have been used by large firms to bridge short-

CHART 4B. FINANCING "DEFICITS" AND "SURPLUSES," AND CHANGES IN NET TRADE CREDIT OF LARGE MANUFACTURING CORPORATIONS, 1948–61



Sources: See Chart 2.

Treasury bill rates, with changes in holdings of free governments for the period 1948–57 suggests similarly that the major factor affecting free governments is corporate surpluses and deficits.

$$Y_1 = 163.6834 - \underset{(.2177)}{.5966X_2} + \underset{(.0660)}{.4485X_3}$$

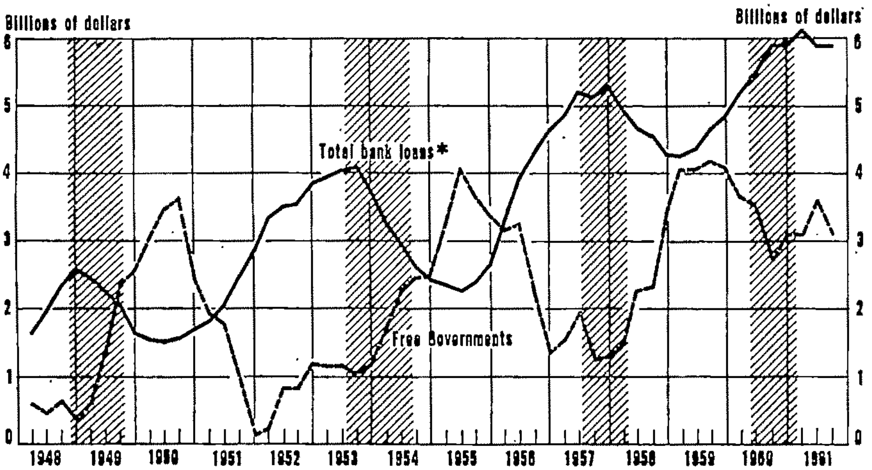
Simple-correlation coefficients	Partial-correlation coefficients
$r_{1.2} = .3522$	$r_{12.3} = -.4154$
$r_{1.3} = .7321$	$r_{13.2} = .7494$
$r_{2.3} = .0964$	

Where Y_1 = change in free governments
X_2 = change in 90-day-bill rate
X_3 = corporate net surplus or deficit
Figures in parentheses are standard errors of coefficients.

term financing gaps that may arise when spending on real assets exceeds available sources of funds, they perform a function similar to bank loans. Banks are presumed to be sources of funds from which financing may be obtained on short notice, particularly by prime borrowers, while their capital-market financing may require lengthy preparations. In this section we ask two questions: (1) Is the timing of the sell-off of free governments consistent with the timing of upsurges of bank borrowing for the (large) firms that have the best credit ratings? (2) Are the funds raised through sales of free governments significantly large in relation to bank borrowings? The answer to both questions is yes.

Chart 5 indicates the striking similarity of timing of corporate financing through bank loans (seasonally adjusted) and through free governments.[14] In particular, there is close correspondence between the peaks of free governments and troughs in bank loans. When corporations needed funds they began simultaneously to sell free governments and borrow more from banks; conversely, bank loans began to be repaid and free governments acquired during the same quarters.

CHART 5. QUARTERLY HOLDINGS OF FREE GOVERNMENTS AND BANK LOANS OUTSTANDING OF LARGE MANUFACTURING CORPORATIONS, 1948-61



* Bank loans have been seasonally adjusted by a ratio to moving average, changing seasonal method.

Sources: See Chart 2.

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[14] It is worth underlining here that the bank-loan series used in this chart includes both short-term and long-term loans, and that this is the only series in the article that was adjusted for seasonal variation. The seasonal adjustment was applied here to make it easier to identify turning points in bank-borrowing cycles.

The answer to the second question is readily shown in the chart. Not only do bank loans rise and fall by amounts roughly similar to (opposite) changes in free governments when these cycles are pronounced, but the weakening of bank-loan demand observable in the last cycle is also reflected in smaller sale of free governments in the period following mid-1959. Both free governments and bank loans held about stable in 1961. These developments of the recent past are to a significant degree explained by the absence of substantial corporate deficits since mid-1958 (see Chart 3). It is clear that free governments have been used as a source of short-term financing in a manner similar to bank loans in timing and in magnitude.

V. *Free Governments and Cash*[15]

If corporations manage their portfolios of short-term governments to provide a ready reservoir of funds, what are the determinants of their management of cash? Can that corporate experience throw any light on the interest-sensitivity of holdings of cash and of its velocity? Most of the literature concerns itself with cash holdings of the economy as a whole, although a growing segment of it concerns the corporate sector.[16] A general criticism made by a number of writers of the interest rate-cash relationship for corporations is that there often are either positive correlations among the independent variables used (such as income, profits, and interest rates) or similar trends in cash and the independent variables used.[17] In view of the fact that most of the correlations pointing to a relationship between cash and interest rates use annual data, the influence—if any—of short cycles in interest rates on cash holdings is damped, and the strength of the statistical relationship will necessarily be based to a greater extent on the positive correlation of trends in the independent variables noted by the critics.

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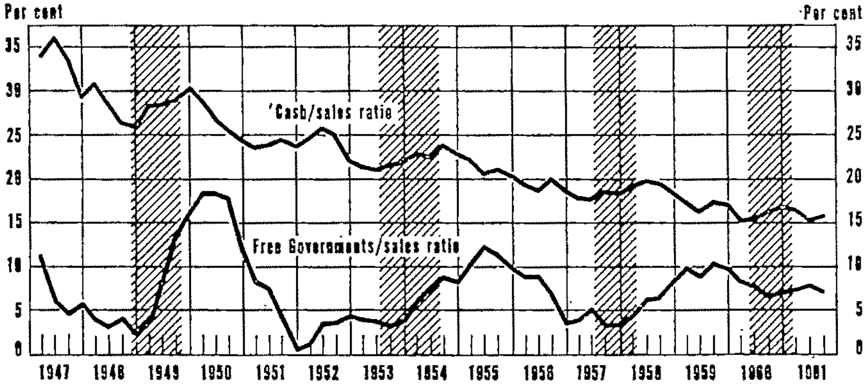
[15] The term "cash" includes bank deposits of all types, as well as currency.

[16] An extended list of empirical studies of velocity of money on an economy-wide basis is contained in R. T. Selden [17] in Milton Friedman, ed., [4, pp. 179-95]. References relating to corporate velocity may be found in Selden [18] and in McGouldrick [12, p. 1567].

[17] On these points see, for example, A. J. Duncan [2, p. 251] and C. Warburton [27]. Schumpeter made a similar point [16, p. 720] as follows: "While it should be admitted that the loss involved in holding idle cash varies with the rate of interest and that this fact does make some difference, the empirical correlation between large[r] cash holdings and low[er] rates of interest must not be interpreted as causally explained by the fact: large cash items and low rate are primarily consequences of decisions to restrict operations in depressive situations, and there would be correlation between them even if there were no functional relation at all."

CHART 6. QUARTERLY RATIOS OF CASH AND FREE GOVERNMENTS TO SALES
LARGE MANUFACTURING CORPORATIONS, 1947-61



Sources: See Chart 2.

Chart 6 shows ratios of cash to sales of final output of the large manufacturing corporations and of free governments to such sales. The aim here is to assess the extent to which cash holdings may be related to short-term movements in interest rates, and whether cash-economization is a cyclical (i.e., short-run) phenomenon. Cash holdings are presented as a ratio to sales of output partly to make our procedure comparable with prior studies of the subject, and partly to assess the changing importance of cash as transactions balances. The chart indicates clearly that the cash-sales ratio has been on a secular decline from 30-35 per cent level in the late 'forties to about 15 per cent in the early' sixties. A facile interpretation of this cash-sales trend would associate it with the general rise in interest rates over this period. Moreover, during this secular decline, there were periods when cash-sales ratios rose, notably during recessions. It might be argued that the typical declines in interest rates in recessions induced cyclical increases in cash ratios.

The view relating cash holdings solely to changes in rates is hard to support, however, in the light of evidence indicating that the amplitude of cyclical increases in cash ratios has been decreasing in the face of a generally increasing amplitude in downswings of short-term interest rates. Similarly, the amplitude of cyclical declines in cash-sales ratios during periods of recovery and boom has tended to become narrower in spite of the fact that interest-rate swings during the period covered tended to become wider from one postwar boom to the next. An examination of the changes in sales and the changes in cash holdings that constitute the ratio used here suggests that short-term movements in sales rather than in cash were responsible for changes in the ratio as a whole. McGouldrick's findings in this general area [12, pp. 1560-61] were much the same: the cash holdings of all nonfinancial corporations remained virtually level for four years (1955-58) and underwent only minor variations in the following three years, when they moved in a range roughly 5 per cent about mean holdings.

If an interest-rate relation applied to cash holdings, it would appear that its strength has been progressively weakened in the postwar period. The evidence suggests instead that the downward drift in cash-sales ratios of the large corporations is primarily secular in nature, related partly to the growth in the number of members of the cash-economizers' club, and partly to the members' increased efficiency. Because absolute cash holdings have remained rather stable in the face of a rising trend of sales or other indicators of corporate economic activity, short cycles in corporate surpluses and deficits until the late 1950's were reflected primarily through cycles in holdings of free governments.

VI. *Diversification in Corporate Portfolios of Near-Moneys, and Conclusions*

The weakness of the 1959-61 economic expansion was reflected in the leveling-off of real investment outlays made by the large corporations (see Chart 3), and in the relative stability of their holdings of free governments. Moreover, corporate sources of funds proved to be more than ample for current needs, thereby boosting corporate liquidity on an over-all basis. The large corporations obviously felt that not all of this new liquidity had to be placed exclusively in governments but that somewhat higher-yielding non-government assets could be acquired. While this change in policy-emphasis involved a marginal increment in risk, this was acceptable in a climate of tepid investment expectations of the period 1959-61. Besides, many corporate treasurers had recognized through the 'fifties that holdings of commercial paper of the largest sales finance firms in fact carry very little risk.[18] As a result, many treasurers readily raised such holdings, and, in addition, acquired substantial amounts of negotiable certificates of deposit (C.D.'s) of the largest commercial banks when these became available, and certain foreign money-market instruments when convertibility made that feasible after 1958.

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[18] D. P. Jacobs [6, p. 342] suggests that there was some tendency toward the substitution of nongovernment issues for government securities in corporate portfolios in the late 'fifties. Nevertheless, Jacobs' last survey, dated December 31, 1957, indicated that portfolios of near-money assets held by about 200 large corporations in his sample still contained at least three-quarters (and possibly 85 per cent) government (or agency) issues, with the remainder in nongovernment issues. It also is worth noting that the date of Jacobs' 1957 survey, which indicated some growth in relative importance of holdings of commercial paper, coincided with a low point in holdings of free governments (see Chart 2).

The volume of finance paper that was held by nonfinancial corporations rose from little more than $1 billion at the end of 1958 to about $1.9 billion in late 1961 according to Selden [19]. Not only was this increase in corporate holdings the sharpest of the postwar period, but so strong was the corporate appetite for commercial paper that the yield advantage it enjoyed over Treasury bills shrank from 50 basis points in 1958 to 25 basis points in 1961.

A second type of privately issued near-money became important in early 1961 when the large New York commercial banks first made available their time-deposit facilities to large firms through the medium of negotiable certificates of deposit (C.D.'s). According to a Federal Reserve study [9, p. 460], the country's largest banks experienced increases in outstandings of negotiable C.D.'s from about $100 million at the end of 1960 to nearly $1.6 billion at the end of 1961. Because the FTC/SEC source used in this study reports C.D.'s as "cash," the leveling-out of cash-sales ratios indicated by Chart 6 for 1961 probably reflects the acquisition of a substantial volume of interest-bearing C.D.'s under the name of "cash."

And finally with the advent of convertibility in 1958, many of the large U.S. manufacturing corporations found that the horizon of availability of near-moneys could be broadened still further to include foreign assets, notably deposits in the Euro-dollar market. While there are no good data available to show the participation in that market by U.S. corporations directly or through foreign subsidiaries, most sources agree that it is substantial.

To sum up, the growing liquidity of the large U.S. corporations in 1958-61 led them to acquire an increasing total and a more diversified portfolio of near-money assets. Although this diversification was necessarily accompanied by some increase in risk, those risks were acceptable in the face of the recession of corporate expectations that investment outlays would surge in the early 'sixties. This experience suggests that corporations may become sensitized to a broader array of short-term rates when some slack appears in their real investment needs, relative to available sources. Conversely, when a surge in real investment needs must be financed, all available liquid assets are preempted. In the period 1958-61 internal cash flows held at a historically high and stable level while real investment outlays stabilized at a somewhat lesser rate. This combination of trends tended to raise corporate liquidity without requiring any cyclical reductions in holdings of liquid assets. As a result, the freedom of corporate treasurers to maximize portfolio income by diversification was dramatically expanded, not only because the funds were available, but also because the likelihood to sell on short notice a large portion of the portfolio kept on receding.

For the large manufacturing corporations, the maximization of yields of short-term portfolios tends to become more important as a goal as the problem of financing real investments recedes in importance. By and large, the corporate management of cash and near-money constitutes a service function akin to proper inventory management. Indeed, for corporations, an appropriate "supply of finance" is the best universal inventory there is, to be accumulated or run off as real investment policy requires. It is the interaction of the real and financial surfaces of decision-making that gives corporate holdings of liquid assets their unique character.

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COMMUNICATIONS

Sales Taxation and the Consumer

A major unresolved controversy in the field of public finance is that of whether or not consumers "bear" sales taxes. The traditional argument that they do (apart from the effect of declines in prices of factors specialized to the industry, or from absorption in excess profits) has been questioned by some of the top scholars in the field, including Rolph, Break, Stockfisch, Buchanan, and to some extent Musgrave. Yet the traditional doctrine remains generally accepted, within the profession and outside. It is the purpose of this note to explore the sources of differences in the conclusions. The task is complicated, unfortunately, by the obscurity of much of the writing in the field, especially the failure to make assumptions explicit.

More specifically, this note is concerned with three aspects of the controversy: (1) the concept of the burden of the sales tax; (2) the relationship between over-all fiscal and monetary policies and tax incidence; and (3) the distinction between a sales tax on total expenditure and one levied only on consumption goods. The conclusion reached is that, except under very restrictive assumptions, the traditional view of sales tax incidence—that it is distributed in relation to consumer spending—is correct.

I. *The Concept of the Burden of a Sales Tax*

The first issue to be resolved is that of the nature of the burden of a tax and the meaning of the term "tax burden on consumers." The real burden of governmental activity, in the sense of reduced private-sector output due to the transfer of resources to the government, is created by government expenditures, not by the taxes, except for their frictional effects. The method of financing, however, determines the pattern in which the real burden is distributed among various groups in the economy, that is, whether the real burden on each person is related to his total income, the source of his income, the allocation of his income between consumption and saving, or other factors. This pattern of burden distribution or, in other words, the pattern of real income redistribution resulting from the tax, may be designated as the incidence of the tax. The real benefits and the real income redistribution arising from the government expenditures are not relevant to the incidence of the tax, but of course must be considered in evaluating the desirability of the government programs, a question beyond the scope of this note.

The statement that the "burden of a tax rests on consumers" is a shorthand method of saying that the pattern of reduction in real income is related to consumption spending. Persons with equal consumption of taxed goods will bear the same tax regardless of their income or wealth, and tax burden rises or falls with changes in consumption. A person can, therefore, vary his tax burden by varying his consumption, whereas changes in his income will not affect his tax burden so long as his consumption remains unchanged. This is the characteristic feature of a spendings tax; other levies, such as a sales tax, can be regarded as consumption taxes only if they meet this requirement.

II. *The Use of the Tax Revenues and the Relation of Tax Incidence and Fiscal-Monetary Policy*

Analysis of the distribution of burden which results from a sales tax requires some basis for comparison; the most suitable one is with a proportional income tax without exemptions. Or, in other words, it may be assumed that the sales tax is imposed to replace (or in lieu of) such an income tax. Alternatively, it could be assumed that the sales tax is imposed to finance additional government expenditures (Musgrave's balanced-budget incidence), but this assumption will result in uniting together the redistributional effects of the expenditures with those of the taxes, whereas it is preferable to keep the tax distinct, as the former approach does.

A major question, however, is that of the relative yields of the two taxes under comparison. If taxes of equal monetary yield are considered, there is likely to be a difference in their effects on aggregate demand (consumption and investment spending); the sales tax may reduce spending to a greater extent than the income tax, per dollar of revenue. The consequent deflationary effect, however, can more satisfactorily be attributed to the over-all fiscal policy employed, not to a sales tax, as such. Thus the appropriate comparison, for purposes of considering the pattern of burden distribution, is between a sales tax and a personal income tax with respective money yields such as to leave aggregate real (private and governmental) factor demand unchanged since it is the economic function of a tax to reduce private-sector real factor demand sufficiently to free a given quantity of resources for government use. If in fact the introduction of a sales tax to replace an income tax reduces real factor demand to such an extent as to produce unemployment, those losing their incomes should not be regarded as bearing a portion of the tax, but rather as being adversely affected by the over-all deflationary fiscal policy.

Most analyses of this question, including previous ones by the author, have failed to distinguish clearly between the pattern of burden distribution of various *types* of taxes and the consequences of deflation or inflation to which the tax may give rise if the *level* of the tax does not accomplish the desired economic result of offsetting the increase in factor demand caused by repeal of another tax or increased government spending. These are two distinct questions. Richard Musgrave recognizes the problem but does not deal clearly with it.

The questioning of the appropriateness of this type of assumption about the use of the revenue and real factor demand was the basis of Earl Rolph's original attack upon the "consumers bear sales taxes" doctrine [3]. Rolph's analysis sought to divorce the question of the burden of the tax from the use of the revenues, and thus demonstrates that the decline in factor demand consequent to the tax will reduce factor prices; under further simplifying assumptions of perfect competition in commodity and factor markets and perfectly inelastic supplies of factors, he concluded that the sales tax is distributed in relation to factor incomes, and thus in the same fashion as a proportional income tax on factor incomes. The decline in factor demand which is the basis of Rolph's analysis occurs because any offsetting demand resulting from government spending or repeal of an alternative tax is ignored.

This reasoning involves, as J. M. Buchanan has stressed [1], an illicit use of the *ceteris paribus* assumption. When a sales tax is imposed, there must be some change on the use-of-funds side: the revenue must replace that of another tax, or be used to finance an increase in government spending, to retire debt, to add to liquid balances, etc. Thus it cannot be assumed that there is no change in any of these elements; the most reasonable assumptions are those noted in the previous section. If the assumption were made that the revenues were "sterilized," continuous deflation would result; the consequent decline in factor incomes, however, should be attributed to the over-all fiscal policy, not to the tax.

A. *Can Increases in the General Price Level be Attributed to Taxes?*

Under the traditional approach to sales tax incidence, prices of commodities rise to cover the amount of the tax (with exceptions) through reduction in supply or, in the typical nonpurely-competitive markets, through direct price increases to cover the tax element in cost. Various exceptions have of course long been recognized. The second line of attack upon the "consumers bear sales taxes" doctrine, first stressed by Buchanan, and then used by Rolph and George Break in their latest work, is aimed at the reasoning that taxes can cause increases in the general price level. In Buchanan's terms "And surely we should not attribute the inflationary policies of the monetary authorities [that is, the increase in the general price consequent to the sales tax] to the tax" [1, p. 139]. Rolph and Break express the same point of view:

> Although the imposition of a selective excise or general sales tax may well be followed by an increase in the general price level, the tax itself is neither a necessary nor a sufficient condition for the price rise. The willingness and ability of the economic system to initiate and support a monetary expansion, on the other hand, is a necessary condition and hence should be regarded as the primary causal factor [4, p. 294].

Thus, according to Buchanan, and Rolph and Break, if the general price level increases as a result of the levying of a sales tax, this is not to be attributed to the tax, and has no significance for the burden of the tax, which still rests on factor owners, whose position has been worsened relative to commodity prices. Implicit in their reasoning is the position that the burden of sales taxes cannot rest on consumers unless prices rise. Since, by their reasoning, prices cannot rise as a result of a sales tax, any possibility of "consumers bearing a sales tax" is effectively eliminated.[1]

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[1] Rolph applies the same reasoning to excise taxes. The prices of the taxed items will rise, but because of the decline in factor prices the prices of other goods will fall. Thus consumers as such do not bear the burden of the tax (although admittedly the position

Obviously, the general price level cannot increase in response to a tax or anything else unless monetary adjustments take place. The real issues, however, are those of causal relationships and appropriate assumptions about monetary policy. Buchanan and Rolph argue that only monetary changes can cause an increase in the general price level. But this argument can be rejected, and price-level increases attributed also to occurrences which increase cost, such as sales taxes or union-induced wage increases. There are a number of determinants of the general price level, and changes in the level can appropriately be attributed to shifts in any of these determinants. The general intent of sales tax legislation typically is that the taxes be shifted forward to consumers through higher prices; thus the appropriate assumption to make about monetary policy is that adjustments will be made in the money supply to permit the realization of this intent. Thus the tax, not the monetary adjustments, may be regarded as the "cause" of the general price level increase. If monetary policy does not permit upward adjustments in prices, unemployment will almost certainly result, because of resistances to downward adjustments in factor prices, particularly wages, and the goal of full employment will no longer be attained, or departure from it will be greater.

The assumptions of goals of full employment on the one hand and of monetary policy which will allow price increases to shift the tax forward on the other are not inconsistent with the usually accepted goals of full employment and price stability. Price increases necessary to shift a tax forward are in a sense merely devices for tax collection and may be considered as not constituting deviations from general price stability.

B. *Musgrave's Emphasis on Relative Price Changes*

Musgrave has sought to avoid the need for any assumption about monetary policy, so far as incidence is concerned, in questioning the second of the Buchanan-Rolph implicit assumptions noted above, that consumers bear sales taxes only if commodity prices rise [2, Chs. 10, 15, 16]. Musgrave's basic position is that the incidence of the tax, compared to that of a proportional income tax, depends upon the relative change in factor and commodity prices, rather than the direction of the absolute change in prices. Thus the incidence is the same (under certain highly simplifying assumptions) whether consumption-goods prices rise relative to factor incomes, or factor incomes fall relative to consumption-goods prices. To Musgrave, so long as the tax is confined to consumption goods and thus capital goods are free of tax, the burden of the tax rests on consumers, regardless of the direction of price changes. If factor prices remain unchanged and consumption-goods prices rise because of the tax, real income is reduced in relation to consumer spending, whereas savers are not injured since the prices of capital goods remain unchanged, and thus the return on saving will be unaffected. If instead, factor prices decline, the

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of those consumers spending relatively large amounts for taxed items is worsened), which rests upon the factor owners. Buchanan, however, maintains that the burden of excises does rest upon the consumers of the products, since the prices of these goods have risen relative to other prices.

prices of capital goods will decline, and thus they will become relatively lower than those of consumption goods, compared to the pretax situation. Thus savers benefit by being able to acquire capital goods more cheaply and obtain the same return on their savings as before, except to the extent to which their returns decline together with other factor prices. Consumers, on the other hand, are burdened by the changed relationship between their incomes and factor prices. Thus the relative burden on various persons is dependent upon the percentage of income spent on consumption, and persons can alter their tax liability by changes in their allocation of income between consumption and saving, regardless of whether factor prices have fallen or commodity prices have increased.[3] The direction of absolute change will depend upon monetary policy.

C. *The Significance of the Direction of Change*

The Musgrave argument that the direction of change in absolute prices is irrelevant for incidence is true only under highly simplifying and unrealistic assumptions, as Musgrave recognizes. In practice the direction of change is likely to be highly significant. While in either case the over-all burden may be considered to rest in relation to consumption, there will be significant redistribution of factor incomes if the general price level remains unchanged, because some factor prices are much more resistant to downward changes than others. The owners of factors whose prices are least resistant to change will bear a disproportionate amount of the tax burden.

III. *The Taxation of Capital Goods*

Most analyses of sales tax incidence have been based on the assumption that the tax applies only to consumption goods. In practice, sales taxes frequently apply to some capital goods, particularly capital equipment. Musgrave argues that if the tax applies to all final-product capital goods, as well as to consumption goods, it is equivalent to a proportional-rate income tax and thus is not borne in relation to consumption expenditures. To Musgrave a tax on capital goods is equivalent to a tax upon income saved and thus in conjunction with a tax on income spent on consumption constitutes a universal tax on all income. In this instance, capital goods do not become relatively cheaper than consumption goods, and, argues Musgrave, a person cannot reduce his tax by saving more and consuming less. This conclusion, he argues, is valid whether commodity prices rise or factor prices fall.

The argument that a tax on capital goods is equivalent to a tax on the income saved is open to serious question. Traditional analysis of the question maintains that the tax on capital goods will be reflected in the prices of the consumption goods produced with the capital goods, and thus likewise will be borne in relation to consumption spending; thus the return on funds saved is not reduced, and the portion of the tax on capital goods is not equivalent to a tax on the portion of income saved. When the government taxes capital goods as well as consumption goods, it essentially forces a reduction in private-sector output of capital goods as well as the output of consumption goods; the same total amount of savings buys a smaller quantity of capital goods, but the quantity available is smaller since the resources have been taken from this sector by the government. But to the extent to which the prices of the consumption goods reflect the tax element in the capital goods used to produce them and a return on the larger sum of money capital involved, the return to savers, per dollar of savings, is not reduced, and both portions of the tax may be considered to be borne in relation to consumer spending. The Musgrave analysis is based on an unsatisfactory capital theory which directly relates a tax on capital goods to the return on savings. It is actually largely immaterial for incidence whether a sales tax is collected entirely at the point of final sale of the consumption goods or in part on goods whose costs enter into the prices of the consumption goods.

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[3] Rolph and Break have come rather close to accepting this position [4, pp. 291-92]. They grant that if capital goods are free of tax while consumption goods are taxed, investment is encouraged relative to consumption (under the assumption of full employment), and thus in a sense consumers, in total, are adversely affected, since the total volume of goods available for consumption falls. Consumers in the future will benefit from the greater current volume of investment. This is a different concept of "consumer burden" than the usual one.

The general question of the incidence of a tax on capital goods warrants further exploration; various issues, such as substitution of other factors for capital goods when the latter are taxed, have scarcely been touched in the literature and are beyond the scope of this note.

IV. *Conclusions*

This brief review of the question suggests several conclusions:

1. The traditional view that "consumers bear sales taxes," in the sense that the pattern of distribution of burden is related to consumer expenditures, is clearly valid for a sales tax limited to consumption goods, whether factor incomes fall or commodity prices rise.

2. Under the following assumptions, which appear to be reasonable ones, the adjustment will take place primarily through increases in commodity prices (with some exceptions noted by traditional theory):

(a) Most prices of finished products are set by the individual firms, on bases of such nature that an increase in average cost, such as that produced by a sales tax, will be reflected in higher prices.

(b) In light of the usual intent that sales taxes be shifted forward, and the emphasis given to the goal of avoiding unemployment, monetary policy will permit an upward movement in the general price level necessary to permit forward shifting.

3. Any change which occurs in aggregate real factor demand as a result of the levying of a sales tax should be attributed to the over-all fiscal policy, not to the tax. The most appropriate basis for analyzing distribution of burden with a particular tax is one between the tax in question and a proportional income tax of such height as to allow the same real factor demand. The redistributive effects of alternative tax structures should be distinguished clearly from the relative effects upon real factor demand and employment of two taxes of equal money yield.

4. If a sales tax applies to final-product capital goods as well as to consumption goods, as most sales taxes do to some extent, the conclusion that the tax is borne in relation to consumption is valid upon the assumption that portion of the tax on capital goods is reflected in higher prices of the consumption goods produced with these capital goods. The alternative assumption by Musgrave, that a tax on capital goods results in reduced return to savers, leads to the conclusion that a sales tax applying to both consumption and capital goods is more nearly equivalent to an income tax. The first assumption appears to be the more reasonable one.

5. The conclusion that sales taxes do not rest on consumers can be reached on the basis of any one of three sets of assumptions:

(a) That the use of the sales tax revenues is not to be considered. As a consequence real factor demand falls when a sales tax is levied, and factor prices fall (under the assumption of perfect competition in commodity and factor markets), thus distributing the burden in relation to factor income. This position may be regarded as involving a methodological error, including in *ceteris paribus* the assumption that no change occurs on the use-of-funds side, when some change must occur, or as only a partial analysis of the problem that ignores the economic function of a tax and is worse than useless for policy purposes.

(b) That general price-level increases cannot be caused by taxation, but only by monetary changes, and that consumers can bear sales taxes only if commodity prices rise. The former assumption is unacceptable; and the latter is invalid, as demonstrated by Musgrave.

(c) The sales tax applies to all final products—capital goods as well as consumption goods—and the portion of the tax on capital goods reduces the net return to savers, rather than being reflected in the prices of consumption goods. This assumption is an unrealistic one in light of usual treatment in price determination of tax elements in the costs of production.

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Diminishing Returns and Linear Homogeneity

In his recent textbook (*The Nature of Price Theory*, Homewood, Ill., 1963, p. 126, note 3), Professor H. H. Liebhafsky states:

> Many writers also draw the total product curve with a point of inflection and then "assume that the production function is linearly homogeneous." A thorough search of the literature has failed to reveal a single case in which a writer has *specified* a linearly homogeneous production

> function which produces such a total product curve! The procedure of drawing the curve in this way and then assuming it to be linearly homogeneous is confusing, and those who draw the curve in this way and make the assumption have the burden of stating the specific function they are employing.

Professors Stonier and Hague are more explicit on the substantive point (*A Textbook of Economic Theory*, New York, 1961, p. 229):

> If returns to scale (outlay) are *constant*, the marginal productivity of a variable factor used in conjunction with a fixed factor will always diminish as more of the variable factor is used.

However lax our colleagues may have been in not writing out functions that combine short-run increasing returns with constant returns to scale, such functions do exist and make economic sense. A representative of this family of functions is:

$$x = 22a^{1/4}b^{3/4} - 20a^{1/3}b^{2/3} \tag{1}$$

where x is output and a and b are inputs of two different productive services. Differentiating with respect to b, we have:

$$\frac{\partial x}{\partial b} = \frac{66a^{1/4}}{4b^{1/4}} - \frac{40a^{1/3}}{3b^{1/3}} \quad . \tag{2}$$

$$\frac{\partial^2 x}{\partial b^2} = -\frac{66a^{1/4}}{16b^{5/4}} + \frac{40a^{1/3}}{9b^{3/4}}. \tag{3}$$

If $a = 1$, equations (1) and (2) are positive for all $b \geq 0.998$; equation (3) is positive for $2.45 > b > 1$ and negative for $b \geq 2.45$. Of course, over the range at which b has increasing returns, a has a negative marginal product. That is,

$$\frac{\partial x}{\partial a} < 0 \quad \text{for} \quad 1 < b/a < 2.45.$$

The essential point is that the condition of linear homogeneity in a production function does not, in and of itself, rule out areas of short-run increasing returns (negative marginal products) to some inputs.

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Socialist Operational Price Systems: Comment

Polish economists have distinguished themselves in recent years by their relatively undogmatic approach to theoretical economics. They have been more willing to borrow tools from the arsenal of "bourgeois economics" than their colleagues in the other nations of the Soviet bloc. The article by A. Wakar and J. G. Zieliński[1] is no exception. Its approach to the pricing problem in a centralized economy is objective and dispassionate. My criticism of their paper turns not on ideological issues but on the precision of their analysis. I shall try to show in this note that the questions they raise could have been dealt with more exactly and more cogently if they had made use of sharper analytical methods.

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[1] This *Review*, March 1963, *53*, 109-27.

The essential problem analyzed by Wakar and Zieliński is to find an optimal combination of managerial incentives and producers' prices "which serves plan fulfillment and precludes price or incentive stimuli not in harmony with planned tasks" (p. 126). In their quest for a solution, they study alternative bonus schemes and speculate on the effects of various price policies on: (1) the fulfillment of the enterprise's "assortment plan" (prescribed product-mix) and (2) on the choice of its inputs and the minimization of its production costs.

Their analysis is focused principally on the level and structure of enterprises' production costs and on the relation of these costs to output prices. My principal objection to their article is that, at crucial points in their study, they fail to specify, to distinguish, or, for the most part, to analyze the effects of the physical and monetary limitations acting as restraints on bonus-maximizing managers. Inasmuch as these restraints condition managers' responses to bonus schemes and price systems, they cannot be left out of any model aspiring to be realistic.

From the context of their article, it is clear that the authors set their problem in the usual framework of Soviet-type economies.[2] The socialized enterprise has at its disposal a limited supply of fixed assets, receives current inputs, such as materials and components, in limited quantities (rationed out by an administrative organ), and must confine its expenditures on wages to a fixed wage fund; it is also usually expected to stay within a planned cost budget. However, the enterprise's input coefficients may be reduced by technical progress or by improved management, or the managers may bargain with higher authorities for larger allotments of certain inputs, so that the constraints within which the enterprise must operate are not entirely rigid.

With the aid of a simple diagram, we may analyze the impact of alternative bonus schemes and price systems in the simplified case of an enterprise producing two products. (The algebraic extension of these results for more than two products presents no special difficulty.) To start with, it is assumed that the constraints imposed by the limits on available inputs are fixed. Since the pricing problem is viewed from the vantage point of the administrative authority whose information about the production processes open to the enterprise is limited, the constraints may be represented by straight lines, the slopes of which are the ratios of approved input coefficients, or "norms."[3] If, for example, the coal norm per unit of output is a for

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[2] See particularly pp. 114-15. Administrative rationing of resources is linked to what Wakar and Zieliński call "direct economic calculation."

[3] Wakar and Zieliński specify that production processes are centrally determined (p. 115). Note that the assumption of straight-line constraints, although convenient for representation, does not affect the analysis in any essential way.

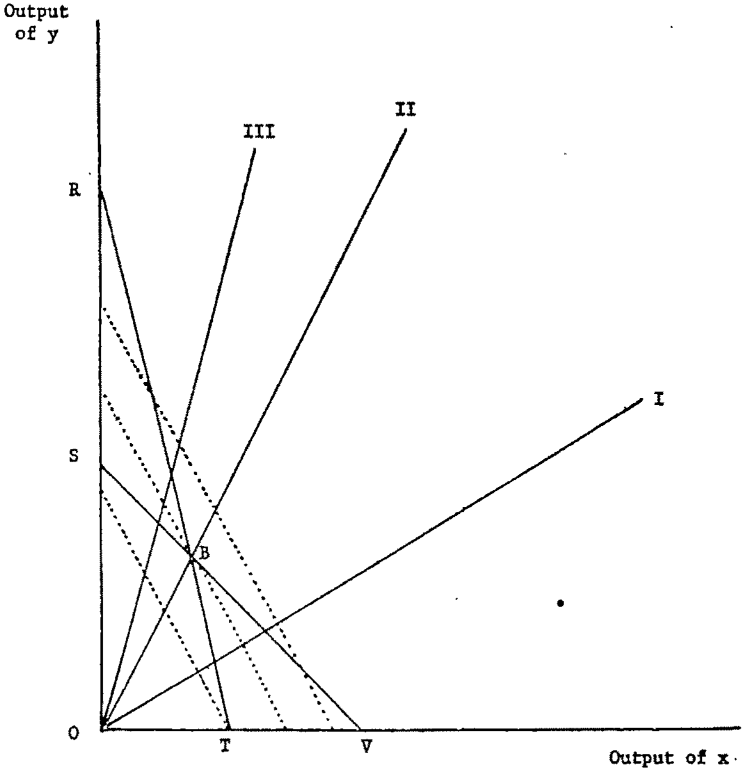

DIAGRAM 1

product x and b for product y, the slope of line will be $-a/b$. The intercepts of the line with the ordinate and abscissa will of course be determined by the amount of coal parceled out to the enterprise.

On the same diagram, we may represent the "bonus function" the managers seek to maximize by lines whose slope and curvature will differ according to the nature of the bonus scheme under consideration. If bonuses are geared to the gross value of output produced at fixed prices, then the lines will be straight and their slope will be equal to the ratio of the price-weights. If bonuses depend on total value-added rather than on gross output, the bonus lines will be affected in at least two respects: (1) their slope will come closer to paralleling the wage-fund or labor-costs restraint than in the preceding case (assuming that relative unit costs for wages and salaries diverge from relative total unit costs); (2) insofar as labor costs per unit increase when the product-mix is biased toward one or the other of the products, the bonus lines may be concave to the origin. In Diagram 1 the bonus lines, for the sake of simplification, have been drawn straight (as dotted lines). In this first case, we assume they correspond to a gross-value-of-output bonus scheme.

The rays from the origin identify three different assortment plans that may be prescribed by the administrative authority. There are two constraint lines: line *SV* is a material constraint, drawn in such a way that the material intensity for making both products is the same; line *TR,* which stands for the labor constraint, shows a higher labor intensity for product x than for product y. If these lines were reinterpreted as cost-budget lines (*SV* as material costs and *TR* as labor costs or value-added), then the diagram might correspond to the Wakar-Zieliński example (p. 111), which shows two products that have equal transferred values (mainly material costs), but one of which has twice as large a margin of value-added as the other.

It is immediately obvious that producing at point *B* is most advantageous to the bonus-maximizing manager, since at *B,* the enterprise produces the maximum value of output compatible with the inputs at its disposal. (Point *B* is the point of the feasible area *OSBT* touching the highest bonus line.) As long as output prices (determining the slope of the bonus line) remain anywhere between the slopes of *SV* and *RT,* there will be no incentive to violate product-mix *II.* Note that the administrative authority, insofar as it determines both the assortment plan and the inputs the enterprise will obtain to fulfill it, will tend to set a plan in the neighborhood of *B,* at least if it has correct information on the enterprise's production possibilities, for such a plan would make the fullest use of the inputs allotted. If it wished the enterprise to produce some other mix, such as mix *I* or *III,* the authority would presumably adjust the structure of its allotments to facilitate the fulfillment of this alternative plan.

The price-setting rule for outputs in this case is very simple: once the binding constraints have been found (*RT* for mix *I, RT* and *SV* for mix *II,* and *SV* for mix *III*), relative output prices should be set as follows: (1) if a single constraint is binding, then relative prices should equal the ratio of input coefficients at the maximum output corresponding to this mix; (2) if two or more constraints are involved (as was the case at point *B*), prices should equal some suitable average of the relative input coefficients at the maximal point.

How does this solution compare to Wakar and Zieliński's? They suggest that the ratio of output prices should be set somewhere between the ratio of the total average costs of the two products and their relative margins of value-added. The rationale for this pricing policy is that average cost-pricing unduly encourages the production of material-intensive items, while value-added pricing stimulates the production of labor-intensive items. Hence the optimal scheme must lie somewhere in between. (They admit they do not know exactly where.)

My first objection is that Wakar and Zieliński, in effect, identify the physical and value constraints to which the enterprise is subject, whereas this is appropriate only if relative input prices are correct indicators of the relative scarcity *to the enterprise* of the various inputs. Suppose, for example, that the lines *TR* and *SV* represented capacity constraints (e.g., forging and casting capacity in a metal-processing plant), and these were the only bottlenecks impeding production—the cost-budget line and the wage-fund line lying entirely beyond the feasible area *OSBT*. Then the Wakar-Zieliński criterion would clearly be irrelevant. The slope of the bonus line, whether determined by cost-price weights or by value-added weights, would have no simple connection with the slopes of any of the binding constraint lines: there would be no prima facie reason why a bonus scheme predicated on a mixture of the two types of weights would be better than either of the "pure" schemes. It is only by chance that the highest attainable bonus line constructed according to the Wakar-Zieliński principle would hit a vertex or a facet of the feasible area at a point of contact with the correct assortment ray.

On the other hand, if the binding constraints happened to coincide with the material costs budget and the wage fund, as we first assumed, then the authors' pricing instructions would at least be relevant, since the slope of the bonus line would be directly related to the slopes of the constraints. If the bonus weights were "cost-plus" prices, their ratio—i.e., the slope of the bonus lines—would have to fall somewhere between the slopes of the *RT* and *SV* lines. On the other hand, value-added weights would swing the slope of the bonus lines closer to the slope of the *RT* line, whose coefficients are based on labor costs per unit of output. The point is therefore valid that if the desired mix were in the proportion shown by ray *I*, a value-added bonus would be more likely to induce compliance than a value-of-output bonus. It would be clearly wrong, though, if mix *III* were in order.

Suppose now that one or more of the constraints can be relaxed. How will this affect the product-mix and the choice of inputs? Evidently, whether the labor supply, the availability of materials, or cost budgets impose binding restraints on output at the optimal point," the enterprise, under a bonus scheme based on the gross value of output, will benefit by ordering more of any of these inputs until their physical marginal productivity is reduced to zero, that is, until the expansion of output is blocked by a capacity constraint that cannot be loosened by the more liberal application of any of the variable inputs. Is it necessarily true, as Wakar and Zieliński argue, that the enterprise seeking to maximize its value-of-output bonus will concentrate on the making of material-intensive goods if output prices are equal to average unit costs plus a profit margin proportional to costs (p. 122)? As can be seen in Diagram 2, we can be sure this tendency will manifest itself only if the material-intensive product is not more capital-intensive than the other product.

Diagram 2 is identical with Diagram 1, except that product y is now more material-intensive than product x and a capacity constraint *LM* has been added that places an eventual limit on the expansion of output, even if labor and materials could be obtained in any desired quantities.

At first, until the capacity ceiling has been reached, the managers may wish to concentrate on y and bid for more materials rather than for more labor. If the bonus scheme hinges on the value of output and if relative prices are based on the cost-plus principle, then the bonus lines should fall between *RT* and *SV*, as in the preceding problem. Whether the labor or the material constraint "pinches" more at an optimal point such as *B* will depend

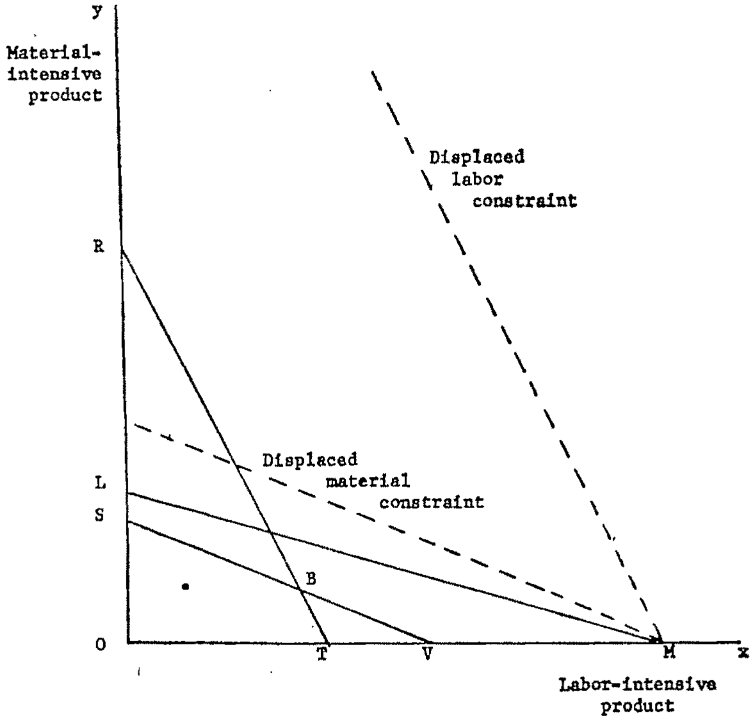
y
Material-intensive product
Displaced labor constraint
R
Displaced material constraint
L
S
B
0
T
V
M
x
Labor-intensive product

DIAGRAM 2

on the implicit prices of the two factors, which, in this case, are nothing but their marginal value products. These shadow prices can easily be computed.[4] At this juncture, it is correct to state that if the bonus lines are sloped in proportion to relative total average costs, the marginal value product of the material, and hence, the intensity of the enterprise's efforts to secure a larger material supply, will be greater than if the bonus lines reflected value-added margins.

The capacity constraint must eventually stem the efforts of the enterprise to obtain more labor and materials. Beyond point *M*, neither of the variable inputs will have a positive marginal productivity. Thus if enough inputs can be obtained to operate at *M*, this will be the optimal point of operation, where the total output bonus will be maximized.[5] This is shown by the displace-

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[4] These implicit prices can be found by solving the dual of the linear-programming problem for computing the optimal outputs. Denoting the relative prices of x and y by p_x and p_y, and the input coefficients per unit of output by a_{mx}, a_{my}, a_{lx}, a_{ly}, then the scarcity prices p_m and p_l of material and labor (to the enterprise) may be computed, for the product-mix corresponding to point *B*, from the equations:

$$a_{mx}\,p_m + a_{lx}\,p_l = p_x$$
$$a_{my}\,p_m + a_{ly}\,p_l = p_y.$$

[5] Note that if the slope of the bonus line lies somewhere between the slopes of *SV* and *RT*, then it must exceed the slope of the capacity line *LM*, which is smaller than the slope of *SV*. Hence, point *M* is the point of contact of the feasible surface *OLM* with the highest attainable bonus line.

ment of the *SV* and *RT* lines to the right (indicating larger supplies of the two inputs), until expansion is blocked by the capacity ceiling at *M*. At this point only the labor-intensive product will be produced—in outright contradiction with our authors' prediction.

The introduction of concave constraints, due to the effects of diminishing returns, would not alter this conclusion substantially. The tendency would still be for managers to skirt the capacity constraint by concentrating on the production of the labor-intensive item; however, in this case, the bias in favor of producing *x* would stop short of full specialization, at a point where the relative marginal costs of producing the two products, in terms of capacity inputs, equalled the slope of the bonus lines, i.e., the ratio of output prices.

Next, let us consider the claim made by the authors that under an output-bonus scheme the response of enterprises to changes in input prices will be diametrically opposed to the one normally expected in a profit-maximizing situation: a rise in the price of an input will cause it to be substituted for other inputs; a drop in price will check its consumption. Their reasoning runs as follows: Suppose output *x* is *a*-intensive and *y* is *b*-intensive (*a* and *b* being distinct inputs); let the price of *a* be raised relative to *b;* then the average cost of *x* must go up relative to *y*. This will shift the bonus line in favor of *x* and stimulate its output; the demand for *a* must rise *pari passu.* From this they infer that "input prices, as a rule, are to be equalized" (p. 112) so that they may be neutral with respect to the fulfillment of the enterprise's assortment plan and to the choice of inputs.

This ingenious conclusion, however, holds only in the absence of cost budgets. For if the enterprise is subject to such a constraint, the rise in the price of *a* will cause a contraction of the constraint line in the direction of the origin along the abscissa. The cost-budget constraint and the bonus lines will remain *approximately* parallel for any change in the relative prices of inputs (always on the assumption that the bonuses are geared to the gross value of output and that outputs are weighted by average-cost prices). If anything, the relative increase in price of a material will cause the cost-budget line to contract more than the bonus line, as material costs make up only a part of the total unit costs for each product that determine the slope of the bonus line, so that the net impact of the change will probably be to reduce the demand for the material whose price has increased. If Wakar and Zieliński chose to ignore these limits on enterprise expenditures in their speculations, then they left themselves open to the charge that their model does serious violence to reality: however important output bonuses may have been at any point in the development of Polish economic institutions—as in the early 1950's for instance—their incentive effects were always restrained to some extent by cost limits.

Finally, a word about the adjustment of the enterprise to the incentives created by a value-added bonus fund. Wakar and Zieliński argue that this scheme will cause enterprises to "minimize 'transferred' value (i.e., value of constant capital) and other items which are subtracted from gross output because this, *ex definitione,* will help to maximize their value added" (p. 123).

They advocate the strict rationing of the wage fund under these conditions to prevent the squandering of labor, which would help to boost value added. They then conclude that, once this restraint has been imposed, price policy for material inputs could be used as "an effective tool for stimulating producers to achieve the planned methods of production and product-mix. Enterprises [will] react to changes in price ratios in the same way as under profit incentives, i.e., they [will] try to equate the marginal value productivity of each factor with its price" (pp. 124-25). Here again, because the other restraints are not specified, neither conclusion need strictly follow. Consider, for example, the following production processes open to a hypothetical enterprise. There is a unique process for making product x and a choice of two processes for making y:

	x	y	
		Process 1	Process 2
Output (dollars)	1	1	1
Inputs (dollars per unit of output)			
Material A	.2	.3	.1
Material B	.1	.3	.2
Labor	.3	.1	.3
Profit (dollars per unit of output)	.4	.3	.4

We let x, y_1 and y_2 stand for the activity levels at which the processes are operated. Suppose that the labor-costs restraint reads:

$$.3x + .1y_1 + .3y_2 = 10.$$

This enterprise, whether it maximizes value-added ($.7x + .4y_1 + .7y_2$) or profits ($.4x + .3y_1 + .4y_2$), will produce only y and will choose the material-intensive process in so doing (Process 1) even though both the value-added margin and profits per unit are higher in Process 2. Thus the value of materials consumed will not be "minimized," as the authors claim. The simple reason for the enterprise's choice is that the labor-cost constraint is such that 100 units of y can be produced by the first process and only $33\frac{1}{3}$ by the second (the total value added in one case will be $100 \cdot 0.4$ or 40 and in the other $33\frac{1}{3} \cdot .7$ or $23\frac{1}{3}$). Any assortment plan calling for the production of x will be violated. By doubling the price of A the price-setting authority could induce a shift to the second process for making y, as may easily be verified. But once this shift has been made, output will still be determined by the labor constraint, and there is no reason to believe that the price of A would be equalized with its marginal value product.

The imposition of a costs limit restraining the combined consumption of A and B may also prod the solution toward the second process for making y or toward the production of x. Even then the enterprise will not necessarily "react to changes in the price ratios in the same way as under profit incentives." For the maximand is value-added, not profits or the gross value of output; the implicit prices that can be computed for each of the restraints will differ from the marginal value products corresponding to these other maximands. However, if the authors simply mean that the prices of materials can be manipulated—by trial and error—so as to induce any desired change in their relative consumption, I should be tempted to agree. But then they have only begun the theoretical analysis that would be needed to predict the nature and extent of these changes. The advice Wakar and Zieliński proffer is too rudimentary a guide for price-setting policy either under a value-of-output or a value-added bonus scheme.

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Socialist Operational Price Systems: Reply

In our article, "Socialist Operational Price Systems," we set ourselves the humble task of showing some of the interdependencies which exist between incentives and prices in a socialist system. We did not, however, intend to give *rules* for the efficient direction of production, as Montias seems to imply.

1. The main theme of our article may be summarized in the short proposition that to every system of incentives there should correspond a definite price structure for the means of production and for final goods. In some cases these price systems may seem to contradict common sense (wonderland prices). Such is the case, for example, with a price structure corresponding to an incentive system based on gross output.

Does this mean that we recommend such a system of prices? Not in the least. We were concerned simply to show that there exists a close connection between prices and incentives and not elaborate the details of the wonderland system of prices corresponding to gross-output bonuses. This task, on the contrary, consumes three quarters of Montias' comments. The reforms now under discussion in Poland tend to eliminate the gross-output bonuses rather than to improve them by appropriate price changes.

2. Incentives, prices, and methods of accounting (cost calculation) together form an organic whole which we propose to call a *formula* for administration of socialist production. This formula is composed, then, not of two, but of three, elements. The behavior of the enterprise depends not only on the structure of prices and incentives, but also on the rules of accounting, i.e., on the regulations governing the cost calculation, receipts, and profits. This behavior depends, moreover, on all kinds of constraints imposed upon the enterprise within the framework of the existing formulae. These include, among others, regulations, mentioned by Montias in his comments, concerning the size of the labor force which can be employed by the enterprise (wages fund), the rationing of some of the material means of production, and the cost limits.

It is evident that this behavior of the enterprise, in the process of plan fulfillment, depends not only on the adopted price system (the parametric part of the formula), but to an equal extent on the nature of these regulations.

3. Montias suggests that the analysis should cover only some of the con-

straints (it would be better to use the term "regulations" since this covers positive orders as well as prohibitive ones). Does this constitute a real improvement of the analysis?

This does not give a more realistic picture, since these constraints are more or less arbitrarily chosen by Montias. The question arises: are these the most important ones? For example, all the regulations concerning the firm's internal accounting have been completely overlooked by Montias. To consider only some of the regulations without showing their relation to the rest does not bring us in any obvious way nearer to a realistic solution.

The choice of regulations made by Montias does not seem to be a good one: they are not the most characteristic, nor are they the most fundamental from the point of view of the management of socialist industry. (a) The wages fund and the rationing of the material means of production are not rigid constraints on the activity of the enterprise. Indeed, this would be contrary to the very essence of a socialist economy, which is based upon the overfulfillment of plans. Consequently, overfulfillment of the plan generally enables the enterprise to increase its wages fund as well as to be assigned additional material means of production. (b) Regulations concerning production costs can easily be bypassed by avoiding in production what is known as long series in favor of a short one. The term "long series" means production of the same assortment of goods over a number of planning periods, thus enabling the planning authorities to require a reduction or, at least, to keep constant the level of cost per unit of output. A short series of output is one that falls within a single time-period of planning. (c) Obstacles in the form of limited productive capacity are overcome in socialist enterprises by means of wider subcontracting. This gives enterprises other advantages as well.

4. We consider Montias' comments as incorrect to the extent that he has reduced the status of the enterprise to a few untypical regulations, thereby disregarding other much more important components of the third element of the formula. Certainly it is possible to suggest many other alternative constructions, equally far from reality and unsuited to the practical management of socialist production.

We would recognize Montias' comments as a valuable development of our model if it were stipulated that the constraints discussed by him are being treated only as *examples* of changes in the formula which entail the necessity of appropriate changes on the price side.

These links between prices and the rules of accounting are obvious. This was brought out in our article, as well as in later publications devoted specifically to the third element of the formula.

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The Criterion for an Adverse Balance of Payments: Comment

In a recent communication [5], Wendell Gordon has argued that the procedure used by the Department of Commerce for measuring the U.S.

payments deficit or surplus is seriously inadequate. According to Gordon:

1. The Commerce procedure "lacks symmetry." For a given gold-stock change, an increase in foreign liquid dollar assets implies a corresponding increase in the U.S. payments deficit, but a rise in U.S. owned short-term assets abroad does not represent any corresponding reduction in the deficit. Such asymmetry is illogical because "United States holdings of short-term balances abroad . . . could be used to cushion the shock in the event foreigners should withdraw their dollar balances on short notice."

2. Any rise in foreign liquid dollar assets (with a given gold-stock change) should not be viewed as a worsening of the U.S. payments position. On the contrary, such a rise reveals that foreigners have confidence in the "financial reliability" of the United States for "Short-term balances are simply not kept in financially weak centers."

3. Accordingly, "a pure 'gold holdings' figure might be better as a measure of what is happening to the balance of payments than the present lop-sided hybrid," a creature which is further "adulterated" because it includes changes in convertible currencies held by the U.S. monetary authorities. (For given changes in the gold stock and in liquid dollar liabilities to foreigners, the Department of Commerce counts an increase in U.S. official holdings of foreign convertible currencies as indicating a reduction in the U.S. payments deficit.)

4. In any meaningful sense, the United States has not recently had "serious balance-of-payments troubles," though it may "in the next decade or two." All of us—from the man in the street to the President of the United States—have been misled by the Department of Commerce's inappropriate technique for measuring the payments deficit.[1]

As the following comments attempt to show, none of the above points withstands analysis:

1. It is true, of course, that the Commerce Department's procedure of not netting changes in U.S. *private* short-term assets abroad against changes in foreign liquid dollar assets is asymmetric.[2] In itself, however, this is no condemnation. The relevant question is: How meaningful and useful is such a procedure? The experts disagree, mainly because they want to use the balance-of-payments figures to answer different questions [3] [6] [7] [8]. For most purposes, a good conceptual case can be made for treating symmetrically flows of domestic and foreign private short-term capital that are highly *interest-sensitive* [6, pp. 149-54]. But as Walther Lederer has emphasized (and as Professor Gordon fails to note), many U.S. private short-term assets abroad represent credits to foreign borrowers which could not be quickly mobilized by actions of the U.S. monetary authorities [7, pp. 116-17]. More generally, foreign central banks and governments have greater influence over the foreign investment policies of their nationals than the

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[1] Gordon does not state this interesting proposition in so many words, but it is clearly implied in his concluding two paragraphs [5, p. 141].

[2] Changes in one kind of short-term asset, namely U.S. official holdings of convertible foreign currencies, *are* netted against changes in foreign liquid dollar holdings in reckoning the "official deficit." But as noted above, Gordon views this as "adulteration."

authorities in this country have over the foreign investments of Americans. In particular, foreign commercial banks hold a large part of their secondary reserves in foreign-currency assets, especially deposits in the Euro-dollar market. By adjusting the rate at which they provide forward cover for these investments, or by administrative changes or moral suasion, foreign central banks can easily "mobilize" these short-term bank investments to defend their exchange rate [1, p. 85]. No comparable possibility exists for the monetary authorities in this country because U.S. banks do not hold any significant amount of secondary reserves abroad.

2. In Professor Gordon's view, there is no reason for concern over the rising liquid dollar holdings of foreigners, for this development indicates that our balance of payments is viewed with equanimity by foreigners. This view of the recent growth of foreign liquid dollar claims seems untenable. As the following analysis suggests, this growth has stemmed from a very real weakness in the U.S. payments position and provides no support for the thesis that foreign confidence in the dollar has been unimpaired by recent balance-of-payments developments.

In the five-year period, 1958-62, U.S. liquid liabilities to various types of foreign holders increased as follows:

Type of holder	Amount ($ billion)	Per cent of total increase
(a) Central banks and governments	$3.9	42%
(b) International Monetary Fund	1.9*	20%
(c) International development-lending institutions	1.4	15%
(d) Foreign private holders	2.2	23%
	$9.4	100%

* Excludes increase in U.S. subscription in June 1959.
Source: Fed. Res. Bull., April 1963, p. 423.

(a) For reasons arising out of the political and economic history of the 1930's and 1940's, most foreign monetary authorities outside the sterling area customarily keep part of their official reserves in liquid dollar assets.[3] (The customary proportion varies among countries and may change over time for any particular country, though seldom very sharply in the short run.) Considered as a group, foreign countries have increased the proportion of their reserves held in gold rather than in dollars during the five years ending in 1962.[4] Moreover, the number of important foreign reserve-holding countries increasing the gold share of their reserves has exceeded those increasing the dollar share, though the difference is small.[5] In the light of these facts, the recent $3.9 billion growth in foreign official dollar holdings can hardly be interpreted as a sign of confidence in the dollar.[6]

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[3] If most countries did not keep some of their reserve gains in dollars or sterling, the existing international payments mechanism would be more fragile than it is. Given the present price of gold, the present distribution of world reserves, the unwillingness of governments to alter exchange rates, and the understandable reluctance of monetary authorities not to sacrifice domestic for external objectives, there is—in a very real sense—not enough gold to go around. Under Secretary of the Treasury Roosa has indicated that the United States intends to accumulate foreign exchange when its payments position permits, in order to help meet the long-run need for growth in "world liqudity" [9]. Similar intentions have been expressed by the Federal Reserve Open Market Committee [10, p. 40].

(b) The rise in the I.M.F.'s dollar holdings (excluding the increased U.S. subscription in 1959) has stemmed (i) from the repayments of dollars borrowed by a number of leading countries during 1957, notably the United Kingdom and France, and (ii) from the sale of $600 million of gold by the I.M.F. to the United States in 1959 and 1960. This rise has weakened the U.S. international liquidity position, for it has increased the likelihood of foreign official gold purchases and has reduced the amount of potential U.S. drawings from the Fund.[7]

(c) The rise in the dollar holdings of the development-lending institutions shows only that these institutions have borrowed (or obtained through grants) long-term funds at a rate exceeding their disbursements. As a matter of policy, these institutions hold most of their interim funds in dollars.

(d) The $2.2 billion increase in foreign private dollar holdings reflects many diverse factors, notably: (i) the growth of advances by Canadian banks to their New York agencies, and by U.S. bank branches abroad to their head offices, for employment in the U.S. market—these advances involve no exchange risk because of the corresponding dollar-denominated deposit liabilities of the Canadian banks or of the U.S. bank branches abroad; (ii) the increase in money-market assets in New York by other foreign banks to meet liquidity and working-balance needs arising from the tremendous growth of transactions in the Euro-dollar market; (iii) the growth of foreign deposits at U.S. banks to meet compensating balance requirements imposed by U.S. banks lending to foreigners.

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[4] Excluding countries in the Soviet bloc and in the outer sterling area (as well as Spain and Portugal, which held most of their exchange reserves in sterling), foreign countries held 47.9 per cent of their official reserves in foreign exchange rather than in gold at the end of 1957; at the end of 1962, this percentage had declined to 41.4 per cent. It is reasonable to assume that nearly all of these foreign exchange holdings were in U.S. dollars [12, pp. 17-19].

[5] I define "important foreign reserve-holding countries" as foreign countries with reserves in excess of $250 million at the end of 1962. Excluding countries in the Soviet bloc and in the outer sterling area (as well as Spain and Portugal), there are 18 such countries. Of these, 11 decreased the proportion of their reserves held in foreign exchange (presumably mostly in dollars) from the end of 1957 to the end of 1962 [12, pp. 17-19]. Two of these 11 countries—Japan and Britain—had large IMF drawings outstanding at the end of 1957, but not at the end of 1962, which may explain the decline in their dollar-gold reserve ratios. If these two countries are excluded from the comparison, there were 9 out of 16 countries which decreased the proportion of their reserves held in dollars. (France also had large IMF drawings outstanding at the end of 1957, but she is not one of the 11 countries which decreased the proportion of their reserves held in dollars during the period in question.)

[6] Actually, $0.3 billion of this amount consisted of foreign-currency-denominated U.S. Treasury securities, free from any exchange risk. (See *Daily Statement of the U.S. Treasury,* Dec. 31, 1962, p. 7.)

[7] The U.S. drawing potential has not been reduced by its gold purchases from the Fund, but has been reduced by other countries' dollar repayments to the Fund.

These three factors probably account for a large part of the total $2.2 billion increase in private foreign liquid holdings in recent years. Consequently, this increase also reveals no special confidence in the dollar,[8] but merely reflects the dollar's widespread use as a means of international exchange and the United States' role as the world's largest exporter of capital. This may well be an appropriate role for the world's richest country, but it does not strengthen our payments position.

3. In Gordon's view, the best measure of the deficit (or surplus) would be the change in the U.S. gold stock. Thus he would count (a) decreases in U.S. official holdings of foreign currencies, (b) increases in foreign private liquid dollar holdings, and (c) increases in foreign official liquid dollar holdings as "above-the-line" credits serving to reduce the deficit rather than as "below-the-line" items helping to finance or settle the deficit.

(a) Excluding changes in U.S. official holdings of foreign convertible currencies from the measure of the deficit seems illogical because such changes are a direct substitute for foreign purchases or sales of gold. For example, if a foreign central bank accumulates more dollars through the exchange market than it prefers to hold, it may seek to buy gold from the United States. Such a gold purchase can be forestalled if the U.S. Treasury or the Federal Reserve uses some of its foreign-currency holdings to buy the "excess dollars" from the foreign central bank. Alternatively, a foreign central bank might acquire needed dollars by selling its own currency to the U.S. authorities; in accepting additional foreign-currency assets, the U.S. authorities obviously forego the opportunity to acquire gold [2, p. 313].

(b) As noted above, a case can be made for counting an increase in certain types of private U.S. short-term foreign assets as an offset to increases in certain types of foreign private liquid dollar claims; this means treating the latter as above-the-line "capital inflows" which reduce the size of the deficit. Treating all foreign private dollar accruals in this way seems inadvisable, however, especially if one's purpose is to measure changes in the capacity of the U.S. authorities to defend the dollar from speculative attack.

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[8] It can be argued that the rise in dollar investments in the United States by Canadian and European banks operating in the Euro-dollar market is a sign of confidence in the dollar even though these investments are fully hedged against exchange risk. For such investments may be the counterpart of dollar placements with foreign banks by foreigners—and these placements may not be covered against exchange risk, or may be covered through a forward sale of dollars to another foreigner whose purchase of forward dollars represents an outright "long position" in dollars.

This argument seems unpersuasive because: (1) A large part of foreign bank holdings of liquid dollar assets in this country may be viewed as the counterpart of dollar deposits placed with Canadian banks by U.S. investors—such deposits probably increased by more than $0.5 billion in the five years ending December 1962; (2) a good part of the exchange cover for foreign dollar placements in the Euro-dollar market is provided by foreign central banks selling spot dollars to, and buying forward dollars from, their commercial banks [1, pp. 55-56]. These purchases of forward dollars by foreign central banks may be viewed as expressing confidence in the dollar but, in my opinion, they are more plausibly viewed as reflecting a willingness by foreign central banks to undergo some exchange risk in order (a) to reduce the liquidity of their domestic banks, and (b) to reduce their official dollar holdings through some means other than buying gold from the United States.

(c) In any event, the exclusion of foreign *official* dollar accruals from the total of settlements financing the deficit seems most undesirable. For example, suppose that in each of two periods the United States has a $2 billion deficit on current and long-term capital account, with—for simplicity—no change in foreign private dollar holdings and no change in U.S. holdings of short-term assets abroad. Suppose, also, that the rest of the world is divided into two "countries," A and B. A maintains all of its reserves in gold and B maintains all of its reserves in dollars. Consequently, if A and B each have an over-all surplus of $1 billion in the first period, U.S. gold losses would be $1 billion. Suppose, now, that in period two A's surplus rises to $2 billion and B's surplus vanishes. In this case U.S. gold losses would also rise to $2 billion. Alternatively, if B's surplus rose to $2 billion and A's surplus fell to zero, U.S. gold losses would also fall to zero. But such changes could occur merely because of a shift in the bilateral balance of trade and long-term capital flows between A and B; i.e., they could occur with absolutely no change in the composition, amount, or direction of U.S. exports, imports, or long-term capital flows. On Gordon's criterion, however, the U.S. deficit would either double or disappear merely because of this change in the bilateral balance between A and B. Any definition of "the deficit" which allows such a result seems absurd.

It might be argued, of course, that the over-all transactions of the United States are affected in a significant way by the particular bilateral balance between A and B. If A has a bilateral surplus with B, the United States will be forced to sell gold; if B has a bilateral surplus with A, the United States will receive a short-term official loan as B adds to her dollar reserves. Measuring the U.S. deficit by the change in our gold holdings amounts to treating such short-term capital inflows as reducing rather than as financing our payments deficit.

This treatment of foreign official dollar accruals seems undesirable on three counts:

(i) Foreign countries view additions to their official dollar holdings (resulting from a surplus in their current and long-term capital accounts) as indicating a surplus in their balance of payments and an improvement in their international liquidity position. On grounds of symmetry, therefore, an increase in these holdings (for an unchanged gold stock) should be treated as indicating a deficit in our balance of payments and a deterioration of our liquidity position, i.e., as a means of financing—not reducing—our deficit.

(ii) Foreign central banks determine the gold-dollar ratio of their official reserves on the basis of custom, prudence, and law—not on the basis of the relative level of interest rates in the United States [4, p. 366]. Actions by the U.S. monetary authorities (aside from such measures as extending guarantees against exchange risks) can do very little to affect these ratios. In contrast, the U.S. monetary authorities can affect inflows of foreign private capital by altering the relative level of interest rates and credit availability in this country. This difference alone seems sufficient for treating an "inflow" of foreign official capital differently from an inflow of foreign private capital.

(iii) Finally, measuring the U.S. payments deficit by changes in our gold stock would be meaningless from an exchange-market viewpoint. As Walter Gardner has argued [3, p. 196], international transactions having an independent economic or political origin, and hence an "autonomous" impact on the exchange rate, should be distinguished from "compensatory" transactions by the monetary authorities designed to prevent the exchange rate from being forced above or below certain limits (or from moving too rapidly within those limits). Whether a foreign central bank converts a large or a small part of its compensatory dollar accruals into gold through a direct transaction with the U.S. Treasury can have no effect on the earlier net balance of autonomous exchange transactions which induced its exchange-market intervention. In a significant "exchange market sense," however, the net balance of these autonomous transactions *is* the balance-of-payments deficit or surplus. Measuring the U.S. deficit by only that portion of foreign official dollar accruals that are converted into gold would give a very false indication of this deficit. (There are certain practical objections against using an unadjusted official settlements concept of the deficit for an exchange-market analysis [6, pp. 18-19]; but these are beside the point at issue here.)

4. The final implication of Gordon's communication is that the United States has not had a serious balance-of-payments problem in the past few years. This contention hardly squares with the evidence. In the five years ending December 1962, our official reserves of gold and foreign currency holdings fell by $6.4 billion, while our liquid liabilities to foreign central banks and governments, to the I.M.F., and to other international institutions rose by $7.1 billion. These changes represent a decline of 28 per cent in our official reserves and an increase of 66 per cent in our liquid liabilities to foreign official and international institutions [11, p. 423]. Mainly as a result of our balance-of-payments deficits, the ratio of our official reserves to our liquid liabilities to nonprivate foreign holders fell during these five years from 2.1 to 0.9—a striking deterioration in our international liquidity position.

This deterioration has occurred, moreover, in spite of our sluggish economic performance as compared with other leading industrial countries, which has probably improved our current account by more than it has harmed our capital account, and in spite of a host of policy actions to reduce our reserve losses and the reserve gains of certain leading surplus countries on the Continent.

On almost *any* definition, our recent deficits would have been larger—perhaps considerably larger—had we not tied most of our economic aid, persuaded our allies to buy a larger amount of their military equipment in this country, received large debt *pre*payments from France and Italy, kept our short-term interest rates from falling, made a vigorous effort at export promotion and price stability, and benefited competitively from the relaxation of discrimination against our exports and from the German and Dutch revaluations.

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The Criterion for an Adverse Balance of Payments: Reply

There seem to be differences in (1) the basic criterion that is used in the balance-of-payments statement for assessing disequilibrium, (2) the basic principles that ought to be used for assessing disequilibrium, and (3) the special knowledge that the Board of Governors or the Department of Commerce may have which may justify identifying as disequilibrium something that corresponds neither with the criterion in the statement nor with reasonable basic principles. It is probably not desirable that there be such differences; but they appear to exist.

The balance-of-payments statement does try to identify a criterion. It was line 48 in the old format, and it is line 51 (or 51a) in the format introduced in the June, 1963 issue of the *Survey of Current Business*. In the old wording it was "reduction in gold and convertible currency holdings and increase in U.S. liquid liabilities." In the new wording it is "reduction in monetary reserve assets and increase in liquid liabilities including U.S. Government non-marketable, medium-term convertible securities." In spite of the difference in wording the two concepts are substantially similar. President Kennedy in his message on the balance of payments as late as July 18, 1963 in fact used the old wording.

In my original communication I alleged that it lacked symmetry to use as an element in the criterion an increase in foreign holdings of liquid dollar assets unaccompanied by an offset for either United States private short-term capital abroad or United States government foreign currency holdings and short-term claims. Also I alleged that the willingness of foreigners to hold liquid assets in the United States may well be evidence of the financial strength of this country rather than being evidence of financial weakness.

I believe that Dr. Goldstein has not questioned these general principles. And the circumstance of the increase in the rediscount rate in July from 3 per cent to 3.5 per cent to attract foreign short-term funds would seem confirmation of the principle that it is desirable for foreigners to hold balances in this country. And yet, if the increase in the rediscount rate should succeed in attracting an increase in foreign holdings of liquid dollar assets, the balance of payments statement would duly record that as an adverse development.

Dr. Goldstein, knowing what has been behind some of the financial manipulations of the period since 1958, indicates that United States financial weakness has been involved in the building up of certain of the foreign-owned balances here. Apparently United States monetary authorities have put strong pressure on foreigners to accept short-term balances (or the new nonmarketable, medium-term convertible securities) instead of demanding gold. Consequently he is probably correct in declaring that the United States needed to recognize that the building up of those particular balances was a threatening development, threatening because the foreigners accepted them under duress and might liquidate them at our inconvenience.

But this avowal has strange implications for the balance-of-payments criterion of adversity. Let us say that the foreign country obligingly keeps these balances for two or three years and then demands gold for them in 1966. This is the development that could really put pressure on the United States monetary system because of the 25 per cent gold reserve requirement involved in the backing of Federal Reserve credit. How would the balance of payments criterion evaluate these developments in 1966? The reduction of foreign holdings of liquid dollar assets would be reported as a favorable development; the loss of gold would be reported as an unfavorable development. The two developments would cancel out, and the criterion of adversity would indicate that this composite of operations had had no net effect one way or the other on the favorableness or adversity of the United States balance-of-payment. And yet this is exactly the sequence of events that would really put pressure on the dollar.

In his discussion of point one, Dr. Goldstein says that "foreign central banks and governments have greater influence over the foreign investment policies of their nationals than the authorities in this country have over the foreign investments of Americans." This would be an argument to justify the lack of symmetry mentioned earlier. If true, this might be an argument for excluding United States private short-term capital abroad from the criterion. But it is hardly an argument for excluding U.S. government foreign currency holdings and short-term claims. But is it true in the black and white sense that justifies 100 per cent inclusion of increase in foreign holdings of liquid dollar assets and 100 per cent exclusion of U.S. private short-term capital abroad? It seems doubtful that foreign governmental control of the private balances of their nationals in this country is as effective as Dr. Goldstein implies. In recent U.S. government documents various "experts" have guessed the foreign short-term balances of Latin America at figures ranging from $5 billion to an unbelievable $25 billion. Much of this is private balances held in the United States and in numbered accounts in Switzerland. It has been pointed out to Latin American governmental officials that they could dispense with a good deal of foreign aid if they would mobilize these resources to finance the import of capital goods. But they argue quite emphatically that it is beyond their power to mobilize such balances. I wonder if Dr. Goldstein really believes that Latin American central banks and governments have significant control over those funds. It could have important implications for the Alliance for Progress if they do.

With regard to Dr. Goldstein's third point, I simply did not say that "the best measure of the deficit (or surplus) would be the change in the U.S. gold stock." In fact the moral of all this may well be that there is no single "best measure" and it is a mistake for the balance-of-payments statement to be set up in a manner that suggests that there is. However, I do believe that it would be a good idea to have an entry in the statement showing net gold movement. The Department of Commerce should be able to provide space for this if it can find space for three different types of U.S. government securities.

With regard to his fourth point, I do not believe I made a statement which is appropriately summarized as: "In any meaningful sense, the United States has not recently had 'serious balance of payments troubles,' though it may 'in the next decade or two.'" It seems to me that I left up in the air the question as to whether there has been significant balance-of-payments trouble. What I did argue was that the basic criterion that is used to measure adversity in the balance of payments is inadequate.

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BOOK REVIEWS

General Economics; Methodology

Essays on Economic Semantics. By Fritz Machlup. Edited by Merton M. Miller. Englewood Cliffs, N.J.: Prentice-Hall, 1963. Pp. xxxii, 304. $5.00.

This book of essays by Professor Machlup has been brought out in honor of his sixtieth birthday by a former pupil, Professor Merton H. Miller. It is intended particularly as a tribute to Machlup as a teacher, and it consists of a number of papers in which fundamental concepts, liable to ambiguity and controversy, are examined with great patience, precision, and lucidity. The first group of papers is on "Semantic Issues in Economic Methodology." They show how terms may be used in a confusingly wide range of senses, as "Statics" and "Dynamics" were (particularly in the thirties; there is rather more congruence now); how a term, such as "equilibrium," may be used either without operational meaning, or with "persuasive," normative-positive ambiguity; and how a term such as "structure," or "structural," may be used as an impressive sign or noise while virtually void of content. Even if acute danger of serious confusions may now, for the sophisticated at any rate, have faded, there is much instruction and illumination to be derived from these essays and all graduate students of economics would do well to read them. The last of this group of papers on "Micro- and Macro-Economics: Contested Boundaries and Claims of Superiority," seems less convincing. This pair of terms is surely quite convenient for the sort of rough comparisons for which they are usually employed, and some of the errors Machlup is here concerned to refute seem extraordinarily crude—he is too polite to say who has perpetrated them—such as that "macro-economics" is more "dynamic" than "micro-economics" (p. 114), or in some sense "superior" (p. 144).

In the next section, "Semantic Issues in Value Theory," the main item is Machlup's well-known paper "Marginal Analysis and Empirical Research" (of 1946), which raises an important methodological question. Machlup argues most lucidly and elegantly that certain empirical evidence should not be taken as refuting "the marginal theory of business conduct of the firm" which "has not been shaken, discredited or disproved by the empirical tests discussed in this paper" (p. 190). But what is not clear, and what it is vital to know, is just *what* empirical material, if any, trivial or nontrivial, *would* refute "the marginal theory," or how far, on the other hand, the "theory" is almost, or completely, irrefutable empirically. Machlup doesn't attempt to explain this. It is, in fact, obscure how far it is an empirical theory which is being discussed, or how far it is the usefulness of concepts, or "marginalism," or possibly some kind of empirically irrefutable "principle." Doubts increase as to how far any empirically refutable theory is really at stake when Machlup concludes that "empirical research on business policies cannot assure useful results . . . if it aims at testing too broad and formal principles rather than more narrowly defined hypotheses" (p. 190), and goes on to state in a subsequent paper (p. 207) that "it would be preferable to speak of a 'marginal productivity principle' which can be used in different theories" (p. 207). Is this "principle" not empirically testable, though the theories in which it is used somehow are empirically testable? Surely there is an ambigüity here, and a question of methodological principle, which ought to be clarified for students. Incidentally, further questions regarding Professor Machlup's conception of the relevance of empirical research are raised by the following distinction between "realism" and "relevance": "In a realistic model the relevant factors may be swamped by all sorts of non-essential details, whereas in a relevant model everything is left out *that in a series of mental experiments has proved non-essential,* that is, has been shown to have little or no influence upon the deduced end-results" (p. 135, italics added). Does this perhaps mean that *empirical* research—except for some kind of mental experiments—is irrelevant for "relevance"? Or doesn't it?

The last two papers are not especially concerned with semantic issues, but with current policy problems. The paper on cost-push and demand-pull inflation is a most valuable piece of elucidation, and that on economic development sorts out the wide range of differing and contradictory views which exist about most aspects of development problems. The two papers seem to be models of how economic policies should be discussed by academic economists.

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Essays in Economic Method—Selected Papers Read to Section F of the British Association for the Advancement of Science, 1860-1913. Edited by R. L. SMYTH. London: Gerald Duckworth & Company, 1962. Pp. 284.

Essays in Economic Method collects some of the Presidential papers read to Section F, the Economics Section of the British Association for the Advancement of Science, between 1860 and 1913. It begins with Nassau Senior and ends with Wicksteed, including also papers by Jevons, Sidgwick, Cunningham, Cannan, Ashley, and others. Together the papers present a less bristling English version of the *methodenstreit.* Classical economics, the historical study of institutions, deduction, poverty, *laissez faire,* free-trade, socialism, and marginal utility constitute the world of discourse, while the names of Smith, Ricardo, John Stuart Mill, Marx, Roscher, Comte, and Marshall wind through the discussions and polemics of the essays. What emerges from the whole is less a battle by men who made the age than contained controversy by men who are the age. The essays are not individually important. Their value lies in the way they collectively delineate 50 years of the changing thoughts of economists.

It may be that Nassau Senior's 1860 paper is not important in itself, but in interpreting economics to be the study of the universal laws of wealth, unencumbered by "any of the other physical or moral sciences" and indifferent whether "wealth be good or evil," he created a polemic. And a decade later, Jevons too sought laws, like those of physics, as guides for policy, invoking basic principles to support a modified claim to *laissez faire.* But Comte had already laid the foundations for the new science of society in which economics played no separate role, while the Germans had denied the abstractions of Ricardo and had sought the substitution of empirical and historical foundations in their search for social organism. Reflecting this discontent with classical economics, Professor Ingram (1878) attacked the isolated study "of the facts of wealth" as well as its dogmatic and "viciously abstract" deductive character. Economics was but part of "the general science of society." The laws of "the economic constitution and movement of society are first obtained by observation, whether directed to contemporary life or to the history of the past." Economics, he protested, was fundamentally an historical science, a science with little room for a priori principles.

However, deduction finds its advocate in Sidgwick (1885) and history in Cunningham (1889); but each with moderation. Sidgwick then denied the universality of self-interest and applied it merely to the "ordinary exchanges of products and services." Deductive reasonings supply "a method of analyzing the phenomena and a statement of the general causes that govern them"; it is not necessary first to ascertain "from the past history of human society the fundamental laws of social evolution as a whole." But *laissez faire,* he would agree, is not itself deducible from a priori principles, nor are the rates of wages naturally determined. And Cunningham, while advocating the historical approach, could maintain that abstract statements "of what tends to happen" in "free competition affords an admirable instrument of scientific investigation for modern society," although these tendencies need not be taken as established for society in general. Thus, when Nicholson (1893) asserted that Smith, Ricardo, Mill, and Cairnes "must still be regarded as fundamental," his voice seemed lost. Orthodoxy was retreating in theory and in practice. All good theory, Bastable (1894) noted, must include "the particular forms of social structure and the special political conditions which have helped to produce them."

The orthodoxy dead, Price (1895), Bonar (1898), Cannan (1902), and Ashley (1907) take stock. Practical problems require general principles, but always in their special contexts. Bonar accepts that "economists are content with less than universality; they are working out a limited field with full consciousness of its limitations." The best thoughts of today may disappear "as careless English of the nineteenth century." Practical does not mean private business, and Cannan notes the usefulness of economic theory to lie in politics. Private property does not exist "for the sake of the particular kind of distribution which it causes." Ethics, it appears, is part of economics. Finally, in Ashley, there is the sense of the bygone times of

Smith, Mill, and Cairnes, whose "sweeping assertions . . . sound antiquated to our ears." Indeed, he finds no new orthodoxy arising. The deductive, statistical methods of marginal utility carry "us a very short way indeed," while there has "as yet been no substantial reconstruction among English-speaking economists on *historical* lines." Ashley senses, regretfully, a "sharp division . . . between economic theory and economic history." This divorce, however, generated the academic fervor of the age.

Little of this air of controversy appears in Bowley (1906) or in Wicksteed (1913). Bowley's technical essay states the need for scientific measurement. He deplores the imperfections of the statistical methods and their consequences for policy and theory. Wicksteed, in what is now a well-known essay, asserts a "differential theory of distribution." There is no apology in his use of deductive method; he has taken it for granted. The marginalist theory of resource allocation (he would eschew the term "marginalist") he asserted with succinctness and clarity. Production, exchange, and distribution were explained by a commonly deduced system of theory. "Costs of production," supply, rent, and surplus were all reduced to a common set of axioms of differential choice in which economics, once again, was remolded in the postulate-deductive form.

These essays, edited by R. L. Smyth, are instructive because they reflect the tone of British economic thought for 50 years. They provide a climate of opinion more than a rigorous statement of the logic of doctrinal change. The concern with grand issues of social and economic policy and of method seems almost foreign to the questions raised today. However, the student of doctrines, that lamentably vanishing American, will find the essays interesting and useful, though not commanding. He will also discover in T. W. Hutchison's excellent introduction a more satisfactory review than I have been able to present.

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Economics—A General Introduction. By LLOYD G. REYNOLDS. Homewood, Ill.: Richard D. Irwin, Inc., 1963. Pp. 739. $7.95.

I think that in most large universities the selection of a textbook for the principles of economics course is a group decision. The relevant decision rule seems to be one requiring something like consensus, so interposition is possible (coalition formation and majority rule are reserved for more critical issues, such as curricula). The deciding group generally consists of a hodgepodge of specialists. The result is that the group demands a textbook that consists of a weighted compromise of the specialties. An author who undertakes to write a book for this market is committed to the encyclopedic strategy. He must attempt to mirror the current state of the subject. This can be a fairly simple and satisfying task when undertaken while the subject is in a quiescent state. But economics is not a quiescent subject, and we are told that growth involves disequilibrium. Thus a subject in a volitant phase must cast some discordant reflections, even when the mirror is held by as skilled an imagist as Professor Reynolds. These limitations are shared with the other good principles texts, which suggests that the limitations are imposed by the contrary nature of the market demand, not by a defect in the supply process.

Reynolds has written a book for the general reader, rather than for the prospective major in economics. He discusses most of the important conceptual tools which characterize an economist, and he does so without error. Of course, he omits the covey of qualifications that would be required at a specialist's level.[1] I think he made the right choice. The book is pleasant to read. Reynolds' style is simple and straightforward, devoid of irritating quirks. A substantial amount of descriptive material is presented in tables, charts, and graphs. He handles data well, and the illustrative material is attractive.

Reynolds' discussion of current policy alternatives is quite judicious. He considers issues in the areas of agriculture, labor unions, balance of payments, antitrust, taxation, fiscal and monetary policy, and foreign aid. His position generally coincides with what seems to be the modal expression on these matters by competent economists. Reynolds skillfully integrates his theoretical material with these policy issues. It is possible that this choice prevented a more ambitious attempt to integrate the theoretical material.

So *Economics* is a carefully done textbook by a distinguished member of the profession. Rather than ending the competition, this just enables the book to enter the field of serious consideration. Economists have joined with publishers to produce an approximation to pure competition in the principles market. All of the good principles books, including Reynolds', incorporate the discipline's trinity—microanalysis, institutional material, and macroanalysis. Often authors will add a fourth section, which generally reflects their specialized interest and competence. In Reynolds' case this consists of a section on the economics of growth. The treatment is descriptive, and centers around the United States, the USSR, and the low-income countries. The paucity of relevant theory in this section is in contrast with the more traditional sections. Of course this is a shortcoming for which we are all partly responsible. But I think that corrective efforts such as Everett Hagen's are more productive than the attempt to extend macroanalysis to the growth problem.

Even though the leading texts are more similar than separate, a reviewer should indicate the degree of differentiation. Differentiation generally takes the form of different weights on topics rather than different topics. I know of no better way to approximate Reynolds' weighting system than to rely on his own summary, provided by the index. Usually the index is the last task undertaken by an author, and its structure provides a guide to the author's evaluation of what he has written. Some idea of Reynolds' book may be gained from the following comparisons.

He refers to more than 80 people in his text, most of them economists.

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[1] For example, Reynolds makes frequent use of the price equals marginal cost adjustment as a criterion of economic performance. This is a valid standard only when the firm is competitive in each of its factor markets. This is not a very probable circumstance if the firm has some degree of monopoly on the product market.

Most frequent reference is made to the work of Simon Kuznets, who leads his closest competitors (Joe Bain and Solomon Fabricant) by a factor of 3. Alfred Marshall and Lord Keynes receive equal weight, as measured by the number of page references.

The index contains the names of 25 countries. If we take the number of lines in the index associated with each country as an indication of the attention received from the author, we get the following measures for the six leaders: United States (51), USSR (27), Japan (11), India (10), Great Britain (9), and Sweden (6).

If the same device is used to evaluate the relative weighting placed on topics, the following order obtains: economic growth (45), foreign aid (39), economic development policies (29), low-income countries (29), market economy (29), wages (26), income (20), taxation (20), taxes (19), economic fluctuation (19), monetary policy (18), money (18), and interest rates (18).

The omissions also indicate something about the book. China is not listed in the index (although there are a few incidental comments in the text). There is no discussion of game theory or the related developments in the field of conflict resolution. The American Negro and his relationship to such issues as housing, occupational patterns, and poverty receive negligible attention. The acceleration principle is omitted. But an author cannot be a successful encyclopedist, and such choices as those indicated above must be made.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Essays in the Theory of Economic Growth. By JOAN ROBINSON. New York: St. Martin's Press, 1962. Pp. x, 138. $4.25.

The business of republishing in book form one's journal articles must, indeed, be immensely profitable. Recent years have witnessed an outpouring of "collected essays," immodestly selected for republication by the authors themselves. In the past, however, the urge to republish was apparently tempered (somewhat) by the wisdom of waiting until one's words or models stood the test of time. But the game must be waxing too hot to justify such restraint. In Mrs. Robinson's latest collection, we have three out of four essays that must have gone to press before the ink was hardly dry on the original journal articles. All three appeared in journals in the same year as that in which the book was published, viz., 1962.

The first essay, "Normal Prices" (*Quarterly Journal of Economics,* Feb. 1962) is designed to show the essential differences between a Walrasian general equilbrium of supply and demand and a Marshallian general equilibrium based on constant-supply (cost) prices. It is the latter that underlies the growth model in the following, and main, essay. Unfortunately, the discussion in the first essay abuts considerably on the next, making for much repetition and tedium in reading the book.

The third essay, "A Model of Technical Progress" (*Revista di Politica Economica,* Feb. 1962), is designed to "clear up some points" in the earlier version of the same topic (Section III of *The Accumulation of Capital*). Whether it illuminates anything is problematical. For one reader it makes fairly obvious (and familiar) points in some places and succeeds in obfuscating matters in others—by raising problems of the author's own creation, sometimes arising out of apparent *non sequiturs,* sometimes out of her own assumptions (implicit or explicit).

The last paper, "A Neo-Neoclassical Theorem" (with minor additions, from the *Review of Economic Studies,* June 1962), sets forth the familiar proposition that in an economy growing at its "natural" or "ceiling" rate, and characterized by perfect competition and constant returns to scale, per capita consumption is maximized when the profit rate (on capital) equals the rate of capital accumulation (or what is the same thing, as will be shown below, saving and investment equal profits). Since the theorem is treated more rigorously in three other papers in the same issue of the journal in which this essay originally appeared (and elsewhere as well), the purpose served by reprinting it in this collection is not clear.

Because the above three essays are readily available in the journals indicated, we shall confine the rest of this review to the second, and by far the longest and most important, essay, "A Model of Capital Accumulation," published for the first time in this collection. Its purpose is to "outline a generalization of the *General Theory*" in relation particularly to long-run growth problems, in the hope of making clearer what Mrs. Robinson was saying in her earlier work (notably the relevant parts of Book II of *The Accumulation of Capital*). We concentrate on the model's long-run or growth behavior, as opposed to the cyclical behavior, because the (abbreviated) discussion of cycles contains little that is new, and because it does not always square with the basic model. (There is an appendix to this essay comparing "classical," "neoclassical," and what she lumps together as "Keynesian" models of economic growth. It is very uneven, suffering, as does much of the rest of the book, too often from the typical Robinsonian defect of shabby treatment of whatever does not conform with her own preconceptions [or is it misconceptions?].)

The model of capital accumulation is a typical closed, two-sector (consumption and investment) model, with constant returns to scale. At the outset, six "determinants of equilibrium" are set forth:

(1) Technical conditions: In the main, these embody the determinants of the familiar "natural" or "ceiling" (called here the "possible") growth rate, viz., the rate of growth of the labor force and the rate of growth of output per head produced by neutral technological progress. However, as in some other models (notably those of Kaldor and Duesenberry), the latter varies with the state of labor scarcity, but to a lesser extent than is assumed in these others.

(2) Thriftiness conditions: Saving is done only by business firms and by rentiers; wage earners save nothing. (This assumption is not important, however, as a reading of Mr. Kaldor's model will reveal.) Given the saving propensities (left largely unexplained) of these groups, the aggregate saving pro-

pensity is determined by the distribution of income, changes in which occur mainly through changes in prices in relation to money-wage rates. In a manner described below, the aggregate saving coefficient combines with the rate of capital accumulation to determine the rate of profit.

(3) Competitive conditions: Elements of monopoly affect saving by their effect on income distribution; in an indeterminate way, they also influence the economy's investment propensities. They also affect price-level behavior, but apparently in an unimportant manner so far as the behavior of the model is concerned.

(4) The wage bargain: This affects the behavior of money wages in response to changes in prices. Principally, the wage bargain poses an "inflation barrier" to the realization of the "possible" growth rate by keeping real wages from falling, and the profit rate from rising, in response to a rise in the rate of capital accumulation.

(5) Financial conditions: These operate mainly to stop inflationary pressures, as banks (without a monetary authority) mysteriously limit the growth in money supply and credit and raise interest rates, so that the profit rate and the incentive to invest are reduced.

(6) Investment policy: Along with thriftiness, this is the most important of the determinants of equilibrium growth. At the same time, because of the absence of a clearly articulated theory of investment (she disclaims all ability to articulate one—by anyone), this is one of the weakest points of the model. On the one hand, investment (or, more accurately, the percentage rate of capital accumulation) is presented as an increasing (nonlinear) function of the expected profit rate. On the other hand, the rate of capital accumulation and the saving coefficient combine to yield a relation in which the profit rate is an increasing, linear, and homogeneous function of the rate of capital accumulation. An exact understanding of this second relation is crucial for understanding much of the model's behavior; hence we present its basis. First, remembering that all saving comes ultimately from profits, distributed and undistributed, we have the saving-investment equality, $sP = I$, where s is the saving coefficient in relation to profits, P is profits per year, and I is net investment per year. This relation is an equality, not merely an identity. How is equality between saving and investment *ex ante* achieved? By rises and falls in prices relative to money-wage rates as planned investment exceeds or falls short of planned saving. In other words, the resulting redistribution of income makes saving adjust passively to whatever planned investment is.

Now if we divide both sides of the above expression by s and K (for capital stock), we arrive at the expression:

$$\frac{P}{K} = \frac{1}{s} \cdot \frac{I}{K},$$

from which it can be seen that the profit rate, P/K, is an increasing function (and hence, incidentally, the real-wage rate is a decreasing function) of the rate of capital accumulation, I/K. The famous widow's cruse feature of this (and the Kaldor) model (namely, that the more capitalists spend the more they earn, while the opposite is true of wage earners) can also be seen in this second expression. For, given the rate of capital accumulation, the profit rate varies inversely with the saving coefficient. In addition, since the rate of accumulation is itself an increasing function of the profit rate, a higher rate of saving will result in a lower rate of capital accumulation. So now the familiar "paradox of thrift"[2] applies in the long, as well as the short, run. (An exception will be noted below.)

Where the schedule relating the rate of capital accumulation to the expected rate of profit intersects that relating the profit rate to the rate of capital accumulation, we get a "desired" rate of capital accumulation, since at the point of intersection the actual profit rate equals the expected one. This point of intersection is assumed to satisfy the usual stability conditions. If short-run conditions, principally the stock of capital and technical conditions, are such as to permit the realization of this desired rate of capital accumulation, then the economy will be in a state of general equilibrium, characterized by steady growth of investment, capital stock, and output.

From this point, the discussion runs in terms of developments when these two provisos are not met. First, the desired rate of growth is confronted with the rate of growth made possible by growth in labor force and output per head. If these two growth rates should be equal, and full, or near-full, employment prevails at the outset, then we have a "golden age," in which steady, equilibrium growth is marked by full employment. However, such equality of growth rates is a matter of chance; in the event that they differ, there is no automatic mechanism to render them equal—and this assertion marks a major departure from the argument in *The Accumulation of Capital.* There it was argued that in case the desired fell short of the possible rate of growth, for example, the pressure on money-wage rates and hence prices resulting from emerging unemployment might increase the *real* rate of accumulation, assuming that entrepreneurs aimed at maintaining the *money* value of investment (a reverse mechanism would work when desired growth rate exceeded the possible one). In the preface of the present book (p. vi), this assumption is acknowledged to be invalid, and hence this kind of "equilibrating" mechanism is dropped from the analysis.

Accordingly, if the possible growth rate exceeds the desired one, a "limping" golden age develops, characterized by growing unemployment. (A "limp" may also arise if the capital stock, though "appropriate to the desired rate of accumulation," is insufficient to employ the entire labor force. However, if the desired rate of growth exceeds the possible one, the system is heading towards full employment.) Growing unemployment is likely to be accompanied by falling living standards. When "Malthusian misery" checks the rate of population growth, the economy descends into a "leaden" age, in which unemployment and misery are great enough to hold the rate of population (and labor-force) growth down to the desired rate of capital accumulation.

When the desired growth rate exceeds the possible one, and full employment prevails, the excess demand for labor poses an inflationary threat in the form of rising money-wages and prices. Financial restraint and rising interest rates come into play to reduce the desired growth rate to the possible one. (Yet, for some reason never explained, a fall in interest rates never occurs to raise the desired growth rate to the possible one in the case of the limping golden age.) In this case, we have a "restrained" (but unstable) golden age.

A "bastard" golden age arises when the desired rate of accumulation is held in check, even in the presence of widespread unemployment, by the refusal of labor to accept a reduction in real wages that would result from rising prices induced by a higher rate of accumulation. (Why prices must rise in a state of widespread unemployment is left unexplained.) Financial restraints are imposed to stop price-money wage inflation, and they do so by holding in check the rate of accumulation.

It is in cases like the last two, when the rate of accumulation is being reined in as a result of full employment or of reaching the minimum real-wage floor (the "inflation barrier"), that the paradox of thrift does not hold in the long run; that is, a rise in the saving coefficient will be accompanied by a rise in the rate of growth. For greater thriftiness, by eliminating the need for financial restraint, will make more investment possible.

"Platinum" ages arise when there is either too little or too much basic capital stock to begin with, and growth is either accelerating or decelerating. The analyses of these situations closely parallel the above, so there is no need to review them here.

As it now stands, Mrs. Robinson's growth model lies between the Kaldor and Harrod models, with the neoclassical production-function model lying to the far extreme of the Harrod model. The absence of an output growth (production?) function comparable to Kaldor's "technical progress function" is the factor that keeps Mrs. Robinson's model from being, or at least behaving, for all intents and purposes like the Kaldor one. And judging from her apparently confused remarks on this function (pp. 118-19), there seems to be no good reason why she shouldn't go the whole hog (as she likes to say) and employ it in her model. Then her desired and possible growth rates would automatically merge in long-run equilibrium, and the economy would grow at the possible growth rate.

This being the case, the crucial aspect in Mrs. Robinson's (and Kaldor's) model would appear to be the assumption (originally due to Kalecki) that saving automatically adjusts to investment via changes in the distribution of income, with the resultant relation between investment and the profit rate shown in the second of the earlier equations. Is aggregate saving really determined only by income distribution in the long run? Or, as seems likely, do a number of other well-known factors also strongly influence the economy's saving propensity? If so, a critical part of the model, and therefore the model itself, is in doubt.[1]

Perhaps it doesn't really matter what determines equilibrium between saving and investment. Despite differences in assumptions about the determination of the aggregate propensity to save, and despite differences in the mechanics of obtaining long-run equilibrium growth with full employment, both the Kaldor and neoclassical growth models arrive at the same growth rates. Further, most of the important conclusions *in equilibrium* are qualitatively the same. So, given our limited knowledge of real-world technical and behavioral relations, what is the significance of the differences?

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[1] Moreover, the mechanism for equilibrating saving and investment makes much more sense in a near-full-employment economy, such as Kaldor postulates, than in an underemployment economy. Mrs. Robinson, however, implicitly suggests that the state of employment doesn't matter. But she then leaves unexplained, as mentioned earlier, why prices must rise in her "bastard" golden age in the presence of widespread unemployment. Accepted at face value, much of her discussion sounds suspiciously like an exact reproduction of the Austrian theory of forced saving, a theory I thought had received its final burial in Cambridge some time ago (with Mr. Kaldor officiating at the burial rites).

Or maybe Mr. Harrod is right after all. Who knows?

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Income Distribution and Social Change—A Critical Study in British Statistics. By Richard M. Titmuss. London: George Allen and Unwin; Toronto: University of Toronto Press, 1962. Pp. 240. $4.25.

The author of this book, who has written extensively and brilliantly in the field of social welfare, is a member of the Labour shadow cabinet. In this book, which is heavily laden with the politics of discontent of a party long out of power, Professor Titmuss aims at unsettling the statistical basis for the anti-egalitarian offensive of recent years.

He suggests that "Future historians with access to the public archives may find a fascinating theme to unravel in studying the connections over the past twenty years between the erosion of the tax base . . ., changes in redistributive social policies, reductions in the 'burden' of income tax and surtax that took place after 1951, and the search by governments for new and more effective fiscal systems. They will note the decline in favour of the progressive tax system along with the principle that ability to pay is the right democratic criterion" (p. 197). He sees all this as meaning "a gradual abandonment of the search for equity."

He explains this "gradual abandonment" as arising out of the growing influence of the "mass media of complacency," the mesmerization of the present generation by the language of the welfare State, and the spread of Alfred Marshall's idea of a built-in trend in the British economy toward greater equality (p. 188). The latter idea was given powerful support by the findings of sharp reductions in income inequality between 1938 and 1948. Hence, the author undertook a study having what he describes as a severely limited purpose, namely, ". . . to examine the statistical foundations on which law and opinion in the 1950's were based, to reinterpret the results of inquiries such as those made by Mr. Lydall and Professor Paish; and to analyse a number of social and demographic factors of change in their bearing on the questions of equity and welfare" (p. 20).

Since the basic data for comparisons of income distributions are sample studies prepared by the Board of Inland Revenue, he first addressed 78 questions to the Board concerning their sampling methods, grouping of incomes of family members, accounting for persons with no taxable income, and so on. The author claims he received few answers and seems to dispute some of those. It will come as no surprise to students of income distribution that tax data are found to be less than satisfactory for comparing distributions over time. But Professor Titmuss is also troubled by such fundamental changes as reduction in unemployment, increase in the marriage rate and higher employment of married women, and reduction in differential mortality. He finds that ". . . many profound changes have been at work, some of which have given the Board's tables a spuriously egalitarian look." However, he concludes, "The genuine effects as distinct from the spurious ones cannot be disentangled" (p. 52).

The author is somewhat more conclusive about the matter of income definition, arguing that the direction of error is clear. He cites with favor the finding of John A. Brittain that, if adjustments are made for missing investment income and undistributed profits, there was no leveling of pre-tax incomes between 1938 and 1949. He reviews in detail, but without estimating their effect, arrangements for spreading income over time and among family members and methods for obtaining tax-free income or benefits. Of particular interest to Americans are income covenants, which transfer income and tax liability to another person, and "the golden handshake," which compensates for loss of office (p. 79 and p. 134).

Titmuss does not press a specific conclusion about the facts. Rather, he turns aside from offering his own estimates and calls upon others to supply a broader and more analytical framework. ". . . the present mould into which the statistics are cast is increasingly delusive as a frame for knowledge. We are in fact expecting too much from the crumbs that fall from the conventional tables" (p. 191).

This weak conclusion leaves him in a defensive position. He similarly avoids following up two related points. One is that there is no evidence of a trend toward equality since 1949; the other is that 1938 is not necessarily the ideal baseline.

The book sets out to be critical of British statistics, and it succeeds in giving an unrelieved account of data limitations. Along the way it gives a less than inspiring picture of the British tax system. But one looks in vain for new insights into the field of income distribution. Most of Titmuss' points were made in U.S. literature a decade or more ago. See, for example, the several volumes published by the Conference on Research in Income and Wealth. Indeed, many of them were made by an earlier scholar-politician of Labour persuasion, Hugh Dalton, whose book on *Some Aspects of the Inequality of Incomes in Modern Communities* (London 1920) is not in Titmuss' apparently limited bibliography. Dalton examined similar questions and came to a similar conclusion. Perhaps he stated it just as well when he wrote (p. 351):

> The question whether the inequality of incomes is increasing or decreasing in modern communities is one of the most important questions in modern economics. Many writers have attempted to answer it, but their answers do not generally carry much conviction. . . . (It) involves the enumeration of a large number of distinct and conflicting tendencies, and

the weighing and balancing of them one against the other. . . . Conclusions pretending too much generality on this subject are to be mistrusted.

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Verteilungstheorie. By WILHELM KRELLE. Tübingen: J. C. B. Mohr (Paul Siebeck), 1962. Pp. xi, 299. DM 59.

Professor Krelle's treatise consists essentially of two parts. The first ten chapters are devoted to an introduction, a collection of illustrative statistics of which, however, no further use is made, and a survey of distribution theories from the classics to the modern macroeconomic theories. To the U.S. reader the critical discussion of the macrotheories of Bombach, Föhl, Preise, Niehans, and Erich Schneider might be of particular interest. Chapter 18 offers in a very condensed form a (comparatively) simple theory originally published by Krelle in 1957. This theory has been discussed by K. W. Rothschild in his excellent paper, "Some Recent Contributions to a Macro-Economic Theory of Distribution" (*Scottish Journal of Political Economy,* October 1961). In Chapter 19 a survey of some major theories of personal income distribution is given, notably those of Pareto, Gini, Lorenz, and Gibrat.

However, the real meat of the book, Krelle's new theory, is contained in Chapters 11 to 17. Although it too belongs to the class of macroeconomic theories, it constitutes a new departure in so far as it attempts to investigate the dependence of income distribution on a large number of the structural parameters appearing in models of the economy. In particular: share of cost of materials and of depreciation in values of total output; rate of increase of unit cost at production beyond optimum capacity (p. 118); degree of monopoly; capital-output ratio; marginal propensities to consume; effect of consumption during preceding period on current consumption; rates of direct taxes on wages, profits, and welfare payments; indirect tax rates; ratio of autonomous investment to total output; balance of payments surplus; budget deficit. Drawing on price, income, and employment theory, such a distribution theory is, in the words of the author, a culmination of economic explanation. To do justice to a work of so wide a scope within the compass of a review is hardly possible. This review will therefore concentrate on only the bare outlines of Krelle's new theory. Following most macroeconomic distribution theories, he divides aggregate income into the entrepreneurial share (including interest), Q, and the wage share, L. The analysis is restricted to the capitalistic sector, thus excluding agriculture, very small enterprises (*Handwerk*), and the professions. The banking system and the effects of changes in the supply of money are likewise disregarded. The argument is developed on three interrelated levels: distribution within the firm and industry, distribution in the capitalistic sector in the short and in the long run. In Chapters 12, 14 and 16 respectively the analysis is presented in mathematical form. Chapters 13, 15 and 17 contain corresponding summaries in literary form. In an appendix to Chapter 16 an analytical survey of techniques to measure technological progress is given.

On the firm level the analysis proceeds under the usual assumption that the firm's income is independent of its own factor payments. Individual firms are supposed to operate more and more with Leontief production functions, while industries continue to operate under conditions of classical production functions. On the whole, however, the form of the production function does not seem to have a direct effect on the functional relationships between the distribution Q/L, and the major parameters—the level of demand (intercept of demand curve on ordinate), slope of demand curve, cost of materials, ratio of wages to productivity, and indirect taxes. The wage share is found to be higher, the higher the wage rate with a given productivity, the higher the costs of materials, the higher depreciation, the smaller the intercept of the demand curve on the ordinate with a given slope, the greater its slope with a given intercept, and the higher indirect taxation.

The short-run analysis is concerned with the determination of Q/L at time t by the values of the structural parameters at $t - 1$. On the aggregate demand side, the consumption function used is essentially the one of the Klein-Goldberger model with disposable income divided among wages, profits, and welfare payments. The discussion of the investment function forms a highly condensed, but pretty exhaustive, analytical survey of current investment theory. The function finally chosen is a weighted combination of three functions in which investment depends respectively on interest, level of output, and profit. The three weights are important structural parameters. Total supply is obtained by aggregating the outputs of individual firms. In order to obtain Q/L in real terms, the price level is explicitly introduced as a variable. In the short run, budget deficits, balance of payments surpluses, and autonomous investment reduce the wage earner's share. Saving by the wage earners themselves increases it. Employment policy and attempts to increase the wage share are here, as in the long run, mutually exclusive. The actual form of the dependence of Q/L on the structural parameters changes with the rate of use of the existing capacity, i.e., with the phases of the business cycle. Depressions tend to be self-curing even in the absence of governmental intervention because there remains a certain volume of autonomous and profit-dependent investment or, at worst, there is a limit below which per capita consumption cannot fall.

Intermediary phases (*Übergangslagen*) are also unstable, leading to either boom or depression. Consequently, long-run equilibrium seems to require an output systematically exceeding the optimum capacity output with both output and capacity growing. By using plausible values for the structural parameters in the equations, Krelle computes numerical solutions for the equilibrium rate of growth and the equilibrium rate of capacity use. Local stability of the equilibrium within a narrow range is likewise demonstrated by illustrative computations; an analytical proof is not given and because of the complexity of the system apparently cannot be given (pp. 220 ff. and 250). The equilibrium distribution in the long run is then the one corresponding to the equilibrium rate of growth. Here the direction of changes in Q/L as the structural parameters change is opposite to that of the short-run changes and thus tends to cancel them out. Budget deficits, balance of payments surpluses, and autonomous investment now increase the wage share by reducing the rate of growth to which the wage share is inversely related. Also, an increase in the marginal propensity to consume of the wage earners increases their share, as does an increase in the propensity to invest (*Investitionsneigung*). Higher indirect taxes reduce the wage share, while higher direct taxes increase the wage share (before taxes). The explanation of this state of affairs is complex but depends essentially on an inverse relationship between the volume of output and the rate of growth (pp. 226 ff.).

The major result, however, obtained by Krelle from his three distribution models is that in the long run Q/L is highly insensitive to changes in the structural parameters, with the exception of the degree of monopoly (defined as the reciprocal of the elasticity of demand) and to some extent indirect taxation. His computations yield a range from 65 to 76 per cent of GNP for the variations in the wage share. Indirect taxes must change by 30 per cent and more before there is a measurable effect on Q/L. Thus only changes in the degree of monopoly may have a substantial effect on Q/L: under perfect competition the wage share may be as large as 86 per cent, while with a degree of monopoly of 1/5 it would fall to 50 per cent (pp. 240-41). Is this result worth the explicit inclusion of so large a number of parameters and the resulting mathematical complexities? In principle at least, the answer must be in the affirmative. Krelle's analysis provides an analytical explanation for what in fact has been observed statistically: the stability of Q/L in the long run. Yet, one point perhaps may be raised. In nonclosed systems where the number of possibly relevant variables is indefinitely large, the attempt at generality by the inclusion of more and more of them will sooner or later run into diminishing returns. To some extent this seems to have happened to the analysis under review: the mathematical expressions tend to become unmanageably long and complex. The interpretations and summaries given in Chapters 13, 15 and 17 are too far removed, not only in space but in logic, from the procedures and manipulations from which they are derived. Since the major results, such as the range within which Q/L can change in the long run, have been obtained by numerical computations, this reviewer believes that a more focal place in the presentation of the argument might have been assigned to these computations. It may be noted also that beyond some scattered and oblique remarks there is no systematic analysis of the implications of changes in Q/L for living standards (especially that of the wage earners) and for wage policy—which is not identical with distribution policy. Finally, the absence of a table of symbols makes the reading of the book unnecessarily difficult. This reviewer could find only a few misprints; but this may be due to his mathematical inability rather than to the publisher's ability.

All these are minor complaints, indeed, about a book which no doubt represents a major contribution to economic theory and which may become the basis for much future research on both the analytical and the empirical levels.

EMILE GRUNBERG

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The Corporate Revolution in America—Economic Reality vs. Economic Theory. By GARDINER C. MEANS. New York: Crowell-Collier Press, 1962. Pp. 191. $3.95.

This little volume by Gardiner C. Means brings together a series of essays and addresses ranging over a period of more than 25 years, all dealing with aspects of "administered pricing," which Means believes takes place when business is dominated by corporations of "tremendous" size with ownership separated from control by dispersion of stock ownership—a situation which he describes as "collective capitalism." I propose to comment first on what Means believes the existence of administered pricing implies for economic theory and second on what it implies for public policy.

The present collection starts with a summary of Means' Ph.D. thesis. Although the theoretical section was rejected at the time (1933) as insufficiently developed, Means felt that his factual analysis made it necessary to replace "four major postulates of received economic theory—the trading character of the market, the determinancy of cost, the unity of the process of real saving, and the efficacy of the profit motive" (p. 16). Elaborating these points, primarily in a lecture delivered in 1957, Means argues that under collective capitalism the market supplies only part of the socially necessary coordination of individual actions, as coordination also takes place within the firm, by laws and customs, and by the acceptance of common goals. Within the firm the efficacy of the profit motive as an explanation of action is doubted, with the suggestion offered "that a study of motivation in the top management of a great university would throw more light on the motivation of top corporate management than any amount of study of small private enterprises" (pp. 63-64). Means also questions whether resources are properly allocated under administered pricing—presumably because such pricing does not reflect costs, and production is not responsive to consumer wants—and pleads for the development of a theory of economic planning.

I must admit I find these criticisms less than devastating; it seems to me that, in these areas, the approximations of inherited theory serve rather well. On the other hand, most economists would, I believe, agree regarding the lack of "unity of the process of real saving"—meaning, I take it, that received theory did not adequately explain economic fluctuations. Most would also, I imagine, agree that fluctuations, as we know them, stem from the existence of less than perfect price flexibility. But inflexibility—and fluctuations—have a long history. It is not clear to me that administered pricing represents more than an increase in price rigidity, so that I am not clear that development of the concept has added much to our understanding of fluctuations.

What of public policy implications? Probably Means' most interesting contribution was the development, around 1957, of the concept of "administrative inflation," and the presentation of evidence showing that it was concentrated industry, especially steel, that was responsible for most of the price increases which took place during the recovery from the 1954 recession. This contribution was, of course, not unique; many others were at the time worried about what was more generally called the "cost push." But Means was among the first to stress that monetary policy could be used to restrain upward pressure on prices from the supply side, only at the expense of employment. As an immediate remedy for the dilemma, he advocated early in 1959 that the Federal Reserve undertake an easy-money policy and that administrative inflation be prevented by public hearings on administrative price increases and "a graduated anti-inflation . . . excess profits tax limited to the larger corporations, perhaps those with assets over $100 million" (p. 149).

To my way of thinking, the major flaw in Means' general analysis is his failure to recognize that the power to administer prices does not in itself free even large corporations from competitive market pressures. Nowhere in this collection is there recognition of the fact that, despite administered pricing, corporate profits have declined from 15 to 11 per cent of the national income over the last decade; nowhere is there recognition of recent arguments that monopoly profits are of trivial importance in the U.S. economy. Further, although the wages achieved by big unions are described as a type of administered pricing, there is no recognition that inflation, however described, can be prevented only if unions are prepared to accept wage increases that are no greater than the increase in productivity. In short, it seems to me that excessive corporate economic power is of no more than minor importance in an economy with persistent unemployment and lagging growth in a world marked by continuing Soviet expansion and an unrestrained increase in population.

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Institutional Economics: Veblen, Commons, Mitchell Reconsidered. By JOSEPH DORFMAN, C. E. AYERS, NEIL W. CHAMBERLAIN, SIMON KUZNETS, R. A. GORDON. Berkeley: University of California Press, 1962. Pp. vi, 183. $4.00.

Gathering at University of California for a series of lectures, five prominent economists paid their respects to institutional economics. This book is a documented version of their lectures.

As the title suggests the discussion centers on the contributions of Thorstein Veblen, John R. Commons, and Wesley C. Mitchell. In addition to chapters on each of these three, there is an introduction tracing the origins of institutionalism and a conclusion which purports to show institutional elements in contemporary economics.

Joseph Dorfman unravels the various intellectual threads which developed into institutionalism: "Institutionalism is a product of the last third of the nineteenth century. The economic problem of this post-Civil War period placed a heavy strain on the dominant *laissez-faire* tradition inherited from the Jacksonian era." "The fathers of institutionalism were intellectual rebels, but they were not critics from the outside with little mastery of the discipline. They were thoroughly trained in traditional economic doctrine, but were dissatisfied with its limitations and rigidities."

To C. E. Ayres falls the task of outlining the legacy of Thorstein Veblen.

Ayres explains the objections which he shares with Veblen toward traditional economics. He claims that, if Veblen were right, "virtually all other economists were quite wrong—not just a little wrong in matters of detail . . . but utterly and incorrigibly wrong." They were on the wrong track. For Veblen "industrial technology is the real substance of modern economy." "This system embraces a body of knowledge and skills and a stock of physical equipment; it also embraces a complex network of personal relations reinforced by custom, ritual, sentiment, and dogma. This concept of the economy . . . is Thorstein Veblen's principal bequest to succeeding generations."

The gem of the entire book can be found in Neil Chamberlain's lecture on John R. Commons. Although many of Commons' own students failed to understand his difficult works, Chamberlain manages to extract his meaning. He explains, "Looking at such a system—never in equilibrium, always in motion, always going somewhere—Commons sought to describe in general terms the parts of the process by which the numerous compromises which were its objectives (volitional values) took the form of transactions through the medium of its institutions, its going concerns, and its collective rules." Chamberlain also outlines Commons' extraordinary career as a reformer, teacher, and field investigator.

Simon Kuznets explains the basic views and contributions of his distinguished teacher, Wesley C. Mitchell. In Mitchell's day the "body of orthodox doctrine was not based on testable evidence, but at best, on highly plausible assumptions concerning human nature and its economic rationality under rigorously specified and oversimplified assumptions." Mitchell, through his own researches, his influence on his students, and through his direction of the National Bureau of Economic Research, did much to make economics an empirical science using quantitative and statistical methods.

In the concluding chapter, R. A. Gordon looks at institutional elements in contemporary economics. He finds some in developmental studies, labor economics, the theory of the firm, and perhaps in macrotheory. However, "orthodox theory is about as vulnerable today to Veblen's barbs as when he first hurled them more than 60 years ago." It also lacks "a grand design, an overall view which takes due account of both time and space and also concerns itself with the cumulative interaction of economic behavior and the changing institutional environment."

For most economists the significant contribution of this book will lie in its treatment of a segment of the history of economic doctrine. For those particularly interested in the three founders of institutionalism the book will also be useful, especially the section on Commons. Undoubtedly the dwindling ranks of institutionalists will take heart upon reading this book. However, this reviewer believes that institutionalists have done their important job. They forced economists within the mainstream to broaden their study. Institutionalists continue to find some approaches to economics objectionable, but today the scope of economics includes the areas which they formerly complained were neglected.

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Economic History; Economic Development; National Economies

Money, Prices and Foreign Exchange in Fourteenth-Century France. By HARRY A. MISKIMIN. New Haven and London: Yale University Press, 1963. Pp. 215, with graphs, maps and appendices. $6.75.

The sparse empirical data in medieval economic history lend themselves more easily to the broad application of economic logic and theory. And by the same token, inevitably, statistical tests become that much more difficult and elusive. The fourteenth century, which Harry Miskimin has chosen for his interesting study of money in France, is no exception. Indeed, it has a particularly critical quality. On the one hand, as the studies of Wilhelm Abel have shown, the long expansion in Europe during the twelfth and thirteenth centuries petered out in this period and turned into recession. And, on the other hand, the geographical position of France, a great agrarian country, acquired a pivotal role in the strategy of medieval international trade.

The book opens with a brief discussion of the economics of the fourteenth century, reviewing in turn the problems of population, of the expenditures and requisitions for war, of credit and the velocity of circulation. This prepares for the second chapter which sets up a model: from the Irving Fisher equation of exchange applied simultaneously to both France and England, the deduction is made that price inflation was in direct proportion to currency debasement (on the assumption both that output was confined to wheat production and that climatic conditions—and therefore the harvest yield—were identical in the two countries). This forms a leading theme of the book. From this, chapters follow on monetary conditions and coinage in France and England, price movements, and monetary depreciation. He favors the conclusions that "new contracted prices rose in proportion to debasement" (p. 82); that "variations in gold coinage had little effect upon wheat prices" (p. 99); and that there are good reasons to assume a high velocity of circulation of bullion between one country and another (p. 116). In the closing discussion, he rejects the applicability of Gresham's law; in the place of international monetary speculation, he suggests that bullion movements were a function of trade.

The weight of the analysis tends to be concerned with short-term effects, and presents the arguments clearly, with the aid of tables of data and excellent illustrations in graphs and maps. Price historians will wonder why Miskimin used "maximum" prices from a changing group of towns in preference to placing more reliance on the almost continuous series of wheat prices for Chartres (quoted every year on February 2nd). Under the circumstances, it is difficult to give definitive proof of the money illusion over such a span of time, especially when, paradoxically, the secular trend in prices turned downwards. Luigi Einaudi and François Simiand favored more complex explanations than those based on purely monetary considerations. But if this question must remain open, it must also be noted that the study by Miskimin has not been accomplished easily. It is an achievement over the documentary material, which brings credit to the school of economic history at Yale University and hope for future contributions in this difficult but fascinating field of enquiry.

FRANK SPOONER

Paris

Problems of Greek Regional Development. By BENJAMIN WARD. Research Monograph Series, No. 4. Athens: Center of Economic Research, n.d. Pp. xvi, 159. Paper, $2.50.

Professor Ward has made a valuable contribution in this book to development literature by analyzing Greek experience in terms of the critical relationship between regional and national economic development. All national development programs must necessarily be implemented on some regional basis. Given the resources available nationally for development, alternative decisions regarding locations of new investment (e.g., in existing centers, or as new growth poles for lagging areas) lead to different pay-offs for the national economy, as well as for the regions involved. If, as in the case of Greece, interregional disparities are great and the population of lower income regions constitutes a significant fraction of the total, then design of a regional development program may be the critical element in the national development program.

The first three chapters of Ward's book deal descriptively and briefly with recent Greek economic development, in order to provide necessary background to his treatment of regional problems. In Chapter 4, Ward begins his analysis by testing the hypothesis that the rank-size distribution of Greek cities can be described by the curve fitted by Pareto to personal income distribution. After testing with census data for the years 1920, 1928, 1940, 1951, and 1961, he concludes that, because of poor fit at the extremes of the distribution and for other reasons, the Pareto distribution is not applicable to the rank-size distribution of cities in Greece. He finds that all cities of over 25,000 population in Greece have tended to grow at less than the expected rate, except for the two largest, Athens and Salonica, which appear to have been absorbing population growth from cities of intermediate size. Since this pattern has been quite stable for a period of at least forty years, Ward is led to explore theoretical and institutional explanations of city growth rates in Greece. He approaches this problem of disaggregating the distribution of employment opportunities by economic sector among cities of different size, in an effort to reveal how the aggregate distribution has been generated. He finds that the city-size employment data disaggregated by economic sector also deviate from the Pareto distribution and give clear evidence of dominance by Athens and Salonica.

In an effort to explain this disproportionate growth of central cities, a widespread phenomenon, Ward examines a number of institutional and theoretical factors, including historical momentum, superior transport facilities, stable percentage growth rates, economies of scale, cumulative advantages in productivity, labor skills and diversification, bureaucratic centralization, and the importance to business success of access to political influence.

In Chapter 5, which is one of the most stimulating sections of the book, Ward examines several methods of regional analysis in relation to policy formation. First, he discusses regional and national development aims, their relationships, and possible conflicts. In order to analyze the situation of possible conflict between regional and national objectives, he develops a two-region income and investment model that can be used to explore the relative effectiveness of government expenditures in each region with respect to its income impact in each region, and upon the national economy. One of the interesting properties of this model is that, depending upon the relationships involved, it may be possible to generate as much income in less developed regions from government expenditures in more developed regions as from expenditures in the less developed regions themselves. This section merits considerable attention and expansion.

Ward next devotes extensive effort to explanation and application of location coefficients and other aggregative indices of regional dispersion. While his discussion of these measures is of interest, their operational significance for analysis and policy guidance is much less apparent than is that of alternative techniques not examined, such as interindustry models, benefit-cost studies, and industrial complex analysis.

The final chapter, which explores some aspects of the administration of regional planning, should be of interest primarily to political scientists and administrators. In his discussion of the pilot-demonstration area program of community improvement in Epirus, a description of the assistance role played by the Organization for Economic Cooperation and Development (then the Organization for European Economic Cooperation) would have been of interest, as would an examination of the general role of technical assistance and pilot areas in regional economic development programs.

Publication in a language obviously not native to the printers has resulted in numerous typographical and other errors. The useful bibliography might well have been supplemented by inclusion of an index, and a simple map depicting the regions and cities referred to would have been most helpful.

Although Ward has not attempted to produce a fully comprehensive account of Greek economic development, nor to deal definitively with its regional dimensions, he has examined some significant aspects of these problems, and his analysis will be of interest to those concerned with theory and practice of national and regional economic development.

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Material'noie Stimulirovanie Trudiashchichsia v SSSR. (Material Incentives for Workers in the USSR.) By F. S. Veselkov. Moscow: Gospolitizdat, 1962. Pp. 239.

This book is described by the publisher as "the first attempt in Soviet economic literature to review the entire complex of material incentives in the USSR." It not only discusses wage incentives for production workers in industry and related economic stimuli in agriculture, but methods of rewarding workers for making discoveries, inventions, and suggestions for rationalizing production, and ways of penalizing workers for poor workmanship and breaches of labor discipline. Most interesting of all, it includes profits of individual firms as a form of material encouragement and concludes with an analysis of manipulations of the price system to goad the population to work harder and better.

The almost inevitable effect upon the reader is to substantiate his growing conviction that the Soviet economy is not moving toward the communist ideal of distribution according to needs, as claimed by Soviet leaders. They may call their system of incentives "socialist" if they like, but so far as labor payments of all kinds are concerned it is also capitalistic.

Technological progress being a main goal of Soviet policy, the author summarizes a somewhat novel accounting scheme for encouraging improvements through the planned price mechanism. Development funds are created by economic councils, ministries, and government departments. Machine-building and metalworking firms draw upon these funds to finance research and development of new products. They deduct the total of these costs from their costs of production, so that when the new products are manufactured they can either be sold for lower prices than would be necessary otherwise, or the firms making them will make more money.

The provisional wholesale prices on the new products are set in conformity with the prices of similar existing articles. Despite the fact that the improved machines cost more to produce, the prices at which they are sold to firms making consumer goods and other final products are no higher than the prices previously paid for existing machinery. However, if the new machinery raises the productivity of labor, improves quality, cheapens production, or lightens the burden of workers, the price may be raised.

The book also discusses the use of different levels of wholesale prices for ready-made consumer goods to encourage improvements in quality. Goods are divided into grades and higher prices are paid for better quality articles. Prices may be reduced on low-quality grades to discourage their production. Price differentials are likewise employed now to encourage improvements in design, new models, and fashions.

In food production, prices are varied to encourage the growing of vegetables in off seasons, with higher prices for vegetables and fruits produced in the winter and spring. In building construction higher prices are charged for wood and lower prices for reinforced concrete to encourage the introduction into use of the new material that is considered more "progressive." Now that fuel oil can be produced more cheaply than coal, the price relationship is shifted to encourage use of oil. Prices on materials in short supply, like nonferrous metals, are raised in relation to those for steel.

In many of these cases the free market mechanism would accomplish the same results, of course, but that would be "unplanned" and therefore "anarchic" from the Soviet point of view.

In a few places the author's explanation is not detailed enough to make the system of material incentives fully clear, especially in the section described above with relation to the cost accounting and pricing of new products, but in general the treatment is lucid and constitutes a good over-all summary of the situation early in 1962 when the work was prepared.

ROLAND GIBSON

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Economic Development—Objectives and Methods. By PAUL ALPERT. New York: Free Press of Glencoe; London: Collier-Macmillan, 1963. Pp. x, 308. $6.95.

This book adds to the already considerable choice among introductions to the subject of economic development. While it requires a modest background in economics of the reader, it is generally written at a level which should be understandable to a considerably broader and more heterogeneous audience than that of professional economists. The book does not attempt to elaborate or detail operationally the techniques for actually carrying out specific programs to achieve economic development.

After a brief introduction that includes the inevitable definitions, prerequisites, and purposes of economic development, there is a brief résumé of the course of development in Western Europe, Japan, and the Soviet Union. These serve as a background for an elaboration of the conditions of the currently underdeveloped countries. While this method of presentation has been used frequently by others, it has seldom been carried out more effectively. Professor Alpert then addresses himself to what is termed the basic dilemma. Surprisingly this turns out to be the determination of the respective functions of government and private enterprise in the development process. While the discussion is couched in these terms, however, the criteria for determining the functions of government are made to turn in substantial measure upon the effects upon the process of allocation of resources, investment criteria, the mobilization of resources, as well as the goals of development. Thus, as Alpert presents the matter, the decision concerning the role of government in development implies some considerable attention to more strictly economic problems than may at first seem to be the case.

The first section of the book concludes with a treatment of the problems associated with planning. These problems are set forth clearly, and the concepts of certain operational techniques are presented, although they are properly, for a book at this level, not developed to include the procedural apparatus for problem solving.

The two remaining sections consider, in turn, domestic problems of development and international aspects of the development process. The section on domestic problems generally makes good reading. It is informative, replete with illustrations, and introduces a number of concepts which have proven useful in analyzing the underdeveloped economy. The final chapter of the section is devoted to the question of priorities, and the author effectively uses these analytical concepts to pull together the earlier descriptive material in the section.

There are, however, some drawbacks to this portion of the book. This is perhaps due to the fact that Alpert has couched this section in terms of problems. The reviewer feels that the theoretical foundations are insufficient to enable the reader to understand the nature of the problems raised. Others may feel less strongly than I do about this point. Nevertheless, there is not sufficient emphasis on the structural adjustments that take place in consumption, production, and income distribution. The consequent stresses and pressures that result cannot be isolated and considered effectively. In addition, the role of market expansion and specialization are slighted.

The chain of reactions to innovations and the inducements created through certain imbalances are discussed briefly in the final chapter of this section. This chapter (on priorities) could have followed the final one (on planning) in the first section of the book to good advantage.

In the final section Alpert argues that "economic development is essentially a world phenomenon." What the book attempts to convey here is the necessity for international cooperation between the industrialized and the underdeveloped countries. The desirability of such an approach to development need not be questioned, but Alpert's case might have been strengthened if he had not placed the example of the Soviet Union before us.

A more important criticism of Alpert's treatment of the international aspects of development, however, may again be made on grounds of insufficient theoretical underpinning for the reader. The basic theory of international trade is not developed fully enough to convey to the reader the possible potential value of international trade to the developing country. Instead, the reader is plunged immediately into the question of the impact of development of underdeveloped countries on industrialized countries—a question which is not resolved in an altogether satisfactory manner. Alpert next moves to world price stabilization and to some questions and problems concerning foreign capital investment. Without an appreciation of the gains from international trade, the difficulties involved must appear scarcely to be worth the candle. The only advantage to underdeveloped countries seems to be the foreign capital investment and various other forms of foreign economic aid. The advantage to developed countries is left in an even more tenuous state.

Despite these not inconsiderable reservations, it is my opinion that the book may serve to introduce the problems of economic development which beset the underdeveloped countries. It is a reasonably well-balanced book and, except for the last two chapters, which become mired in details of the organizational structure of various agencies for international assistance, it is interestingly written.

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Statistical Methods; Econometrics; Social Accounting

The National Wealth of the United States in the Postwar Period. By RAYMOND W. GOLDSMITH. Princeton: Princeton University Press, for National Bureau of Economic Research, 1962. Pp. xxix, 434. $12.50.

As Raymond Goldsmith succinctly asserts, no system of national accounts can be regarded as complete without balance sheets for the nation and for the main sectors distinguished in the accounts. Yet, to date there are no official estimates of balance sheets for the United States, and indeed few estimates of stocks of tangible assets. The National Bureau of Economic Research, and Raymond Goldsmith in particular, are to be congratulated for providing the only comprehensive estimates of national wealth and balance sheets that are available.

This volume provides the reader with 110 pages of text and a set of appendix tables of some 320 pages. For the most part, the text is merely a summary and discussion of the data and methods of estimation. The tables themselves constitute the heart of the book, and these will be used by research workers for many different purposes.

National wealth consists of reproducible and nonreproducible assets. The estimates for reproducible assets follow the perpetual inventory method, by which the stock of a given category of assets is derived as the cumulation of past expenditures on that category, in current or constant prices, depreciated in accordance with the average length of life of the asset. Nonreproducible assets are estimated independently on the basis of census-type data or application of ratios of land value to structural value. Goldsmith provides the national wealth estimates on a net (depreciated) and a gross (undepreciated) basis, and on two valuation bases: constant (1947-49) prices and current (replacement value)—and occasionally also on the third basis of original cost. The scope of the estimates is quite broad and covers categories not always contained in estimates of national wealth. For example, Goldmith includes estimates of consumer durables, government civilian structures, military equipment, forests, and subsoil minerals.

One cannot help being struck with the similarity between the use of national wealth as an indicator of welfare and early discussions of national income in this connection. It is difficult to appraise the meaningfulness of a 2 per cent annual rate of increase in per capita income. Perhaps comparisons between countries, or even between periods of time, may have some meaning, but this reviewer believes that such exercises are indeed futile. The real value of the study of national wealth and balance sheets, as in the case of national income accounts, lies in the structure of assets held by different sectors and the changes in this structure over time. Thus it is of interest to find that the share of equipment in total assets has been steadily increasing while the relative importance of inventories has been declining. Similarly, it is also interesting to note that, even though the share of consumer durables rose from 19 per cent in 1945 to 22 per cent in 1958, this movement failed to bring the share back to the level of 1929.

In summary, this book is a valuable source book for economists who are interested in macroeconomic analysis. It should be considered to be an intermediate product—a basic material intended as input for further economic research. In terms of the advancement of the field of national wealth and balance sheets, it is a major pioneering effort. After the blueprints provided by the National Bureau and Raymond Goldsmith, it is to be hoped that the government can be convinced of the feasibility and usefulness of developing official estimates.

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Economic Systems; Planning and Reform; Cooperation

The Contrasting Economies—A Study of Modern Economic Systems. By LYNN TURGEON. Boston: Allyn and Bacon, 1963. Pp. ix, 382. Paper, $4.25.

The study of comparative economic systems is a subject which, like many others in economics, lends itself to controversy—only more so. Lynn Turgeon's *The Contrasting Economies,* designed to be a textbook for such a course, is admirably suited to such controversy.

There are two criteria by which to evaluate Turgeon's book: as a text for comparative economic systems and as a contribution to economic study. It might seem impossible to rate it high on the first count if it leaves something to be desired on the second, but Turgeon's organizational framework in the field of comparative economics is a needed one. Rather than examine a series of separate economies one after the other, he attempts to discuss eight different economic phenomena from investment to international trade and see wherein there are similarities in the various countries under study. This allows him to set out the general characteristics and functions of a field like labor, for example, and then examine how the different economic systems compare with this norm. This not only informs the student about economic life in a particular country, it also provides an insight into the nature of the economic activity itself. It requires a good deal of skill to operate within this framework, but it is one which has been unavailable. The organization of the labor and price chapter is especially well handled. Unfortunately, Turgeon does not succeed in developing this format quite as satisfactorily in his other chapters.

One other comment should be made about this work as a text for comparative economic systems. Turgeon divides his economic systems into only two categories: the capitalist and the noncapitalist. This inevitably breaks down into the United States and the USSR, with at best two paragraphs per chapter devoted to additional countries. Among others, this omits from separate consideration fascism, the period of nationalization in England, the revisions in Yugoslavia, and the problems of developing countries. In an arbitrary fashion, he places India in the noncapitalist division despite the fact that the private sector is much larger than the public. While the size of the private sector is partly explained by the role of agriculture, it does seem an uncomfortable definition. Nevertheless, despite some shortcomings, Turgeon's effort to provide a new approach in a "systems" textbook should be applauded.

It is as an economic study, however, that there are serious questions. Of course, some of Turgeon's remarks are very perceptive. For those who complain about the statistical juggling in the USSR he shows how U.S. organizations will sometimes do the same thing. When steel production started to fall, the American Iron and Steel Institute abolished the concept of production as a per cent of capacity. Another example of his perception is his recognition of the fact that 1961 ruble revaluation was actually a devaluation from the official rate, not an appreciation, as the Russians claim. He also manages to incorporate the organizational changes in the Soviet economic structure as of January 1963, although as others have found, this is a never-ending task since there have been new changes since then.

The criticisms of this book arise not from any major errors, but from a host of small issues. In making his comparisons, Turgeon often leans over backward or is inconsistent or takes matters out of context. He says many things that are not worth quibbling about in themselves, but because he does not always apply the same standard to both countries, he ends up with a far more negative picture of the United States than he probably intended to create.

Because of space limitations, only a few of the many questionable comparisons can be cited. For example, on p. 167, he relies on and cites a *Soviet source* to show that U.S. pensioners receive 20 per cent of their preretirement wage, while the Russians receive 60 per cent. It is an understatement to say that it is not always wise to base such conclusions solely on Soviet statistics. The Soviet figure is partly explained (as Turgeon notes earlier) by the fact that the preretirement wage in the USSR is lower to start with, and that Russian peasants (about one-half of the population) normally do not receive pensions. Recourse to U.S. statistics would have shown that the U.S. pensions are closer to 30 to 40 per cent of preretirement income if the total pension is calculated (government and private). Furthermore, by assuming the Soviet family is larger than it actually is, he appears to exaggerate the "representative" family income in the Soviet Union by about 300 rubles (almost $300) a year (p. 110). Similarly, any reference to income disparities in the two countries should include a reference to the collective farm peasants who are in even worse financial straits than our rural poor.

Turgeon is not always consistent in his comparisons. He correctly points out that U.S. unions are difficult to deal with and are an obstacle to automation primarily because they actively reflect the feelings of their members. He notes that Russian unions are much less of an obstacle to automation but he fails to stress that Soviet trade unions are less effective instruments of their members, especially with respect to pay and layoffs. In Chapter 10 Turgeon leaves the impression that inequality of income is bad in the United States but permissible in the USSR because there it is needed "to stimulate productivity." It may be obvious, but he feels no need to make the same statement when noting the inequality of income in the United States.

At times Turgeon seems to suggest some bewildering conclusions. By making an irrelevant analogy to an efficiently run Soviet medical institute, he creates the impression that Soviet industry is less bureaucratic than U.S. industry (p. 225). In another instance, he gives the incorrect impression that there has been no distortion in Chinese statistics (p. 27). Soviet exploitation of the brother satellites through the joint stock companies is written off as a just punishment for attacks on the USSR during the war. As he himself explains, however, reparations were supposed to have satisfied such claims (p. 299). Furthermore, while it might be fair to blame "the increased defense appropriations of the Kennedy Administration" for the postponement of the "present withering away of the Soviet state," Turgeon incorrectly leaves the impression that this is also an excuse for the increased centralization of industrial management (p. 328).

In this conclusion Turgeon has a good deal to say about consumption. He asserts that "logically" the Russians should never have the problem of inadequate demand, and thus "theoretically" they should have no need to waste their resources on advertising, installment credit, market research, product obsolescence, output restrictions, and exportation of surplus consumer goods (p. 329). As he acknowledges earlier in his book, however, they have done all of these things. (Some Soviet marketing officials have even expressed a strange curiosity about trading stamps.) Moreover, these activities are no longer just frills; they are fulfilling important economic functions, and most Soviet economists expect these activities to grow as consumer goods production increases and varying price and income elasticities begin to have more of an impact.

It is true that the Soviet rate of growth is faster than our own, that the Russians have achieved some notable economic successes, and that there are serious wastes (unemployment, cigarette advertising, etc.) in our own economy. Yet while balance in a picture of economic systems is indeed necessary, so is focus.

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Ekonomicheskie zakonomernosti perehoda ot sotsializma k kommunizmu (Economic Laws of Transition from Socialism to Communism.) Edited by N. V. BAUTINA. Moscow: Ekonomizdat, 1962. Pp. 293.

Among the statements the Soviet leaders have made at the twenty-second congress of the Communist Party of the Soviet Union, few were given as much publicity as those that announced that the Soviet Union was entering the "period of full-scale Communist construction." This momentous news to those brought up in the Marxist-Leninist tradition inspired a spate of books, of which Bautina's is fairly representative, on the economic aspects of the transition from socialism to communism.

Like most other Soviet works of this nature, the one under review is a collective effort. The contributors, ten in number, are all economists with academic qualifications. The literature they cite consists of the works of Marx, Engels, Lenin, and Khrushchev, some recent monographs by Soviet economists, and articles in Soviet newspapers and learned journals. A place of honor is given to the *Documents* of the twenty-second congress of the C.P.S.U., a gathering that is described in the opening sentence of the book as "a gigantic landmark in the development of world history." The statistical data which abound in this volume are used on more than one occasion to show how well the USSR compares with czarist Russia circa 1913 and the rate at which the socialist world is catching up on the capitalist.

Confidence in the inevitability of communism coupled with a strong emphasis on the role of certain prerequisites for a successful transition from socialism to communism permeate the entire book. The prerequisites receive more attention in the chapters that discuss the need to create a strong material and technical base, increase public property, raise labor productivity, and overtake the most developed capitalist societies in production per capita. The remainder of the book deals with the role of the state machine, agriculture, and trade in the period of transition, "the distribution of material and spiritual wealth," and some problems of the "world socialist economy."

Almost invariably the contributors are simply content to elaborate Krushchev's views, which are now part and parcel of the Communist doctrine. Polemics are few and far between. Only three Western students of the Soviet economy are mentioned; two of them are accused of "slanders," while the third one is quoted with approval for what is considered as a sympathetic reference to the USSR in his book. As has been the practice in the Soviet Union for several years, Stalin is condemned for some of his statements in his last theoretical work, *Economic Problems of Socialism in the U.S.S.R.* Several Soviet economists are criticized for not being sufficiently precise when describing some contemporary developments and for using certain terms liable to misrepresentation. Not that Editor Bautina and her collaborators are models of clarity. Their failure to provide a readable, let alone a thorough analysis of what is really involved in building a communist society, seriously detracts from the value of this book. Their contributions serve to confirm the melancholy fact that Soviet economists in general are still expected to provide a scholarly coating for the relatively crude and imprecise directives and slogans of the Agitation-Propaganda Department of the central committee of the C.P.S.U.

IVAN AVAKUMOVIC

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Effektivnost' kapital'nykh vlozhenii v sotsialisticheskom obshchestve—Voprosy teorii. (Efficiency of Capital Investments in the Socialist Society—Problems of Theory.) By B. A. VOROTILOV. Leningrad: Publishing House of the University of Leningrad, 1961. Pp. 161.

This is a concise and workmanlike survey of the voluminous Soviet literature on the subject of criteria for investment choice. The concluding chapter, which deals with the ways of further improvements in the efficiency of investment, has interesting illustrative examples taken from actual experience of enterprises of the Leningrad region.

ALEXANDER ERLICH

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Bidrag till teorin för verkningarna av räntevariationer. (A Contribution to the Theory of the Effects of Changes in Interest Rates.) Lund Economic Studies, No. 1. By GUY ARVIDSSON. Lund: C. W. K. Gleerup, 1962. Pp. 67. SKr. 5, (95¢).

According to Professor Arvidsson's preface, this stimulating essay in capital theory, originally written in 1954, is a part of a more comprehensive work which he is revising for publication in English. If this essay is representative of the larger treatise, there are good grounds for welcoming its eventual arrival, for in this study Arvidsson makes some notable contributions to the challenging Wicksell-Lindahl formulations in capital theory.

Arvidsson deals with a venerable problem, namely the most probable effects, linking an initial with a subsequent equilibrium situation separated by a transition interval, of a change (chiefly a *reduction*) in the interest rate (1) on output as such, (2) on the composition of output (production of capital versus consumption goods), (3) on the structure of production (capital deepening or lengthening of the "period of production"), (4) on real income streams, particularly on real wages, (5) on employment, and (6) on the price level. The traditional view is that, if full employment prevails in the initial situation, a reduction in the interest rate induces additional capital formation, generally at the expense of some reduction in consumption-goods output during the transition interval, as more workers are transferred at rising real wages into production of capital goods. In the new equilibrium it is generally conceded that total output will have increased, capital deepening will have occurred, real wages will be higher and the interest rate and property incomes in general lower than before.

In his first chapter, devoted to comparative static analysis Arvidsson, using a very restrictive framework of reasoning (a closed, one-commodity, one-factor economy operating at full employment on linear and homogeneous production functions under perfect competition, pp. 9-14) scrutinizes this sequence carefully. He finds grounds for qualifying it in more than one particular by showing more clearly and conclusively than has been done before the necessary and sufficient conditions under which it holds. It is particularly interesting to see how slightly his findings are modified when he relaxes the restrictive assumptions (pp. 18-19).

It was with reference to the transition interval between equilibria in this sequence that Wicksell arrived at an enigmatic conclusion, nowadays referred to as the "Wicksell effect." This was the observation that as wages rise in this process of capital expansion some of the savings intended for capital formation are "absorbed" in rising wages. As a result the social marginal productivity rate of capital (the quotient of the output increment yielded by this investment process and the savings used up in this sequence) is always lower than the interest rate in the new equilibrium (the quotient of the output increment and the new real capital created in this process). Arvidsson has made a distinct contribution by clearing up this enigma and showing Wicksell's observation to be valid as a limiting case. Moreover Arvidsson goes on to generalize Wicksell's analysis, a matter which should be of interest to all serious students of capital theory, and not least to Professor Metzler, whose work on this problem is also scrutinized (pp. 19, 23-26).

Arvidsson's second chapter (p. 27, ff.), the lion's share and the most suggestive part of his essay, pursues the problem of the impact of a change in interest rate as a dynamic process analysis. In this part of his study he draws more on the heritage of Lindahl than on that of Wicksell, but he soon leaves doctrinal history behind in order to extend his approach to the contemporary scene.

Again, in order to argue from "strong cases," Arvidsson revives several of the earlier restrictive assumptions and considers particularly the impact of interest changes on the distribution of income and wealth first under conditions of constant inventories and unchanged production structure. As he relaxes these conditions, he extends his observations to the short- and medium-run effects observable under perfect and imperfect competition, and for an open as contrasted with a closed economy. The models he develops are deliberately truncated in the interest of simplicity. Yet he is able to apply them with illuminating effect to certain phenomena observable in the housing sector of the economy, where interest rate changes have presumably a greater and more readily discernible impact than elsewhere. Finally, he considers the role of interest as a cost within the framework of the "full cost" theory of pricing.

All told, this essay indicates that Arvidsson has most of the building blocks at hand for issuing in the larger work to be published in English a comprehensive treatise on the effects of interest-rate changes, and such a treatise should be a significant and welcome contribution to contemporary capital theory.

CARL G. UHR

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Renteverschillen en renteniveau—een onderzoek naar de oorzaken van de rendements verschillen tussen staatsobligaties en, daarmede samenhangend, van het renteniveau, over de periode 1949 t/m 1960 in Nederland. By THEODORUS ALOYSIUS STEVERS Rotterdam: Nijgh en Van Ditmar, 1962. Pp. vi, 199. *f.* 17.50.

In his dissertation on *Interest Differentials and Interest Level,* submitted to the Catholic Economics Institute at Tilburg, Dr. Stevers departs from the usual focus of attention on hypothetical long-term and short-term securities and works, instead, with actual Netherlands government issues outstanding over the entire period 1949-1960. A priori reasoning and casual empiricism dismiss as unimportant most of some 16 characteristics that might conceivably account for differences in their effective yields. Besides time remaining to maturity, though, the nominal coupon rate and call provisions do prove worthy of continued attention. After also presenting a formidable list of "aspects" through which these objective differences between issues should influence their yields, Stevers finds only four really important. The speculation aspect concerns not only interest-rate expectations but also the size of the change in a bond's market price that would correspond to a given change in its effective yield. The liquidity aspect also concerns this price variability (considered, however, without definite expectations about the direction of change). The investment aspect concerns how attractive its maturity makes a bond in view of the relation between current and expected future yields. The conversion aspect concerns the size and likelihood of loss from exercise of any call provisions (something much emphasized throughout the book).

Attention to these aspects governs how Stevers combines the independent variables of an explanatory equation in—or hides them behind—several differences and ratios and other derived variables. The dependent variable is the relative spread between the yield on an actual bond issue and an indicator of the general interest-rate level, which Stevers computes by averaging the yields on a few issues chosen and weighted so as to form a "representative" bond with about 15 years to run. (The theoretical differential between yields on two actual bonds may be found by first separately computing their respective deviations from the "representative" yield.) The underlying independent variables are remaining time to maturity, a parameter related to the nominal coupon rate, the current representative yield as just defined, and a modified average of the latter over the preceding 16 quarters (with the relation between these last two computed figures supposedly indicating interest-rate expectations). Stevers fits an equation of this type to time-series data for each of seven actual bond issues and obtains what he considers satisfactorily close correlations. He explains much of what discrepancies there are between computed and observed yields by referring to otherwise neglected minor characteristics of the bond in question, such as usability in paying certain taxes, possibility of government support purchases, call features not already taken into account, and the saving in reinvestment expenses that a very long bond or a perpetuity offers.

Stevers' theory accounts for certain other phenomena as well, such as that the effective yields on different issues sometimes move in opposite directions over rather long time spans. One thing it does not do, however, is define any equilibrium sizes of yield differentials between bonds of different maturity; the theoretical differentials can only be computed from equations containing empirically derived parameters.

From Stevers' perhaps unnecessarily strung-out parade of characteristics and aspects one may extract some useful insights and some ideas for modifying the exposition of rate-structure theory. His contribution to the pot of statistical evidence is also welcome, though one wonders whether some more straightforward regression equation than his might not have fitted the data equally well.

A less favorable verdict applies to Stevers' claim that his theory of differentials and especially his representative-yield concept contribute to understanding the general interest-rate *level*. He offers a liquidity-preference theory with unessential modifications. The independent variables in his regression equation are the ratio of an adjusted stock of money and near-moneys to national income, an indicator of business investment activity, and the average representative yield over the preceding 16 quarters. To explain a variable largely in terms of its recent level is hardly profound.

LELAND B. YEAGER

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Zuiver Uw Zilver. By PIETER LIEFTINCK. The Hague: Martinus Nijhoff, 1962. Pp. xi, 238. *f* 17.50.

The author of ***Purify Your Silver*** (the title is a play on words alluding to monetary reform) was a professor at Rotterdam before the war, was Netherlands Finance Minister during the first seven postwar years, and is currently an Executive Director of the World Bank and IMF. The book brings together a number of his speeches and articles dating from 1935 to 1961.

Chapters 1, 2, and 10 offer generalities about monetary and economic policy. Chapter 11, written in English and the same as No. 39 of the Princeton international finance essays, broadly surveys international financial problems and proposals, including the U.S. balance of payments and the Triffin Plan. Chapters 4 through 9 consist of excerpts from parliamentary debates on a tax on wartime increments in wealth, a capital levy, compensation for wartime property destruction, nationalization of the Netherlands Bank, tightening of control over the private banking system, and the final stages of the early-postwar monetary reform. The debates over the one-shot tax and compensation measures raise intriguing issues of value judgments, ethics, and natural law. How are distinctions to be drawn between proper and improper wartime gains? What is the fair way to redistribute the burdens of wartime losses? Are Netherlanders in a situation analogous more to that of a town struck by a natural disaster or to that of owners of cargo that had to be partly jettisoned to save the ship?

To an uninformed foreigner (like this reviewer), Lieftinck's parliamentary speeches are frustrating because they lack the compactness and the organization of systematic essays on their topics and because they convey only tantalizing one-sided echoes of what must have been lively controversies. Chapter 3 is more satisfactory in these respects because it consists of five radio addresses delivered in July through December 1945 to a nonspecialist audience, namely, the whole Dutch population. Lieftinck explains the anti-inflationary purpose of the reform to mop up the excess money supply. He refers repeatedly, though only implicitly, to the real-balance effect; for example, the reduction in cash balances was expected to—and as things turned out, apparently did—stimulate the supply of labor. He gives even more emphasis to depriving collaborators, black-marketeers, and tax-evaders of their ill-gotten "contaminated money." He threatens unpleasant surprises for people who think they have been clever enough to circumvent the reform measures. Big Brother is watching you, he intimates, in part through use of informers. All this holds a certain morbid fascination for the reader, as do references to the administrative difficulties of blocking the existing money supply and replacing the old paper currency with a new issue and as do references to hardship cases. Also of interest are samples of extreme reliance on the open-mouth technique of economic policy. Lieftinck repeatedly admonishes: Save. Stay out of the black market. Don't hoard small change. Don't hold more currency than you really need. Get accustomed to the giro system of payments but (after a certain date) don't overburden the system with payments of under 25 guilders. Don't regard giro funds as worth less than paper money. Help out your decent neighbors who innocently just happened to have most of their money in forms now blocked. Pay your bills faster than ordinarily to those small businessmen who are hard up for usable funds.

One of the first reform measures was invalidation of all 100-guilder banknotes, which could only be deposited in blocked accounts. Yet the government had previously promised that it would *not* discriminate among denominations in its treatment of paper money. Lieftinck's attempt to maintain that this promise was not being broken is hardly convincing.

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Monetary Policy and the Financial System. By PAUL M. HORVITZ. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. xx, 492. $8.25.

The author's text is designed for a one-semester course in money and banking. As the title indicates, the book places considerable emphasis on the structure and operations of the financial system, and this material occupies approximately one-half of the text and is followed by a section on monetary theory, which occupies another fifth of the text. The remainder of the work, slightly less than a third of the total, is devoted to a discussion of U.S. monetary policy.

Although the text is designed for a single semester, it contains as broad a survey of monetary problems as can be found in a text for a one-year course. The description of the financial system is particularly comprehensive and, in accordance with recent developments, it deals extensively with the role and function of nonbank financial intermediaries, as well as presenting some of the basic problems faced by managers of financial institutions. The extensive use of balance sheets in the description of the financial institutions gives the student a useful approach to the analysis of financial institutions.

The theoretical material presented is self-contained and assumes little or no prior economic training on the part of the reader. This means that the book may also be used in a course directed toward students not majoring in economics. The level of monetary theory goes only a little beyond that of the one-year principles course and, for courses where it may be assumed that a majority of the students have completed the principles course, it will be necessary to supplement the theoretical portions of the text with additional readings.

The concluding sections dealing with monetary policy are especially comprehensive for a book at this level and touch upon most of the major problems confronting the U.S. monetary system. Many of the discussions of controversial issues are centered about the recommendations of the Commission on Money and Credit. The policy discussions are topical and include an extensive review of Federal Reserve policy down to 1962.

The extensive treatment of U.S. monetary policy is clearly written and well balanced. However, in the opinion of this reviewer it will be difficult for the student to form his own judgment on the monetary policy of the Federal Reserve System and the opinions expressed in this volume solely on the basis of the theoretical apparatus presented in the book.

Horvitz devotes an entire chapter to the often-neglected subject of competition in banking. Although this section contains much original material, it does not show the relationship between banking competition and the effectiveness of monetary policy but rather concentrates on the adequacy and cost of banking services rendered to the public.

The author does not shy away from controversial issues and offers many opinions of the Commission on Money and Credit, of the economics profession in general, as well as his own, on policy questions, not only with respect to their technical desirability but also with reference to their political acceptability to Congress and the general community. While this reviewer heartily endorses the treatment of controversial issues, the political judgments, however, run the risk of becoming outdated rapidly as economic and political conditions change and, almost needless to say, the reader will not be in agreement with every one of the opinions expressed.

This book provides a well-balanced, policy-oriented text for a one-semester course in money and banking, especially for the student not specializing in economics who may wish to obtain a systematic introduction to current U.S. monetary problems.

RICHARD S. THORN

Committee of Nine
Alliance for Progress

Public Finance; Fiscal Policy

Measures of State and Local Fiscal Capacity and Tax Effort. A Staff Report, The Advisory Commission on Intergovernmental Relations. Washington: Superintendent of Documents, 1962. Pp. x, 150. $1.00.

This represents a valuable statistical exercise in a long-neglected but increasingly important field. It is an informational report, covering an exploratory study undertaken for the Advisory Commission on Intergovernmental Relations by its staff; it contains no recommendations or policy conclusions. The directive was to investigate measures which, for the purpose of formulating intergovernmental tax and grant-in-aid policies, might achieve a more accurate and equitable comparison between states than the measure conventionally used, namely, per capita personal income in each state.

It should be noted that this report is for the expert. The complex concepts involved defy a simple, concise and trenchant presentation of findings; the casual reader would have difficulty understanding some of the points made. In addition, a large part of the report necessarily is devoted to a detailed explanation of the wide variety of statistical sources and techniques employed. Despite these handicaps, this compendium, with its 51 statistical tables, constitutes a mine of information and throws much light on many areas of state and local government taxation.

"Fiscal capacity" as a concept is often taken to mean the potential amount of taxes which can be raised from the economic resources available to a taxing body. However, in line with the Commission's objective, the term in this study relates not to absolute capacity but to the comparative ability of the states (including their local governments) to raise funds through taxation. The authors, Selma J. Mushkin and Alice M. Rivlin, have developed two new measures of fiscal capacity—one based on income flows and the other on assessable tax bases. The study goes on to relate actual tax collections in each state to the various measures of fiscal capacity to show the extent to which fiscal capacity is actually used, and thus to compare "tax effort."

The new income measure of fiscal capacity developed in the study is a composite of several of the various available statistical series on income; it is intended to reflect the income base at the successive stages available for taxation in a state more closely than does any individual series. The assumptions are made that personal direct taxes (including taxes on residential real estate) are paid out of personal income, that business taxes are paid out of income produced, and that corporate income taxes are paid out of corporate net income. The proportion that each of these three types of taxes represents of total state and local government taxes for the country as a whole is used to weight the income series upon which the incidence of each type of tax is presumed to fall.

The authors, however, de-emphasize their composite income approach by pointing out that, since indirect business taxes represent such a large share of total state and local government taxes, comparisons obtained from their composite index of income do not differ greatly from those derived from the series on income produced. This does leave the question of whether additional research on methods of weighting the various income series might have furthered the Commission's objective.

The authors then direct their inquiry to the state and local government tax structure itself, thereby bringing to bear the wealth of information that makes the study rewarding. They have constructed a second measure of fiscal capacity which, for want of a better name, they call the "representative tax system." This hypothetical uniform tax structure shows the amount of each state's taxes under a uniform system of taxing assessable tax bases, both the rates of tax and the assessable tax bases chosen being designed to represent average tax practice across the country. In effect, the total of each major type of state and local government tax currently collected is reallocated among the states.

As the authors recognize, the question of a uniform tax system is highly controversial; their objective in devising such a system was merely to compare potentials under current average tax practice (fiscal capacity) and to measure these potentials against actual collections (tax effort). However, since the calculations involved obviously necessitate many arbitrary assumptions, any one of which can open the results to attack, it appears doubtful that such a system may realistically be expected to become a workable guidepost for decisions of public policy.

Consequently, after analyzing the results obtained from their new assessable tax base measure, the authors themselves champion it only cautiously, warning the reader that "conclusions about the relative capacity and effort position of a number of states are strongly dependent upon which index is used." They point out that, because of some weakness in the underlying statistical data, the indices that they have introduced are not sufficiently firm to serve immediate policy ends. Nevertheless, their final conclusion that indices can be constructed which would materially aid in the formulation of public policy appears to be validated by their study.

Sally S. Ronk

Bankers Trust Company

Den offentliga sektorns expansion. By Erik Höök. Uppsala: Almqvist & Wiksell, 1962. Pp. 572. SKr. 45.

The Expansion of the Public Sector, to give Erik Höök's study its English title, was issued for the Industries Utredningsinstitut (the Industrial Institute for Economic and Social Research, Stockholm). It covers the development of public civilian expenditures in Sweden during the years 1913 to 1958. Not all governmental and municipal outlays have been included, however; those activities of a business nature are excluded, such as public utilities, communications, transport, etc.

Höök's most difficult problem, and one which he discusses at great length, is the measurement of productivity changes in government services. We know that during the period covered the private sector, particularly in the production of material goods, increased its productivity greatly (and measurably). This increase, in turn, helped to raise real income, both total and per capita. The rise in real income in turn increased the demand for government services—in effect, this category is income-elastic. If the productivity of such services, however, has been rising slowly, or even not at all, then the effect of this rising demand would be reflected in some sort of price inflation of government services, so that much of the rise in productivity in the private sector accrues as gains to the public sector. The rise in government services is thus exaggerated by the inflation, and, since we lack a physical or volume indicator, we cannot correct for the extent of this component.

Illustrations of the difficulties involved are numerous. For example, if we now have college graduates on the police force, where before we had been satisfied with high school students, the higher education of the average member will be reflected in an increased outlay for salaries. Is the result a more expensive man now doing the same job or a more productive man doing a better job, thus returning to society a greater service for its greater cost? Or again, is the increasing cost of prisoner care a reflection of rising prices or more and better rehabilitation services and the provision of a better prison environment? How do we correct for services that were nonexistent in the earlier years and now are large items, such as the control of automobile traffic?

The magnitude of the problem can be put very simply: public expenditures covered by this study grew almost fifty-fold, from 381 million Swedish crowns in 1913 to 18.2 billion in 1958. As his growth yardstick Höök selects total civilian consumption—private consumption and public consumption for nonmilitary purposes—compared with which public expenditures rose four times as much. Interestingly enough, military costs rose more slowly than other government outlays, falling from 22 per cent of the total in 1913 to 16 per cent in 1958.

The government's share of public civilian expenditures rose during the period from 38.6 per cent to 50.1 per cent as the municipal share declined. Transfers from the state grew from 10 per cent to 27 per cent of municipal expenditures, mainly because of the establishment of national pensions and child allowances.

As a share of total public civilian consumption, the major shifts were relative declines in general administration costs and the justice and police outlays, while the relative increases were for health and sick care and roads and streets. Although education rose only slightly in relative terms, it continued to be the largest category, absorbing 27.5 per cent of the total in 1958, with health and sick care accounting for 22.6 per cent, and roads and streets for 11.4 per cent.

A good deal of the growth in government services represents a transfer from private or individual costs to government undertakings—perhaps the major influence. For example, a shift in medical care from the home to the hospital raises public outlays, but may not represent a rise in total outlays for this item, since the individual is spared this cost. This shift is well illustrated for childbirths, of which 93 per cent were at home in 1913 and only 1 per cent in 1958. During the postwar period there was also the introduction of free schoolbooks and school meals; since there does not seem to have been a growth in the total consumption of these items commensurate with the rise in government spending on them, it can be assumed that here, too, there has simply been a transfer from the private to the public sector for much of the apparent increase. Similarly, the expansion of the trade schools has lessened the training burden for industry. And probably most important in its impact is the care of the aged; in a rural society the retired member was simply absorbed by his family, whereas today the support cost is a public outlay. Apparently it is these services for which the income elasticity is highest, thus accelerating the reported increase.

Other ways in which this income elasticity is reflected are the new demands arising from growing prosperity. Increased attention and rising standards of care for alcoholics, the mentally retarded children, disabled adults—all these contribute to the more rapid rise in government outlays. In turn, these added services have caused a companion growth in the administrative costs of government. More road construction means more traffic and a greater need for policemen; more care for the aged means more social workers; a rising degree of urbanization requires more police protection, etc.

For the reader interested in the details of this study, the publishers have provided an English summary at the end. In addition, even those totally ignorant of the Swedish language can understand the numerous tables and graphs, since the headings can be easily understood with the use of any standard Swedish-English dictionary. With some 70 major tables (and numerous minor ones) and about as many graphs, it is not too difficult to follow the details of the findings even if the text itself is foreign. The language barrier should hardly prove to be formidable with all these aids.

EDWARD MARCUS

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Fiscal Policy, Cycles and Growth. By MICHAEL E. LEVY. Studies in Business Economics, No. 81. New York: National Industrial Conference Board, 1963. Pp. 141. $2.50; $12.50, nonassociates; $1.00, students and educators giving school address.

If the rate of growth, the international position, and internal rigidities and irreversibilities of the U.S. economy could be safely ignored, the macroeconomic goal of federal policy is simply stated: government saving (i.e., the surplus of the national-income-accounts budget) must exactly offset the gap between private investment and private saving that is generated at full-employment without inflation. A full-employment government surplus in excess of this gap, given the aggregate structural relationships in the economy, results in underutilization of the nation's productive resources. It is the contention of the Council of Economic Advisers, emphasized in its 1962 *Annual Report,* that the federal fiscal system does indeed tend automatically to develop excessively large government saving at full employment, and that this is largely responsible for the relatively slow growth of the U.S. economy and for its failure to perform up to full potential during the last half decade. In line with this diagnosis, the Kennedy administration has proposed extensive revision of the tax structure in the expectation that private investment will thereby be stimulated and, significantly, that the full-employment budgetary surplus will be substantially reduced. Yet, the correctness of the Council's diagnosis may be questioned, since competing hypotheses of the causes of the failure of macroeconomic performance—the abortive recovery from the 1957-58 downswing upon which was superimposed the recession of 1960-61 and the recent prevalence of structural imbalances—have been persuasively proposed.

The major achievement of Levy's study is to subject the Council's "fiscal stagnation" thesis to alternative empirical analysis. In spite of its brevity, it is a valuable effort, not merely for its own provocative examination of this thesis but also because it summarizes, from scattered sources (some as yet unpublished), the previous empirical work on the thesis. An interesting, rudimentary international comparison of actual (not full employment) budget balances, growth rates, and rates of inflation suggests a lack of correlation between the budget balance, on the one hand, and rates of growth and inflation, on the other. And as a by-product of his analysis, Levy gives useful insights into the little-appreciated implications of a full employment budgetary balance calculation, into nuances of the estimation of potential GNP, and into the economic significance of the various methods of budgetary accounting. Only a moment's reflection is needed to appreciate that estimates of full employment GNPs and budgetary balances are integral to the appraisal of the Council's hypothesis. Yet they are also ancillary and to dwell on these computations, although the exercise would be enlightening, would detract from the important theme. Suffice it to report, not unexpectedly, that different computational methods yield, according to Levy, significantly different results for the magnitudes of both the full-employment budgetary balances and potential outputs (at various points the findings of the Council, Denison, Knowles, and Okun as well as those of Levy are compared). Therefore, estimates of absolute sizes of the budgetary balance and the gap between actual and potential GNP are not reliable. Relative orders of magnitude are reliable, however, so that changes in the size of the macroeconomic performance gap are a good index of changes in resource utilization, and changes in the full-employment budget balance indicate reasonably well changes in the restrictiveness of the fiscal structure.

Levy, in evaluating the "fiscal stagnation" thesis, develops a method of estimating the saving and investment magnitudes consistent with full-employment equilibrium that is different in several respects from that of the Council. The Council's data are semiannual; Levy's are quarterly. Levy determines structural relationships between investment and the two forms of saving, as percentages of actual GNP, and the unemployment rate; the Council estimates absolute levels rather than regression equations. Most importantly, the Council calculates full-employment government saving directly from its estimate of full-employment GNP; Levy estimates full-employment saving and investment magnitudes by computing their full-employment (four per cent unemployment) value directly from the equations derived by regressing *actual* percentage budgetary balances, private saving, and private investment on the unemployment rate. Both of these methods are, of course, open to criticism. The Council must somehow "allocate" full-utilization GNP among its "revenue base" and "expenditure inducing" components to produce a full-employment budget. Levy's statistical procedures are perhaps not as refined as his data permit or his analysis suggests. In view of the apparent inconsistency between a four per cent unemployment rate and price-level stability, it can be asked in what sense Levy's estimates are the full-employment *without inflation* values. Finally, if the study is to be useful for policy prescription, it is important to know precisely what "composition" of fiscal structure is implied by Levy's derivation.

Despite these difficulties, Levy's conclusions should be of great interest. They constitute in part a confirmation of the Council's position on fiscal policy and its explanation of sluggish macroeconomic performance; but according to Levy they present as well a challenge to the Council. Levy finds that after 1953, except for 1960-61, government and private saving at full employment would have balanced full-employment investment at, roughly, 16.5 per cent of GNP. Thus, the fiscal structure was not incompatible with the attainment of full employment, given the propensities of private savers and investors. In contrast, due to an increased restrictiveness of the federal fiscal system in 1960-61—the budget *shifted* to a significantly higher saving structure at all unemployment rates—the "deflationary gap" was approximately 1.3 to 1.8 per cent of GNP. For these years the "fiscal stagnation" hypothesis receives support. Beginning in 1962, however, the budget structure apparently reverted back to its late 1950's lower saving level. If Levy's speculation on this is correct (and it is highly conjectural, being based on two observations only), the Council's persistence in its "fiscal stagnation" interpretation of the continuing high unemployment and low growth rates is surely misdirected. Rather, Levy implies that the more reasonable explanation is likely to be found in structural and sectoral imbalances.

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International Economics

Problems of the United States as World Trader and Banker. By HAL B. LARY. Studies in International Economic Relations, No. 1. Princeton: Princeton University Press, for National Bureau of Economic Research, 1963. Pp. xvi, 175. $4.50.

This is a useful analysis of the U.S. balance of payments from the end of World War II. Its stated aim is to identify problems, with emphasis on those needing further research. It discusses alternative policies "not with a view to arriving at specific proposals, but in order to indicate possible choices and some of the principal considerations . . . in weighing them" (p. 136).

Chapter 2 and Appendix A discuss alternatives to the Department of Commerce measure of the payments deficit. The Commerce definition takes the sum of "basic transactions," recorded flows of U.S. private short-term capital, and errors and omissions. The adjusting items against this balance are official gold sales, the increase of liquid liabilities to foreign monetary authorities, and the increase of U.S. liquid liabilities to foreign commercial banks and others. The Lary definition reduces the Commerce deficit by the increase of U.S. liquid liabilities. That is, *all* private short-term capital flows, whether U.S.-owned or foreign-owned, are treated as basic transactions. Hal Lary calls this the "deficit on an official settlements basis."

Esthetic consistency would seem to argue for the Lary definition. But is there perhaps a stronger rationale for the Commerce definition? So long as foreigners use the dollar for financing international transactions, it is correct to view U.S. liquid-liability increases as a close substitute for gold sales. It reflects changes in the payments balances of countries that finance their trade in dollars, be it with the United States or with third countries. On the other hand, U.S. firms finance their trade mainly in dollars in any event. Their holdings of foreign deposits and trade credits change not as passive responses to trade changes but because of active decisions to hold more or less of foreign currencies (to be sure, sometimes in connection with trade promotion).

Once, however, speculative behavior of foreign dollar-holders becomes important, U.S. liquid-liabilities take on a more nearly autonomous character, and the official-settlements measure becomes more interesting. I suggest that the Commerce definition is the more appropriate one for most of the postwar period, and that the official-settlements definition becomes useful, at least as a supplement, for the last three or four years. Of course, for the question of fundamental disequilibrium, the current-plus-long-term capital balance remains the appropriate measure, rather than either of these.

What do the data reveal as to the nature of the dollar disequilibria of the postwar years? Lary thinks that the early postwar dollar shortage was due to a coincidence of misfortunes and not to any fundamental set of forces (p. 6). From 1950 onward, the surplus changed to a deficit, which until Suez (1956) was welcome because it redistributed the world's gold reserves. After Suez the deficit began to strain our reserves. The cumulative deficit for 1950 to 1962 was large by any of the three measures: $25.3 billion on the Commerce definition, $23.1 billion on the basic transactions definition, and $18.5 on the official-settlements definition. The last was smallest because foreign firms and commercial banks chose to build up their dollar balances rather than accumulate their home currencies. Foreign monetary authorities, too, accumulated more dollars than gold in this period, although the balance tipped to gold after 1958.

Lary emphasizes that the difficulties of 1960-62 were due to an important extent to short-term recorded and unrecorded U.S. capital flows. Exports had recovered strongly from their 1958 drop and were the main force in reducing the basic deficit. Beyond the unfavorable impact of short-term capital movements was the fact of the increasing burden from government transfers, military spending, and private investment. These long-term items required a substantially faster growth of exports than imports, a growth that was not quite realized.

Chapter 3 deals with "Elements of Strength and Weakness in Basic Transactions." Most of the ingredients of the discussion are not new and need only brief mention here. On the unfavorable side are the growing transfer and investment burdens already mentioned, structural improvements in the economies of Europe and advances in foreign technologies. Inconclusive is the behavior of U.S. export-price indexes, while our wage-cost position has developed favorably compared to our competitors. An interesting point is that our research-intensive exports have grown faster than general exports, and that this fact may be connected with our heavier business outlay on research and development. On the whole there is reason for cautious optimism on the future of our competitive position.

But balance-of-payments predictions have a high degree of uncertainty, and this fact makes it desirable to discuss possible combinations of measures. This is the task of Chapter 4. Lary believes that our freedom of action is limited by our key-currency position, by our role in world affairs, and by the characteristics of our economy. His argument against deflationary measures is curious. Instead of arguing, as many would, that induced recession is a bad thing per se, he argues that the United States would need a larger deflation than other countries for the same effect because of the small ratio of our trade to our GNP. The supporting example (p. 106) compares the United States with a typical European country, where both have a deficit equal to 1 per cent of GNP, but the shares of imports in GNP are respectively 5 per cent and 20 per cent. The United States therefore requires a 20 per cent reduction of activity (1 per cent times the reciprocal of 5 per cent), and the other country needs only a 5 per cent reduction. Does he wish to imply that 5 per cent is still an acceptable reduction of activity? But the example itself is rigged. Is not the reasonable assumption for a comparison that both countries have the same deficit relative to their trade volume? In that case both would need exactly the same degree of recession, provided that their income elasticities of import demand are the same. I would regard the smaller reduction of activity as thoroughly objectionable, too.

It is difficult to pin the author down on his exchange-rate policy position. In the first place, in his view the U.S. payments position looks strong enough that no action appears necessary; and so the issue of one-time devaluation versus flexibility becomes for him a hypothetical question. He thinks that other countries would practically all follow suit in the case of devaluation—without making clear why—and believes that in addition the action would provoke a rush to hoard gold through loss of confidence in currencies generally. Flexible rates have the added danger of provoking speculative capital flights, which would be especially troublesome for the United States. He regards the recent Canadian experience as supporting this pessimistic view. This issue is a matter of judgment, but it seems to me that Lary is overly impressed with the European experience of the 1930's, and a bit selective in taking only one episode out of 15 years of generally successful Canadian experience. In sum, the author opposes flexible rates under foreseeable conditions for the United States, but admits that conditions *could* arise where fixed rates would create undue strains of adjustment.

Once rate adjustments in any form are rejected under present conditions, it remains to see what adjustments can be made in the fixed-rate framework. Lary has some hope for differential upward price movements, whereby the United States would hold its price-level more tightly than the surplus countries. He also sees adjustment of a slow sort through "competitive response" taking place (e.g., the compact car) and through export promotion.

The book ends with a cursory discussion of ways to increase U.S. reserves. Viewed as a step forward is the new "supplementary resources" agreement among the United States and nine other countries. The Triffin proposal for centralizing monetary reserves in the Fund also receives favorable treatment. But Lary sees two main difficulties in all proposals to increase international liquidity: (1) the apportioning of amounts of financing to be given or received by individual members, and (2), the degree of automaticity to be attached to such rights and obligations (p. 133). The surplus countries would have interests diametrically opposed to those of the deficit countries. This point is of course correct; but when we look at the successful experience of EPU with similar problems, it seems to me that the problem is surmountable. Indeed, we *need* opposition of interests, because there is such a thing as giving deficit countries excessive credits.

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The Terms of Trade. By F. V. MEYER. Copenhagen: Munksgaard, 1962. Pp. 160. Paper, DKr. 28.

This small softbound volume represents a highly specialized exercise that is clearly a product of considerable thought by the author, but will appeal to only a small percentage of the profession—presumably international trade theorists. As suggested by the title, the book's substantive content deals with a rather nebulous area of trade theory in which greater knowledge is obviously needed. However, unless the author can express his empirical findings and resultant conclusions with greater clarity, few of his colleagues will benefit from the research efforts. Indeed, the assertion that "it is not necessary to be any good at mathematics to be able to follow the argument" (p. 10) does not, in this reviewer's judgment, describe reality. Although higher math is not utilized, a general economist, facing page after page of ratios, will feel handicapped in terms of understanding, owing to the type of symbolic thinking required throughout the text.

Chapter 1, in the main a former journal article, analyzes historically the factors that determined the terms of trade in manufactures between the 1880's and 1950's, with due acknowledgement to the work of W. A. Lewis. This examination relates the growth of trade in manufactures to two variables: (a) the growth of all trade, and (b) the rate of growth in the production of manufactures. In Chapter 2, a modernized version of the theory of reciprocal demand is developed, involving changes in the relative values of trade. Rather serious data limitations necessitate rigorous assumptions which are adopted in order to test the theory on seven countries with results that indicate ". . . concomitant movements in expected and actual terms of trade suggest a rapid adjustment of relative quantities to relative prices and of relative prices to relative quantities" (p. 81). Chapter 3, utilizing ordinal rather than cardinal relationships, attempts to combine and systematize the preceding analysis by discussing four possible growth rates in terms of the most probable direction of the terms of trade in addition to the likely changes in international values relative to domestic values. In short, an effort is made to find the association between changes in these growth rates and movements of relative values in trading activity.

Following a general commentary relating to qualifications of the analysis noted above, Meyer writes, "It must be realized, however, that the influence of governmental authorities and trade organizations over the terms of trade is limited" (p. 153). From this observation, one may reasonably conclude that the direct public policy implications of this study are not particularly significant. Finally, it must again be noted that it is unfortunate the basic thesis of the book is not presented more succinctly. One cannot help but think the argument, as presented, would receive a wider hearing if summarized for distribution via a professional journal.

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A Study of United Kingdom Imports. By M. Fg. Scott. Economic and Social Studies No. 20 of the National Institute of Economic and Social Research. New York: Cambridge University Press, 1963. Pp. 269. $10.50.

The aim of this book is to isolate and measure the effects of the more important factors influencing the quantity of imports into the United Kingdom. Particular emphasis is given to explaining the changes in four broad categories of imports: food, raw materials, fuels, and manufactures, between the periods 1935-38, 1946-49, and 1954-55.

In general, the factors influencing the quantity of imports are those which cause shifts in domestic demand and supply curves and those affecting the relative price of imports. Mr. Scott demonstrates that the importance of these various factors varies among groups of imports. While relative price movements account for a large proportion of the movement, between 1935-38 and 1954-55, in imports of food and manufactures, such price movements are not as important in explaining the movement in raw material imports.

Scott notes that, while total home market sales of manufactures were 67 per cent higher in the period 1954-55 than in the base period 1935-38, imports of manufactures rose only 6 per cent between the two periods. He explains this postwar decline in the relative importance of manufactured imports in the total home market by the fact that imports became relatively more expensive than home-produced manufactures. He thus suggests that there is a high price elasticity of demand for manufactured goods. He offers as proof of this proposition the experience of manufactured imports during the years 1931 and 1932. During this time, the British pound depreciated, relative to the currencies of the major suppliers of its imports, and in addition tariffs were imposed on most manufactured imports. This two-barreled increase in the relative price of imports resulted in an almost immediate drop of about 40 per cent in the volume of manufactured imports—this at a time when there was no significant drop in *total* sales of manufactures in the home market.

The fact that Scott's findings lead him to derive a considerably higher price elasticity of demand (about -7) than other investigators, does not disturb him:

> The fact that the relative prices of imported and home-produced manufactures changed little in peace-time before the last war also explains why previous econometric studies of the demand for imports of manufactures often found that relative price changes were not closely related to changes in the quantity of imports. Such relative price changes as occurred were small, and are best regarded as merely chance fluctuations such as one expects to find in index numbers. One cannot conclude from this that the price-elasticity of demand for imports is low. Instead one should ask *why* the relative price movements were so small, and the most plausible explanation seems to be that the price elasticity of demand was high (p. 43).

The rapid increase in imports of manufactures between 1946-49 and 1954-55 cannot be explained, however, by any marked cheapening of imports relative to home production—rather this explanation rests with the inability of home production to keep up with the rapidly rising domestic demands.

The author has obviously performed a great many original and painstaking statistical computations which provide a wealth of information that should be useful to other investigators in the field. If one can find fault with these calculations, it is in the author's zeal to set forth nearly every one of them in his chapters. This tends to make the reading somewhat slower and more tedious than is perhaps entirely necessary by the empirical nature of the study. The writing as a whole is clear and pointed, and the many assumptions necessary to such an empirical endeavor are set forth cleary and boldly.

An all too brief Chapter 4 examines the possible impact on the British balance of payments of government policy measures such as devaluation, deflation, import controls, subsidies to agriculture, and tariffs. Although the analyses set forth in this chapter are not new or unique, the attempts to quantify them are. In my opinion, both the usefulness and the readability of the book would have been increased by an expansion of this section of the book (and perhaps a shortening of others).

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The United Kingdom and the Six—An Essay on Economic Growth in Western Europe. By A. Lamfalussy. Homewood, Ill.: Richard D. Irwin, Inc., 1963. Pp. xviii, 147. $4.50.

This publication of The Economic Growth Center, Yale University, written while the author was on leave from the *Banque de Bruxelles,* undertakes to explain why the economies of the "Six," primarily Germany, Italy, France, and the Netherlands, have done far better than Britain's by the standards of growth, price stability, and external balance. The author has previously argued that there were no obvious reasons to attribute, at least until 1961, the spectacular growth of the continental countries to the establishment of the European Ecomonic Community.[1]

The first chapter describes the main characteristics of growth on the continent and in Britain between 1950 and 1961 and concludes that attention must focus on both supply and demand, the latter being influenced by the trend of exports as well as by the ability to let domestic, especially investment, demand expand freely. Chapter 2 finds that, although the British economy suffered less direct war damage, enjoyed a faster immediate postwar recovery, and had long enjoyed a higher standard of living than most continental countries, the process of catching up with the British might explain the growth rate on the continent prior to 1953 but has lost its explanatory validity since then. The next three chapters argue that the rate of growth of labor productivity has been more important than differences in the rates of increase in the supply of this factor, and that heavy and repeated accidental and political pressures on sterling do not fully explain the marked differences in the export performances of the Common Market countries on the one hand and of the United Kingdom on the other. The fall in Britain's share of world exports between 1953 and 1960, while the share of the "Six" rose, is ascribed mainly to the unfavorable trend of unit labor costs in the United Kingdom, which is in turn attributed to a lower rate of increase in labor productivity in Britain rather than to more rapidly rising wage rates.

Chapters 6, 7, and 8 explore the intricate relationship between capital accumulation and the rate of growth of productivity in order to contrast Britain's "vicious circle" with the continent's "virtuous" one. The thesis is developed that although the net marginal capital-output ratio in *all industry* was higher, 1953-60, in the United Kingdom than in Germany or Italy, the efficiency of net investment (the net investment-productivity ratio) in increasing output per head in the United Kingdom was slightly higher than in Italy but lower than in Germany. The net marginal capital-output ratio for the *manufacturing sector only*, however, was a bit higher for the United Kingdom than for Germany and Italy, but the net investment-productivity ratio for manufacturing was lower in the United Kingdom than in either of the other two countries, and markedly lower after allowing for unused British capacity. From this it is argued in the two concluding chapters that it was not slower technological and organizational progress in the United Kingdom manufacturing but "export-led" growth that enabled the continental countries to outstrip Britain. The rapid development of German and Italian exports, it is claimed, raised both the investment and savings ratios, the latter more than the former, thereby enabling capacity and per capita productivity to grow faster without producing inflation and pressure on the balance of payments. Growth in France was stimulated by home expenditures and accompanied by inflation prior to 1958, when she also began to experience "export-oriented" growth. High labor costs and military expenditures prevented

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[1] *Lloyds Bank Review*, Oct. 1961.

Britain from following the path of "export-oriented" growth, and because of her key-currency responsibilities she could not disregard her balance-of-payments and pursue the French pattern. The author concludes that the United Kingdom could join the higher orbit of the Common Market countries only if she raised her investment ratio sufficiently to raise the rate of increase of her labor productivity, but even then it may take her a long time to overcome the competitive advantages recently gained by the Six.

Some readers may question the direction of causality described by Dr. Lamfalussy. It is always difficult to obtain unanimity in "chicken and egg" sequences, but this reviewer found the historical analysis convincing. More troublesome to me was whether the author had adequately "explained away" differences in such things as the quality of labor, technological progress, industrial structures, rates of depreciation, obsolete facilities, etc. The author does not attempt to relate his analysis to the Harrod-Domar or Abramowitz-Solow growth models. It should be noted that "capital widening" and "capital deepening" appear to be in reverse order at the top of page 88.

Despite the author's well taken caveat (p. 11) that it would be misleading to insist on easy analogies between British difficulties and North American problems, this volume fully warrants high priority on the reading lists of all who are concerned with the theory of growth in mature economies or who are responsible for their economic policies.

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The Economic Functions of Gold. By IAN SHANNON. Melbourne: F. W. Cheshire, 1962. Pp. 139. A£1/1.

The role of gold in the present international payments system is a worthy subject of economic research. Why do most economists insist on speaking of a "gold exchange" standard when national currencies are exclusively used as means of international payments and, together with resources of supranational institutions, predominantly used as international reserves? Does gold fulfill a useful function in the system, and if so, how could it be expanded, and if not, how could it be reduced?

The author fails to pose or answer any of these questions. Instead, he presents some rather elementary remarks on the tradition of gold (Chapter 1), the interwar gold muddle (Chapter 2), the allegedly perplexed postwar world (Chapter 3), and the monetary functions of reserves (Chapter 4), and devotes the remaining five chapters to arguments in favor of a tripling of the dollar price of gold.

Without producing any convincing evidence, he states that "the world has . . . always been short of reserves since the breakdown of the traditional gold standard" (p. 60); that this shortage explains the "poor export prices of primary exporting countries and their balance of payments difficulties" (p. 62); and that it impedes "the unsteady progress of gross national product" (p. 69). In particular, the less developed nations need "much higher reserves" (p. 76), and economic aid granted by fully developed countries should rise to "*at least*

10 billion per annum" (p. 79, author's italics). Apparently, the author believes that the U.S. Congress as well as the European legislatures will immediately triple their foreign aid programs as soon as the dollar is declared equal to 1/105th instead of 1/35th of an ounce of gold.

Interestingly (and laudably) enough, the author concedes that "a return to the nineteenth century gold standard" would be contrary to "contemporary economic and political philosophy" (p. 88). But if gold is not again to become the ultimate standard for domestic and international monetary policy, it remains all but incomprehensible why it should become again the exclusive international means of payments and reserve asset.

Apparently the author's position is based on a misunderstanding of the meaning of money, reserves, and liquidity. Since national currencies (like all modern forms of money) are technically liabilities as well as assets, the author believes that additions to holdings of national currencies "do not really give additions to liquidity in an overall international sense" (whatever "an overall international sense" may mean [p. 90]). Does the author also believe that domestic liquidity in the United States can be increased only by expanding the circulation of coins because Federal Reserve notes and bank deposits are liabilities as well as assets?

This misunderstanding prompts the author to dismiss any correction of a (nonexistent) present or a (possible) future liquidity shortage by means of "credit, of the type made available through the Fund, Basle Agreements or lender clubs"; for "each addition to the lending arrangements now in vogue presupposes a shortening of the total surplus available in the collective hands of surplus countries" (p. 93). Credit is indeed limited by the potential effects of excessive expansion on prices and costs and thus on output and employment; but it is almost infinitely elastic in relation to the supply of means of payments and does not reflect an unchangeable "total surplus" (of what?), in either individual or "collective" hands, in either a national or the international economy.

Only at one point does the author seem to perceive one of the genuine problems of the present payments system, "the instability arising from the choice of two or more alternative sources for switching volatile funds" (p. 96). But he fails to see any solution other than the elimination of such choice. In particular, he considers the "further extension of the key currency principle" to be "the least satisfactory and clearly the most potentially unstable" solution (p. 122). He does not ask whether ways could be found to reduce instability without giving up the obvious advantages of a system that is consistent with both the historical development and the actual practice of national and international payments.

In the last chapter, the author tries to refute arguments against an increase in the price of gold. In dismissing the "rebuttal that it is foolish to raise the price of gold since the whole operation needs to be repeated at regular intervals," he quotes Busschau's argument that this is "like saying that a distressed traveller should not be comforted by drinking from the St. Bernard's brandy keg because it would mean that he would keep on drinking until he became a drunkard" (p. 126). But this analogy does not hold. We may assume that the rescued traveler will continue on his way and will not need to be rescued over and over again. But if gold were the exclusive international means of payments and if it were true (as the author believes) that the value of the means of payments must always be increased in line with the expansion of domestic incomes and international commerce, then periodic revaluation of gold would be inevitable—unless we could assume, contrary to all present indications, a rather fantastic expansion in world gold output. But periodic revaluation of gold in terms of all national currencies would cause gold hoarding to become the safest and most profitable economic activity all over the world, and would cause all newly mined gold to flow into private hoards. Thus, the rise in the price of gold would create, not cure, an international liquidity shortage.

The patient reader may well ask why a book so deficient in novel or correct analysis deserves lengthy review. But the book is important as the representative of a species rather than as an individual work. The false gospel that promises to solve the world's economic ills by the stroke of a pen that would raise the dollar price of gold seems to exert an unholy fascination on the minds of economists. As long as this heresy flourishes, the genuine problems of the international payments mechanism and of international liquidity will defy rational solution.

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The History of Foreign Exchange. By PAUL EINZIG. New York: St. Martin's Press, 1962. Pp. xvi, 319. $7.50.

Over the past 40 years Paul Einzig has written a total of no less than 50 books, mainly on international financial topics, as well as innumerable articles in newspaper columns, banking magazines, and professional journals. The great majority of these books have been of a topical character written chiefly for practical specialists or a more general audience and in a journalistic and sometimes aggressively polemical style. Many of them, as Einzig himself admits in his autobiography (*In the Centre of Things*, 1961), have been rather superficial and one-sided in the presentation of their arguments, and some were written in a matter of days. But several of his books have been of a more technical and scholarly character and a few of them, most notably his *Theory of Forward Exchange,* written in 1937 and considerably revised in 1961, have justly earned the status of standard works on their subjects.

In the preface to one of his earlier books, Einzig described himself as standing somewhere halfway between practical specialists and academic economists. This judgment is not likely to be significantly altered as a result of the publication of his latest book. While the *History of Foreign Exchange* is one of the more scholarly, objective, and mellow products of his pen, evidencing considerable research into the ancient and modern literature on the subject, it is essentially a semitechnical book written for a diversified audience. In it he attempts to present a broad outline of the history of foreign exchange in its practical and theoretical aspects from the earliest days to the present time. It is an exercise both in economic history and *Dogmengeschichte* undertaken with a view to providing a summary guide to the changing pattern over time of foreign exchange practices, theories, and policies. While there is already an extensive literature on these matters for given historical periods and for specific aspects of the larger subject, no one had as yet attempted to encompass the entire history of foreign exchange in all its aspects since its origins in the ancient world. The appearance of this book is thus to be welcomed.

In his treatment Einzig distinguishes five historical periods, which include the Ancient Period from Babylonia to the fall of the Western Roman Empire; the Medieval Period up to the discovery of America; the Early Modern Period up to the French Revolution; the 19th century and the years up to the outbreak of World War I; and the period from 1914 to the present. For each of these periods he discusses his subject from the viewpoint, respectively, of foreign exchange organization and practices, trends in exchange rates, theories of foreign exchange, and foreign exchange policies. This method of organizing his material involves a certain amount of overlapping, and his attempt to cover so vast a subject over so many years within the confines of a relatively small book inevitably involves sketchiness and some superficiality of treatment.

To an economist not specialized in foreign exchange, the more revealing chapters are likely to be those dealing with the earlier periods. Apart from finding a useful if brief survey of the origins and early evolution of the foreign exchange market and exchange practices, he may be amused to learn, among other things, that Plato recommended the adoption of the gold exchange standard, that Aristophanes "invented" Gresham's Law, that a rudimentary Purchasing-Power-Parity Theory made its appearance as early as the 16th century, that the first known forward exchange transactions were initiated in 1317, and that the first known calculation of specie point was made in 1558. These chapters also bring together some useful material on early monetary and banking developments and on the role of the Church in the evolution in foreign exchange. As one approaches the more modern period, however, the treatment becomes increasingly sketchy and the information provided increasingly less novel.

The least satisfactory chapters are those dealing with foreign exchange theory which, as Einzig points out, did not really come into existence until the 16th century. For one thing, he nowhere defines exactly what is to be included in this term and at times, e.g., his discussion of the Hume-Cantillon theory of the price-specie-flow mechanism, he seems to embrace a wider area than is customarily subsumed under it. Second, his treatment of the Mercantilist and Bullionist controversies would have profited greatly from a more careful reading of Viner and Heckscher. Weakest of all is his discussion of modern foreign exchange theory which, apart from his survey of forward exchange theory to which he himself has contributed importantly, focuses heavily on the purchasing-power-parity and related controversies of the 1920's. While there is passing reference to the path-breaking articles on foreign exchange by Joan

Robinson and Fritz Machlup in the late 1930's, there is virtually no reference to the post-1945 literature and none at all to the important discussions on the stability of the foreign exchange market and the theory of flexible exchanges.

In his chapters on foreign exchange practices and organization, Einzig writes with more assurance and reveals his intimate knowledge of the functioning of exchange markets in the 19th and 20th centuries. As for foreign exchange policy, he is not always as clear as he might have been regarding the content of this term, which he interprets to include not only official purchases and sales of foreign exchange to influence exchange rates, exchange rate adjustments, and exchange control, but also at times discount rate policy and even tariff changes. The chapters on exchange rate trends for the later periods would have been more effective had the author made some use of available statistical series rather than rely exclusively on a straightforward, and at times somewhat tedious, narrative.

It is easy to find fault with this book on specific points and to overlook the modest claims which Einzig makes for it as a broad guide to developments in foreign exchange over a period of 4,000 years. In this respect it is a very useful reference book indeed. But it is clearly no substitute for an examination of the more specialized literature available on specific aspects of the subject and for specific periods. The limitations of the book stem from the fact that the author attempted too much in too short a space. It should be a challenge for someone to try to do better.

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Business Finance; Investment and Security Markets; Insurance

The Theory of Financial Management. By Ezra Solomon. New York: Columbia University Press, 1963. Pp. xii, 170.

Corporation finance is becoming an increasingly exciting subject. For generations, almost by basic definition, the subject was descriptive and static. It consisted largely of the legal principles involved in establishing, running, or dissolving a business, some widely accepted accounting conventions, and discussions concerning the suitability of financial instruments under various circumstances. It was a subject of bits and pieces with little in the way of fundamental theory or rigorous analysis.

Now corporation finance is being remade into one of the most dynamic areas of economic analysis. The trend has been gathering steam for little more than ten years. Anyone who doubts it, should read Professor Solomon's *The Theory of Financial Management.*

To speed the diffusion of the new direction of thought, the Ford Foundation encouraged the preparation of this manuscript. And although the preface explicitly recommends the book solely for the use of teachers, it will undoubtedly contribute much to the enrichment of students enrolled in advanced graduate courses.

The principles of marginalism, so long embedded in the economic theory of the firm, are gradually finding their way into corporation finance. As Solomon states early in his work "the theory of financial management can be viewed as an extension of the theory of the firm. But whereas the traditional emphasis in microeconomics is on the relationship between profits and the volume of output, with the amount of capital input taken as fixed, the theory of financial management is expressly concerned with the relationship between profitability and the volume of capital used." Thus the concern of the new corporation finance is with an integrated theory of the optimal use of a firm's *financial* resources, of the best combination of inputs for any given structure of output in the light of prevailing market conditions and prices.

Although this book does not break new ground and therefore is unlikely to become the *General Theory* of the new finance, it is a well-organized, succinct, and critical presentation which brings together a considerable amount of recent, scattered, and controversial material. Its major focus is on the proper measurement of the cost of various types of capital—new issues of common stock, retained earnings, and debt. This emphasis is well placed, since answers to a host of financial questions revolve about these costs. Should a firm commit capital funds for certain purposes? How should the required funds be raised? How does the cost of capital vary with the changing capital structure of firms?

Investment decisions revolve about the marginal return on new investments versus the marginal cost of new capital. This book seeks to isolate the critical variables. Admittedly, the algebraic equations presented by Solomon cannot be applied directly by the business manager. The state of the science is not sufficiently advanced to make the new principles immediately operational. But laying bare the key elements that need to be weighed in financing decisions is a major step forward.

In tackling these issues, it is only natural for Solomon to draw heavily on his previous writings in the field. Thus, the chapter dealing with measuring the profitability of new investments is very close to his article on the subject in "Measuring a Company's Cost of Capital" (*Journal of Business*, October, 1955). But where his present analysis leads him to take a position contradictory to an earlier one, as in the formulation of the cost of retained funds, he is frank to admit it.

He believes so strongly in the sequential marginal costing of capital that he does not even inveigh against the average cost of capital school. The cost of external equity capital is k_e, or the capitalization rate at which the market values an expected stream of earnings. The inadequacy of any other standard such as book value per share, return on investment valued at book, or dividend requirements is clearly presented. All investment proposals must be screened according to whether or not they increase the firm's present net worth. Complications arise in the case of growth models where the valuation formulae take into account current earnings and dividends as well as assumed growth rates for these variables. Chapter 6, dealing with the combined cost of debt and equity, takes up the effect of debt on the capitalization rate of equity and therefore on the over-all cost of capital to the firm. The basic relationships involve weighting the respective costs of new debt and equity capital (based on the capital structure *after* the new investment is made) by the proportions of debt and equity valued at market.

On the question of leverage, Solomon has an incisive analysis of the Modigliani-Miller thesis. He arrives at the conclusion that, so far as the cost of capital is concerned, there *is* an optimal range of leverage but that the horizontal segment of the curve is probably wide. In connection with the issue of deciding upon an optimum volume of debt, it is unfortunate that the book was unable to draw upon the provocative contribution of Professor Gordon Donaldson in *Corporate Debt Capacity* (Harvard, 1961).

The general solutions to the theory of financial management currently available provide a useful conceptual framework within which judgment can be exercised. Obviously, only the surface has so far been scratched. Numerous issues must still be resolved. For instance, what is the cost of depreciation, of debt, and of preferred stock, taking corporate income tax rates into account? What is the effect of the diverse personal income tax rates of stockholders on the cost of capital? (On page 54, Solomon argues that it is unnecessary to consider this refinement.) How is it possible to estimate the effect of prospective financing on the capitalization rate of a firm's outstanding securities?

Solomon has contributed importantly in moving us to the threshold of exciting new developments in corporation finance. He does not answer all questions, but he asks the right ones. What is needed now is a bridge between theory and practice. We need more empirical studies on current practices, a rewriting of corporation finance texts, and studies and proposals on ways to integrate the theory of financial management with business practices.

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Business Organization; Managerial Economics; Marketing; Accounting

Price, Output and Inventory Policy—A Study in the Economics of the Firm and Industry. By Edwin S. Mills. New York: John Wiley and Sons, 1962. Pp. vii, 271. $7.95.

As the title indicates, this is a book about the price and output, or sales and output, decisions of business firms. The analysis is dynamic, allowing for uncertainty of demand and calling for intertemporal cost minimization. In the author's words (p. 124), "the two main strands of the dynamic literature [i.e. the "price stability" literature and the "inventory accelerator" literature] can be separated according to whether they emphasize the effect of inventory on prices or on production . . . the models developed in this book can be thought of as an attempt to unify the two strands of the literature by showing the interactions between inventories, prices, and production in a model of rational decision making." The importance as well as the difficulties of the task are obvious. That it could be carried through the stages of empirical verification is remarkable; it is a tribute both to the author's skills as an economist and to his judicious use of such tools as dynamic programming, computer simulation, and regression analysis. That the attempt could only be partly successful (in particular, that short-run price changes should remain largely unexplained and unpredictable) is not surprising—but somewhat disappointing all the same.

The main steps in the analysis consist in justifying (Ch. 2), deriving (Chs. 5-7), experimenting with (Ch. 8), and estimating empirically (Chs. 9-12), linear decision rules for output and prices, under imperfect competition and stochastic demand. This last feature introduces expectations in the analysis, and the author's original views on this subject are developed (Ch. 3) and applied digressively to a competitive industry (Ch. 4).

The analytic derivation of an optimal price and output policy, taking into account present and expected future costs of production, of inventory carrying, of shortages and of production changes, remains beyond the scope of currently available techniques—and this limitation must be faced by business firms as well as by the economist interested in their behavior. "Therefore, a rational firm should often approximate its true decision problems by simpler ones that it can solve without excessive computational and data-gathering costs. The implication is that the economist's problem in constructing models of firms' behavior is to discover and analyze the simpler decision problem that the firm has used as an approximation of its true problem" (p. 253). The "simpler problem" used by Mills involves only two consecutive time periods. Expected profit is maximized with respect to the first period's output and price, where the expectation is taken with respect to the (additive) random component of demand, and where profit is defined as sales receipts plus the value of terminal inventory minus the costs of production, inventory carrying, shortages and production changes. (Thus, no explicit attention is paid to competitors' reactions.)

Under "appropriate" assumptions (linear or quadratic cost curves, rectangular density), the optimality conditions can be solved explicitly for price and output as functions of past production, initial inventory, and expected sales (themselves a function of prices, etc.). Given data on prices, output, inventory and sales expectations, the model could be tested by statistical estimation of these two functions.

Three serious problems arise at that level. The first one refers to the measurement of sales expectations. Mills argues—convincingly in theory, perhaps somewhat less convincingly in view of the perplexing evidence provided by expectational data—that "decision makers should not be assumed to retain systematic biases in their expectations" (p. 35). Accordingly, Mills suggests that the discrepancy u between a variable x and the decision-maker's expectation x^e be treated as a random variable with zero mean, and the usual additional statistical properties (constant variance, independence of x and independence of its own lagged values). It is now a simple matter, if x^e appears in some exact relation (such as a decision rule) to replace it by x (e.g. to replace sales expectations by actual sales) and to treat the relation as stochastic;

the regression residuals are then to be interpreted as expectational errors, by which x has to be corrected in order to provide estimates x^e of the expectations themselves. This procedure, called by Mills the "implicit expectations" approach, is used in all the empirical studies in this book. The merits and drawbacks of the approach are lucidly discussed in Ch. 3.

The second problem arises because the decision rules give output as a slightly nonlinear and price as a strongly nonlinear function of the explanatory variables. Mills falls back on the natural linear approximations—but takes the extra precaution of testing the adequacy of the approximation by a Monte-Carlo experiment. This consists in first selecting some initial conditions, cost and demand parameters, to be altered in successive experiments; then generating artificial time series by application of the exact (nonlinear) decision rules, and random drawings for the stochastic components of demand. The time series are then used to estimate the linear approximations to the decision rules, and the properties of the regression estimates and of the resulting implicit expectations are scrutinized.

The third problem is identification. The price and output decision rules do not by themselves form a complete model, in the absence of a demand equation. In order to avoid the added complication of having to specify the variables that enter into that demand equation, Mills simply treats sales as if it were an exogenous variable. This is probably the most questionable step in the whole analysis, for the identifiability of the supply side of the model can no longer be determined, and the estimation of the linear approximation to the decision rules (by direct least squares) suffers from a simultaneity bias. This bias is the less severe, the more exogenous variations affect the demand equations. (Thus, in the Monte-Carlo experiment, when no such exogenous variations are introduced, the estimates of the linearized price decision rule yield the equation of the demand curve; when a trend and cycle affect demand, the estimates are drastically altered.) Mills argues that such variations are present in his data and trusts himself to luck.

The four empirical studies are based on industry data (price, output, inventory, and sales) for the major part of the 1930's. Monthly data are used for "southern pine lumber," "pneumatic tires," and "department store shoes"; quarterly data are used for "cement," and independent estimates of sales expectations (the well-known "shippers' forecast") are also available for that industry.

The regression estimates of the output decision rules are quite satisfactory, with large multiple correlation and with regression coefficients having the right signs and reasonable values. Their main shortcomings are: (i) small and unreliable coefficients for the "initial inventory" variable; (ii) serial correlation of the residuals, when monthly data are used; (iii) low correlation in the case of department-store shoes; (iv) questionable agreement with the numerical implications of the theoretical model, which prevents interpretation of the regression coefficients in terms of cost parameters, etc.

The regression estimates of the price decision rules are much less satisfactory. If lagged price is included among the explanatory variables (e.g. on the grounds that price changes are costly), it dominates the regression, accounting for most of the (high) correlation. In the absence of lagged price, correlations are low (R ranging from .1 to .5), particularly for basing-point (cement) or administered (tires) prices. In all cases the coefficients of the nonprice terms are unsatisfactory.

These rather disappointing findings should not, in this reviewer's opinion, be regarded as definitive. Rather, they should be regarded as an indication that a more sophisticated statistical treatment is called for (explicit specification of the demand functions, simultaneous estimation, removal of serial correlation, etc.). It is only after having exhausted these possibilities that one would eventually share the author's conclusion that "a considerable part of short-run price movements must be explained by pervasive, but noneconomic, factors" (p. 260).

This review would not be complete if it failed to mention that the theoretical models of price, output, and inventory policy developed in the book lead to some qualitative statements about the stability of competitive markets (Sections 3-5 of Ch. 4) and the effect of demand uncertainty upon monopolistic prices (Section 4 of Ch. 5 and 6). The generality of these statements is, however, open to question.

All told, this is a very instructive and constructive book that should find its place on reading lists for courses in price theory, in econometrics, and in business or industrial economics. It is clearly written and does not call for any mathematical prerequisites; familiarity with multiple-regression analysis is, however, expected from the reader of the empirical studies.

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Analysis of Inventory Systems. By G. HADLEY AND T. M. WHITIN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. vii, 452. $8.95.

Plant and Stocks in the Production of Goods in Seasonal Demand. By LUIGI D'ALESSANDRO. Oxford: Basil Blackwell, 1963. Pp. 88. 10s 6d.

Despite the striking difference between these two books, the former being a text on the analysis of mathematical models of inventory systems and the latter being a monograph devoted to the analysis and application of a particular mathematical model of a particular inventory situation, it is nevertheless interesting to juxtapose them in a joint review. The theory of optimal inventory policies is of direct importance to the economic theory of the firm, as well as to the analysis of business-cycle models. The Hadley-Whitin text does not touch on these ramifications of inventory theory to economics, yet a study of this book is definitely recommended for economists with research interests associated with inventory theory. In particular, the model studied in the d'Alessandro monograph would have been considerably enriched had the author been conversant with the material in the Hadley-Whitin book.

There are two major dichotomies by which one can classify inventory models: (1) is demand a known (deterministic) or random (stochastic) quantity; and (2) is the model one of a single time period with a single inventory decision to be made (one-stage) or of many time periods with many inventory decisions to be made (multistage)? After devoting a chapter to deterministic models in which the demand for an item is a known constant λ per month and is independent of time, the Hadley-Whitin book deals in the remainder with one-stage and multistage stochastic inventory models.

The d'Alessandro monograph deals with a generalization of the deterministic situation discussed by Hadley and Whitin to the case where the demands for an item are either the known constant L per month in n known months and l per month in the remaining $12—n$ months. The mathematical machinery needed to deal with this model, as with the simpler deterministic model of Hadley and Whitin, is well within the ken of any economist. On the other hand, much more realistic deterministic models have been analyzed in the literature (cf., e.g., Arrow, Karlin, and Scarf, *Studies in the Mathematical Theory of Inventory and Production*). The mathematical maturity needed to deal with these models is beyond the requirements for the Hadley-Whitin text, and so the models are not included there. However, such a limitation should not have been a necessary self-imposition to a monograph; only the empirical validity of such a model, given the existence of more complex models, justifies its usage, and it is not clear from the three examples, production of cement, Christmas cakes, and silver articles, that demand has the property postulated by the model.

In fact, d'Alessandro treats the monthly average demand as a deterministic quantity, when in fact his datum is but a sample statistic which is only an estimate of the expected value of the (stochastic) monthly demand. It is in the exposition of stochastic-demand inventory models that the Hadley-Whitin book shines. After reviewing the necessary probability theory (in about 80 pages), Hadley and Whitin treat models in which the central problem is that of finding the optimal lot size to order at each inventory review period and, if the review period is also a variable to be determined, the optimal reorder point, both when the stochastic process governing the demand is in a steady state and when the average rate of demand changes with time.

In addition to being well written, the Hadley-Whitin text contains a wealth of exercises. This is to be commended, as the only way to absorb and understand a mathematical discipline is in the old-fashioned pedantic way of "doing the problems at the end of the chapter."

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RAND Corp.

Kostenvariabiliteit en variabele-kostencalculatie. (Direct Costing.) By R. SLOT. Leiden: Stenfert Kroese, 1962. Pp. 215. *f* 18.

Why should an issue in accounting practice be of any interest to economists? Because so much of the economist's data originates in the accounts of the firm; because the firm itself also bases many of its economic decisions on its accounts; and because issues in accounting are themselves necessarily economic issues.

Nonetheless, in the English-speaking world economists have paid scant attention to the literature and practices of accounting. Those among them who have tackled the British and American accounting literature have found that the bulk of it—insofar as it refers to the economic principles of the subject at all—is far too loosely written to be of any help. Those who have examined accounting practices have found an extreme range from almost meaningless conventionalism to admirable sophistication.

Many of the writers on accounting from Western Europe have also concerned themselves intensively with microeconomics, and thus one will find more stress on economic aspects and a more theoretical approach in their writings. Unfortunately, however, the theoretical approach has often led these writers into dogmatism, pointless polemics, and neglect of the practical (and consequently important economic) aspects of the issues.

The present writer for many years has meant to submit these thoughts to the consideration of his colleagues. The reader can therefore understand how pleased he was to find in a book he was asked to review much careful documentation and support for these views. Moreover, this book, which deals with cost variability and the so-called direct-costing controversy, marks a retreat from the excesses mentioned above. As far as the direct-cost controversy is concerned, it accomplishes a considerable clarification of the issues discussed in such a hit-and-miss fashion in the literature.

A brief statement of the controversy follows. The proponents of the (misnamed) direct-costing method of accounting differ somewhat in their proposals, but in essence they wish to charge the sales transactions—or category of transactions—of the firm during any one accounting period with the direct cost incurred in manufacturing and selling the items involved, but not to charge them with a share of the firm's fixed cost, as is done in the full- or absorption-costing method. The proponents argue that for much of the fixed-cost allocation the basis is necessarily arbitrary; that fixed costs are not relevant for pricing and production decisions;[1] that the profit and loss statements and break-even charts[2] are simpler and so much more easily understood by management when based on variable costs; and that under their system department heads are no longer bothered with figures on costs over which they have no control.

The author analyzes these claims and counterclaims. We shall restrict ourselves to mentioning two of the author's points. Much clarification is obtained from his point that the pros and cons should be considered as they would appear when the best version of either method is adopted, rather than some of the very imperfect versions found in practice or in the literature. Also important is his point that it is not evident (and not usually discussed) how the direct-costing method should treat variable costs which do not vary proportionately with period output (stagewise variable costs, degressively variable costs, etc.).

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[1] Perhaps too many works on microeconomics assert this in a dogmatic way. The decisions involved often relate to a planning period well in excess of the accounting period, so that many costs that are fixed from the accounting point of view are variable for the purpose of the decision. Moreover, a knowledge of fixed (replacement) costs can be of importance when figuring the position of competitors, particularly of potential competitors who have not yet sunk all these costs.

[2] In fact, as the author points out, if sales and production differ, there is not a unique break-even point; under full costing he speaks of a break-even or isoprofit *line*.

The author makes some attempt at economic analysis of the issues. Thus he notes that it is not incidental that advocates of direct costing often have in mind firms with sales which are stable but production which is variable over the year—and opponents with variable sales and stable production. He points to the lesser importance of knowing full costs for firms, not themselves price leaders, selling in a market with price leadership. One could have wished for a much fuller economic analysis to take the place of these and some additional valuable, but rather offhand, remarks.

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Industrial Organization; Government and Business; Industry Studies

Promotion and Control of Industry in Postwar France. By JOHN SHEAHAN. Cambridge: Harvard University Press, 1963. Pp. xiii, 301. $5.95.

The interpretation of growth can serve as a Rorschach test by which we can detect brands of political prejudice. For a *dirigiste,* planning has produced the Monnet miracle; for a liberal (to whom Monnet is the root of all evil) freedom underlies the Erhard miracle. Advocates of reserve armies see how in Italy and Germany it was reserves of manpower which speeded up growth, while those with a vested interest in labor scarcity show that in the early United States it was shortage of manpower that led to mechanized progress. Advocates of wage policies refer to Holland, opponents to Germany. Those who believe that inflation, buttressed by controls, stimulates growth point to the French revival of the early 'fifties, sound money men to Germany. Those who pin their faith on integration and size focus on the Common Market, those who distrust them exemplify by Japan, Austria, and the Six before 1957. Neo-Mercantilists, who swear by trade surpluses as the source of growth, select Germany, anti-Mercantilists illustrate with Japan.

Still, there is more than a blot and prejudice, and it is tempting to say that growth occurs not because of, but in spite of, planning, nationalization, free markets, inflation, sound money, unemployment, etc. There is the view that countries grow because they have been destroyed, the profound (and irrefutable) exponential view that countries grow because they grow, and (transposing the axes) the logarithmic view that they grow in spite of it. The best of both worlds can be combined in the logistic view.

Professor John Sheahan, in his highly readable book, offers no single or simple answer to the question: what were the causes of French growth? He is against unicausal explanations and in favor of narrowing down the investigation to the determinants of the choices of individual enterprises. His main points are that explanations have to be sought on the supply side rather than on the side of demand (though rapidly growing supply permits higher levels of demand relative to *existing* capacity, thus creating the illusion that demand creates its own supply); in attitudes and responses to new opportunities rather than in systems of ownership and control; in adaptability and flexibility rather than in central intervention; before reconstruction in activating supply responses and, after reconstruction, in the removal of obstacles.

Sheahan examines in some detail several characteristic manufacturing industries and their public and private promotion and control between 1950 and 1960 and contrasts these with practices in the United States. His discussion of French planning is suggestive, and his chapters on selected industries are excellent, but the transition from the evidence to the conclusions is elusive.

Together with perfumes and *haute couture,* French planning has become a major French export. But, as with perfumes and *haute couture,* the wrong model and the wrong context can spoil the effect. One is reminded of the series of recent advertisements extolling chewing gum which run under the title CERTAINLY NOT . . . and depict various situations in which no gentleman would use chewing gum. Sheahan concludes that, in spite of the tendency for the U.S. and French economies to converge, the French experience has only very limited relevance to the United States.

If the French mood of planning is indicative, then the Russian is imperative, and the English subjunctive. Any fully effective method must combine all three: it must be based on correct alternative hypothetical prognoses, must provide the required information and coordination, and must use rewards and penalties. Sheahan considers it a "danger of planning as practiced in France . . . that it sets up the possibility of a system of rewards and penalties ensuring adherence to the plan" (p. 170) and concludes his discussion of "indicative" planning by saying that it is one of its merits that the "Commission has not had the power to block individual firms which did wish to move ahead more rapidly than collective planning seemed to indicate as desirable" (p. 189). The Rorschach test shows up the American creed. This is confirmed when we find that Sheahan approvingly quotes Wellisz' argument that it is the function of planning to overcome distortions caused by the use of administrative price controls and by inflation. The dismissal of imperative planning and the belief that a justification of planning is its own negation (thus vindicating free enterprise) are not fully substantiated by the detailed and interesting industry studies in the book. Indeed, Sheahan himself stresses selective credit controls in other places in the book. During a conference organized jointly by the National Institute of Economic and Social Research and the Institut de Science Économique Appliquée in 1961, it became fairly obvious (not only to Rorschach test victims) that the threat of depriving firms of funds by not giving their issues official support can be an effective method of selective control, and that such threats formed an important part of French policy. It can, of course, be argued that there are two sectors in French industry: the private sector which is controlled and the public sector which is free. Quotations from Pierre Lefaucheux, the former head of Renault, seem to confirm this (p. 203). But it is not consistent with the evidence to believe that what Harry Johnson has called planning by bark is not supplemented by planning by bite: that the indicative is not supported by the imperative. The close ties between French businessmen and civil servants may reduce the applicability of the dangerous-dog metaphor, unless we add planning by sniffing. But is sweet suasion all? The trouble is that, in spite of the fact that the rate of growth of publications on Monnet planning has been even higher than that of French industrial production, we do not know what, if any, contribution "indicative" planning has made to growth.

Special emphasis is laid on the "human factor" in management. This is in line with C. P. Kindleberger's conclusion that "the quality of leaders—Massé in electricity and Armoud in railroads, along with Lefaucheux—was more important than state ownership."[1] It is also confirmed in the British context by Tibor Barna, whose ultimate reference is to Sheahan's study of the automobile industry.[2] But this type of explanation raises more questions than it answers. What differences in the system of education and training, in experience, and in the typical career pattern, what social values and what mechanism of selection of *élites* bring forward such men and enable them to exercise their powers in the jobs for which they are suited?

The book contains a number of other valuable and question-raising discussions, such as that of the relation between technological change and market structure in which François Perroux' appealing notion of *pôles de croissance,* as interpreted by W. E. G. Salter, is subjected to empirical tests. Sheahan neglects neither the great achievements of the Fourth Republic nor the possibility that planning may retard growth. He penetrates macroeconomic generalizations by offering case studies of a price-controlled duopoly, of balanced growth, of dispersion with protection, and of competition through nationalization. What is now needed is a theory of growth cycles. It is easier to explain cumulative growth once it is under way than the turning points in the growth rate; easier to say why those who grow fast can grow fast than to say why they begin or cease to do so. If growth is the main cause of growth, what is the cause of its beginning and end?

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Competition, Cartels and Their Regulation. Studies in Industrial Economics No. 4. Edited by JOHN PERRY MILLER. Amsterdam: North-Holland Publishing Co., 1962. Pp. 428. $9.80.

Most of this collection of essays is concerned with the historical and current legal status of monopoly and restrictive business practices in Western-type industrial economies. The topic is especially timely. Public constraints on private monopoly is a relevant issue only when, as a matter of national economic policy, reliance is placed on market forces to govern a significant portion of total economic activity. The shift from national socialism to greater reliance on free market forces in Western nations, notably Western Europe, has been evident for the past two decades. In consequence, the same nations have enacted legislation, or revived and strengthened old legislation, limiting the anticompetitive activities of private firms.

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[1] C. P. Kindleberger, "The Postwar Resurgence of the French Economy," in Stanley Hoffman et al., *In Search of France,* p. 155.

[2] Tibor Barna, *Investment and Growth Policies in British Industrial Firms,* New York, 1962, p. 62.

In his introductory essay Professor Miller presents in the best tradition of the political economist the case for a policy of competition. The historic claims for such a policy, according to Miller, are that it supports the broad social and political objectives of opportunity and freedom and that it offers the best assurance for steady economic growth. The validity of the first claim is obvious, but that of the second, as stated, is questionable. More accurately, the case for competitive market enterprise is that it assures a rate of economic growth more compatible with the broad objectives of opportunity and freedom; but unless one is prepared to ignore Schumpeter's analysis of this issue, there is little reason to conclude that the ensuing growth rate will be steadier than that of alternative forms of economic organization. Moreover, we cannot avoid wondering why Miller omitted altogether that highly prized goal of efficient resource allocation assured through the optimal marginal equivalences of competitive equilibrium. But while some will differ with Miller on what the historical arguments for competition might have been, he gets the volume off to an excellent start by posing the important questions. Unfortunately, through no lack of competence on the part of the contributors, the rest of the book is not addressed to these questions.

Joseph Spengler's essay on competition and monopoly in economic development is a notable exception. This penetrating and lucid analysis is sufficiently general in approach to be pertinent to any economy, irrespective of its present stage of development. On balance, Spengler finds the three major immediate determinants of economic development—technical progress, capital formation, and factor mobility—favorably affected by competitive enterprise. But Spengler defines competition broadly enough to include the temporary monopoly and unstable oligopoly often considered a necessary concomitant of a high rate of technical progress.

The chapters concerned with national and supranational policies toward monopolistic practices are informative and reflect the high level of competence of their respective authors. William Boserup's and Uffe Schlichtkrull's chapter provides a handy reference to the cartel legislation of European countries; its substantive content was undoubtedly enriched by Boserup's experience as consultant editor of the *Guide to Legislation on Restrictive Business Practices,* recently published by the Organization for European Economic Cooperation. Professor Thomas Wilson's chapter on restrictive practices, while leaning heavily on recent British experience, makes extensive use of the U.S. antitrust record and assesses the principles of public policy in both countries in the light of contemporary price theory. Professor Fritz Voight's chapter on the German experience covers in remarkable detail the activities and legal status of cartels in Germany throughout the century 1860-1960—from the tin plate cartel of 1862 to the activities of the Federal Cartel Office in its first three years of existence after its creation in January 1958. Miller traces out the history of antitrust in the United States with a deft hand, but his cautious conclusion that "the net effect of antitrust has probably been to increase the competitive pressures and reduce the extent of monopoly power even though in some cases encouraging concentration" will surely cause many to question what the 75-year continuing struggle from Senator Sherman to Senator Kefauver has all been about. Richard R. Holton's chapter on distribution and Alfred E. Kahn's chapter on the U.S. patent system round out the discussion of national policies. Holton attributes the lack of antitrust action against retailers and wholesalers to freedom of entry, and Kahn concludes that whatever may be the shortcomings of the patent system there are no clearly superior means for stimulating commercial innovation.

The two concluding chapters on the Coal and Steel Community and the Common Market by Richard Hamburger and Pierre Uri are welcone additions to the growing body of literature on Western Europe's economic reorganization. Coal and steel clearly are not the two most promising industries for testing out a newly designed policy of regulation through competitive market forces—in Europe or anywhere else. The main thrust of Hamburger's analysis is that if in the first six years of the new policy these industries have not been made effectively competitive by U.S. antitrust standards, they at least have been unshackled from their traditional cartel moorings. Uri's chapter makes available in a highly readable and abbreviated form much of what is contained in President Walter Hallstein's *United Europe: Challenge and Opportunity* (Cambridge: Harvard University Press, 1963). His conclusion that agreement on a common foreign policy stands as the principal obstacle to the political union (economic union having already been virtually attained) of Western Europe prophetically emphasized the later discord among the Six on the United Kingdom's admission to the Common Market and the treaty on atomic testing.

As individual craftsmen go about their appointed tasks in collections of essays, there is the risk that the central questions on which adhesion depends will be overlooked. It is regrettable that, in view of the exceptional competence of the editor and contributors, this risk was not anticipated and met. Why does there seem to be a revival in national policies fostering regulation by the competitive market mechanism? How can one explain the great differences among Western nations in policies toward market power, price-fixing agreements, and resale price maintenance? Did the European Economic Community seek integration through greater reliance on competitive market forces because of the historical arguments for competitive resource allocation or because centrally directed economies require powerful central governments, and none of the Six wished to proceed that far in relinquishing its own national prerogatives? A concluding chapter synthesizing the excellent parts, addressed to Miller's opening questions, would have greatly enhanced the value of the volume.

Less seriously, but no more understandably, why in a volume concerned with public policies toward competition are Canada, whose basic statute antedates that of the United States, and Japan, whose recently enacted statutes are modeled (with considerable modification in substance) after those of the United States, omitted? Surely the antimonopoly activities going on in

Ottawa and Tokyo are as important, and of as much interest, as those going on in Stockholm and Copenhagen.

In spite of these serious omissions, the editor and contributors are to be commended for their highly competent analysis and clear description of the rapidly developing public policies toward competitive regulation in the West. The only serious criticism is that they did not do more, which itself speaks highly of what they did.

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An Analysis of Urban Travel Demands. By WALTER Y. OI and PAUL W. SHULDINER. Evanston: Northwestern University Press, for the Transportation Center, Northwestern University, 1962. Pp. x, 281, $12.50.

With this book Walter Oi and Paul Shuldiner initiate a needed and promising collaboration between economists and engineers dealing with urban transportation problems. In recent years highway engineers and city planners have been designing transportation systems under an increasing barrage of criticism from economists standing behind the curb lines. Three targets in the engineer's view of the world have drawn their fire: his tendency to focus on technological characteristics of transportation systems at the expense of their role in the economic and social life of the city, his "requirements" approach to transportation investment decisions, and his failure to conceive of transportation users in behavioral terms. Oi and Shuldiner join forces to attack this last problem, and in doing so demonstrate again the rewards—and the difficulties—of bringing the economist's and the engineer's views of the world into common focus.

The main thrust of the book is the observation that urban transportation users behave like consumers, a fact which has a great deal to do with the performance of urban transportation systems, and which, hence, should be a critical input in planning and decision-making for future transportation services. The state of the art of transportation planning can be vastly improved, they contend, by developing transportation planning procedures based on consumer behavior in lieu of the current practice of using small geographic areas as the units of analysis, and areal averages of household, land use, and other characteristics as variables. Methodological reforms, as well as a greater respect for theory and causation, will need to accompany the coupling of projections more firmly to analytical procedures. In this sort of discourse Oi and Shuldiner perform well. They are on intimate terms with their subject, and their suggestions for such reforms in the study procedures employed in urban transportation planning as extending household interviews over time in place of pure cross section illustrate how competent scholarship and experience researchers can improve the analyses that underlie the decision-making processes of public agencies.

Notwithstanding these virtues, the book leaves the reader curiously unsatisfied. The authors were clearly ambiguous (or unable to agree) about their audience: economists will find some of the conceptual explanations irritatingly elementary, and the interested laity will be frustrated by inadequate methodological discussions. The transportation planner will find the extensive review of the development of origin and destination studies uninformative.

In addition, the several parts of the book never seem to get properly tied together. Three reasonably independent reports of empirical research are preceded by three loosely coupled essays characterizing the general state of urban transportation-planning analysis, setting out theoretical aspects of the consumption behavior of the urban traveler, and detailing the development of the methodology of origin-destination studies. Of these, only the discussion of consumer behavior will tell the transportation planner anything new, and even this fails to provide, as it should, a framework on which to hang the research reports which are the authors' real business.

The three research reports that make up Chapters 4 through 6 should command the attention of urban transportation experts and city planners alike for the rigor with which the authors treat their data and carry out their analyses. Their conclusions are rarely startling, always respectable.

In Chapter 4 the authors seek to identify the relative importance of the factors generating the aggregate volume of urban travel by persons. Using individual household data available from origin-destination studies conducted in Detroit and Modesto, they demonstrated the dominant role of household-size and auto-ownership variables in the volume of household trip-making.

The total volume of travel is disaggregated by trip purpose and mode of travel in Chapter 5. This is probably the least satisfying of the three research chapters, simply because neither Modesto nor Detroit is sufficiently endowed with public transportation facilities to make the conclusions about choice of mode very persuasive.

Modesto and Detroit are left behind in Chapter 6, where transportation expenditures in the family budget are analyzed with the help of consumer-expenditure data. Some income elasticities for transportation expenditures are tentatively identified and discussed, especially among cities. The research is solidly reported and adds some new dimensions to the state of knowledge about the consumption of urban transportation services. Unfortunately little effort was made to relate this report to its companions or to the loose theoretical discussion earlier.

The final chapter, which could have drawn together all these threads into a new perspective about urban transportation analysis, fails to be anything more than a summary recounting of the findings of the research reports. Clearly the findings are of interest to all who have some involvement in urban transportation issues generally, but they might have shown up more effectively as research reports, conference papers, or journal articles. The "breakthrough" book in urban transportation is yet to be written—laying out a new framework of analysis which will (1) relate the urban transportation system more intimately to the social and economic systems it serves, and (2) integrate decision-making about transportation systems more effectively with the context of metropolitan development policies. Oi and Shuldiner have made a modest beginning in their book; we will have to wait for a critical review of the experience from the later generation of urban transportation studies now underway before the larger advance can be made.

LOWDON WINGO, JR.

Resources for the Future, Inc.

The Economics of the Postal Service. By MORTON S. BARATZ. Washington: Public Affairs Press, 1962. Pp. vi, 104. Paper, $3.25.

As the author indicates in his introduction, the post office, the nation's oldest and largest public enterprise, has received surprisingly little attention from professional economists, despite the obvious significance of the economic services it renders. In this interesting little book Dr. Baratz had done much to remedy the deficiency.

The author has wisely refrained from burying the reader in a mass of detail about organization, administration, and physical aspects of the postal operation except as they bear directly on the elements of economic performance. He has therefore been able to compress into one hundred pages a useful discussion of the basic economics of the postal operation.

The organization of the book is straightforward and logical. A discussion of income and price elasticity of demand for postal services is followed by a series of chapters dealing with the rate structure and several alternative explanations of its character; the structure and behavior of costs in both short and long run; interclass cost differentials; and the major issues for public policy raised by an appraisal of performance in economic terms.

It is hardly surprising that Baratz finds a number of anomalies and contradictions in the development of postal rate structures and in public discussion of the ends to be served by postal price policy. What is different, of course, is the limited but definitely useful degree to which he has quantified cost and rate differentials and thus buttressed some conclusions of general applicability as to appropriate pricing policies.

The general tone of the book, although analytical, is neither formal nor rigorous in the academic sense. The techniques employed do not go beyond a simple static marginal formulation, and no serious attempt is made to link the postal service with other elements of national economic development. The book should be well within the capabilities of the average undergraduate student and would make an excellent supplement to readings in the field of industrial organization and price policy.

For the professional economist and informed layman the book raises and deals quite adequately with the significant policy issues underlying controversies over postal service and postal rates in recent years. The most important of these relate to the interclass rate structure, which—to the extent that it reflects any rational economic calculus—must rest on the mistaken assumption that the marginal cost of handling second and third class mail is very low. Baratz' alternative proposals with respect to relative prices for postal services are thorough and well reasoned. He is probably justified in his conclusion that the aggregate price for postal services—i.e., the problem of the size of the aggregate postal subsidy (if any) to cover the costs of nonvendible postal benefits (if any)—is far less important. One wonders, however, if economic analysis leads only to his view that ". . . the 'correct' subsidy is the sum decided upon by a majority of Congressmen, each acting upon his personal preference . . . any rule of thumb the decision makers choose to use in arriving at the amount to be appropriated in the name of national welfare is as defensible as any other."

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Energie in Westeuropa. By HERBERT F. MUELLER in association with Niels Grosse. Munich and Karlsruhe; Technischer Verlag H. Resch for Twentieth Century Fund, 1961. Pp. xi, 235.

Energie et expansion économique. Evolution 1950-1961 et prévisions pour 1965 et 1970 dans la Communauté Européenne. By VITTORIO PARETTI and GILBERT BLOCH. E. N. I. publication no. 19. Milan: Guiffrè, 1963. Pp. 258.

Untersuchung über die Entwicklung der gegenwärtigen und zukünftigen Struktur von Angebot und Nachfrage in der Energiewirtschaft der Bundesrepublik unter besonderer Berücksichtigung des Steinkohlenbergbaus. Berlin: Duncker & Humblot, 1962, Pp. 300.

The fundamental issue underlying the analyses in all three books is whether Western Europe can retain its position among the leading industrial and commercial powers of the world and yet have its economic machinery run predominantly on imported fuels. Evidently this predicament, loaded with political overtones, is not limited to fuels and Western Europe. Even in our own, a continent-embracing, economy the problem gave rise to a rather apprehensive evaluation of nationally available resources in the well-known Paley Report [4]. And although a more reassuring perspective for our future economic growth has now been offered in a study by Landsberg-Fishman-Fisher [2], it is hardly surprising that in Europe, where the nation-splitting process produced some unfortunate economic consequences, serious efforts have recently been made to view the problem within a new, more or less supranational, framework of reference.

Of course, the three above-mentioned books concentrate on a much narrower predicament, that is, the adequacy of European energy supplies. In this connection, Professor Mueller's book seems to be the best stepping stone to an understanding of the issues involved.

A prolific writer in the field of energy, Mueller, of the Karlsruhe Technological Institute, undertook his study in association with Niels Grosse within the program of a survey of European resources conducted by the Twentieth Century Fund [1]. The inquiry covers Western Europe except Yugoslavia, and is divided into two parts. The first, chiefly theoretical, centers on a discussion of essential concepts in energy economics, in addition to an examination of over-all economic and energy conditions as well as reserves in Western Europe. A foundation for this, in the second portion of the book, consists of analyses of individual states. They are grouped in accordance with their energy-flow characteristics in four categories: (1) classic coal countries; (2) typical hydropower-oriented countries; (3) industrialized states with a varied energy-supply base; and (4) a somewhat heterogeneous group of remaining states.

Introduced by a brief review of each particular economic structure, the country chapters analyze quantitatively the past patterns of production and consumption of different kinds of energy, with special attention to the role imports and liquid fuels play in individual national economies. In this respect, the author presents some valuable data on countries like Finland or Spain whose composite energy-flows are not often readily available. Unfortunately, tables throughout the book lack any indication of the sources from which they were derived.

Regarding conclusions, Mueller refuses to engage in quantification of Europe's future energy demand, chiefly on account of insufficient data and a great number of imponderables. (His criticism in this respect of Professor Robinson's projections [5] brings out quite a few valid points.) Nevertheless, he anticipates a great increase in energy consumption in Western Europe, presuming a continuing economic growth and incessant demand for higher living standards. He does not, however, believe that this should be the reason for permitting free and unlimited imports of fuels, chiefly petroleum, into Europe. As he observes, a number of unfavorable ramifications of such policy on the balance of payments, employment, security of supplies, and prevention of loss of coal mines through flooding, etc., would militate against its full acceptance.

These factors, *mutatis mutandis,* are also considered by Dr. Paretti and Dr. Bloch, of the European Communities Statistical Office, of which the former is the Director. For instance, they stress that energy net imports resulted, on an average, in an annual deficit of $1.5 billion during 1958 to 1960 and could have presented a serious threat to the financial equilibrium of the Community but for the otherwise favorable balance of payments.

On the whole, however, the Paretti-Bloch book is basically different from Mueller's because these authors investigate historical statistics mainly for the purpose of finding quantitative measurements of future energy patterns. In addition, their inquiry has a much narrower geographical scope, as it is limited to the European Economic Community. Moreover, it is based on a breakdown of energy-flows by different sectors of the economy and types of energy, of which a composite picture is then presented on the level of the entire Community, rather than on the breakdown by component states.

The authors commence with a short review of elements which significantly influenced energy-flows in the Community during the past decade, continuing with a detailed statistical analysis of energy supplies, the share provided by the Community energy-producing industry and by imports, respectively. Then follows an inquiry in depth into the evolution of energy final consumption by individual economic sectors. Results are presented in a series of energy balance sheets (in Terakilocalories) depicting the Community flow of available supplies, their transformation, and end uses.

This is the basis for projecting energy requirements of the Community for the next decade. Using a modified input-output model, they derive linear and exponential correlation coefficients between the evolution of consumption of each type of energy in a determinant economic sector and the output of this sector. Results are checked against energy global requirements of each particular economic sector and Community total correlated to a projected economic growth.

The outcome of this inquiry strikingly illustrates the significance for Western Europe of the problem posited at the beginning of this review. As Paretti and Bloch point out, in 1950 the Community's indigenous supplies covered more than 86 per cent of total primary fuel consumption but ten years later only 64 per cent. In another ten years, the authors estimate, should economic activities develop as projected, domestic energy resources will not be able to satisfy more than 45 per cent of total primary energy demand. This means that out of the Community's projected total primary fuel requirements of approximately 650 million tons of hard-coal equivalent, about 370 million tons, mostly petroleum, will have to be imported by 1970. Clearly, the anticipated economic growth in the six states of the Community during the decade of the 'sixties will depend on imported fuels. By then, as the authors anticipate, the substitution of coal by petroleum will have leveled off and, in the long run, coal prospects are expected to improve. Yet, it is only a massive deployment of atomic energy which, in their view, can save Europe from the unfortunate repercussions which fuel imports on such a scale would exercise on political security, the balance of payments, and the entire economic growth of Europe.

It should not go unmentioned that the book has a statistical appendix summarizing the most important aspects of a prior historical-statistical study by the authors [3]. It is supplemented with recent figures and charts showing projections for 1965 and 1970.

What the new pattern in energy consumption may mean for a country like West Germany with a still predominantly coal-oriented economy is examined in the third book. It is a study undertaken by a Working Group of German Institutes for Economic Research in execution of a resolution passed by the Federal Parliament in the wake of mounting coal surplus in 1959. The Group, headed by Professors F. Friedensburg of Berlin and F. Baade of Kiel, directed its attention, in accordance with the parliamentary directive, primarily to the economic impact which substitution of domestic coal by foreign coal and petroleum will have in Germany by 1975.

In order to arrive at a demand pattern at this date, the Group employed a sector analysis of the end-uses of energy as a base for estimating future global demand for primary resources. This approach makes possible a somewhat more definite delineation of the areas in which characteristics of production functions and anticipated changes in prices can facilitate mutual substitution of fuels. Ultimately, after taking into account assumptions about other economic indicators the Group estimated the 1975 German requirements for primary energy in the neighborhood of 320 million tons of hard coal equivalent (h.c.e.).

The subsequent chapters analyze in great detail, and with copious statistical data, the supply side and market conditions under which the future requirements can be considered assured. In this context, the authors have also examined Western Germany's potential competitors, chiefly the United States and Soviet-bloc coal industries. Assuming unsubstitutable petroleum imports on the order of 85 million tons (121 million tons h.c.e.) in 1975, the issue was then narrowed to a question as to what degree *German* coal can hope to furnish all, or at least a major part, of an anticipated future demand for coal. To answer it, the Working Group offered three alternative assumptions: (1) free and open energy market; (2) a DM 20-tariff per ton of imported coal and an excise tax of DM 10 and DM 20 on fuel oils; (3) coal tariff of DM 30, while excise tax would remain the same as in alternative (2).

The resulting picture is far from cheerful to the German hard-coal industry whose production costs would make only an 85-million ton output of coal competitive with foreign fuels under the first alternative. In effect, if the present annual level of coal production—between 120 to 140 million tons—is to be maintained, and that would also mean that most of the future demand for coal could be supplied domestically, only the third alternative would do. Acceptance of the first would further entail a drop in employment of underground workers by approximately 200,000 (from 300,000 in 1960) by 1975.

Of course, the Group's report represents merely an advisory opinion spelling out economic consequences of alternative decisions. It is the German government which now faces the embarrassment of choice.

Considered together, these three publications offer an excellent research instrument into Western Europe's energy problems on three different geographic, economic, and political levels. The German Report has also energy-flow tables for the entire decade of the 'fifties and an instructive analysis of the situation in Eastern Germany with some additional notes on energy, legal, and institutional arrangements in Western Europe. It is, however, regrettable that none of these publications has a list of tables (the three books together contain several hundreds of them), and not even a rudimentary index. The tables are not numbered in the German Report and often lack any heading. Some, as on page 251, are particularly inappropriate.

Despite these shortcomings, a thorough study of the three volumes is recommended. Solid and concise in their factual presentations, they should facilitate—to paraphrase Jean Monnet's words from the Preface to the Paretti-Bloch book—defining and applying energy policy within the framework of a new Europe.

JAY G. POLACH

Resources for the Future, Inc.
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Highway Transportation Management. By GAYTON K. GERMANE, NICHOLAS A. GLASKOWSKY, JR., AND J. L. HESKETT. New York: McGraw-Hill Book Co., 1963. Pp. xiv, 469. $9.50.

This excellent book states that its purpose is to aid in the development of the student's ability to analyze problems and arrive at well-reasoned decisions in the economic regulatory and competitive environment in which the highway transportation industry is operated. There is little doubt that this laudable goal has largely been achieved. The authors have divided the book into functional management areas, viz., organization of the firm, operations and equipment, traffic and sales, advertising and public relations, motor carrier policy formulation, and public policy. These eight chapters are preceded by an introductory chapter setting forth briefly the scope and economic character of the motor carrier industry.

The authors have done an outstanding job of blending text and cases. The 39 cases are distributed unevenly among the functional areas. Chapter 8 is especially interesting and unique, since it covers a hypothetical case of long-range carrier policy involving many facets of the motor carrier industry. The book is intended to be used as a text in motor carrier management courses for advanced students who have a basic knowledge of transportation economics. It would seem that a rather substantial background in the institutional aspects of the motor carrier industry would be most helpful. The introductory chapter is somewhat brief and covers only the bare outline of the industry. Each chapter devoted to the functional areas is preceded by several pages of text material introducing the chapter and discussing the importance and scope of the problem area involved. The instructor thus has an excellent vehicle for introducing refinements, driving home points, and pointing out pitfalls in the student's analysis of the problem.

An especially valuable portion of the book is found in the section on public policy. The authors clearly recognize the important role of public regulation in the motor carrier industry and devote considerable space to issues in this area. At the same time, the authors have endeavored to put due emphasis on the broad aspects of management and to discuss at length issues of general business policy and operations. This serves to remind the student that the motor carrier industry is after all a profit-making business. While this may seem to be all too obvious, it is likely that students are often overly impressed with the idea that transportation is unique, and undue stress is placed on the public utility aspects of the industry. While this theme is woven into the text throughout, it is especially strong in Chapters 8 and 9.

Most case-type books suffer from the fact that they are necessarily somewhat compartmentalized, and this one is no exception, although the authors have made generally successful efforts to unify the contents. However, this is essentially the job of the instructor, who can select the cases and arrange the material so as to fit into the framework of his choosing. The variety of cases is such that in the normal course of a quarter or semester the instructor could choose those of special interest or significance, blending them with such background material, outside reading, and projects as might be desirable.

In all, this book makes a valuable contribution to the body of analytical teaching material in the field of transportation economics and could be used with profit by those interested in problems of general management as well as those more directly concerned with transportation.

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Land Economics; Agricultural Economics; Economic Geography; Housing

Resources in America's Future: Patterns of Requirements and Availabilities, 1960-2000. By Hans H. Landsberg, Leonard L. Fischman, and Joseph L. Fisher. Baltimore: The John Hopkins Press for Resources for the Future, Inc., 1963. Pp. xx, 1017. $15.00.

This is a massive book of more than 1,000 pages with 107 text tables and 116 figures, plus 293 appendix tables. It is a book that deals in an authoritative and systematic manner with an important set of national problems. In the treatment of future requirements and availabilities, the scope of the endeavor has been kept within reasonable bounds. The following quotation is a clear statement of what has and what has not been attempted:

> To predict what will happen in the next forty years is a feat beyond the powers of social science. Our object here is to evaluate the consequences of the most realistic assumptions we can make on the basis of past history, current trends, and reasonable expectations of new discoveries, substitutions, more intensive use of existing materials, imports, new patterns of multiple use, and other such developments. Projections from such assumptions will show the supply problems either of scarcity or of

> surplus that would be likely to emerge in the absence of changes in relative price trends or corrective policy action. We have deliberately not allowed for further substitutions or other adjustments that would occur if a resource product became scarce. To do so would obscure the very situations we are trying to uncover. Throughout the study, therefore, we have assumed that there will be no significant changes in present price relationships (pp. 17-18).

The projections of requirements were made in a rather simple manner. Population projections based on recent Bureau of Census projections were used to derive labor force numbers for 1980 and 2000. The projected labor force is then multiplied by a projected GNP per worker, which is assumed to increase at an annual rate of 1¾ to 2½ per cent with 2 per cent considered as the most likely. The resulting projection of GNP for 2000 ranged from 1,680 to 3,290 billion 1960 dollars. The growth rate of GNP was projected at a low of 3 per cent per annum and a high of 4¾ per cent with the medium rate at about 3¾ per cent. Based on past trends of the disposition of GNP, estimated GNP demand components by decade intervals through 2000 were derived. The usual GNP demand components were used—personal consumption expenditure, gross private domestic investment, and government expenditures, the latter being separated into defense and nondefense expenditures. Further breakdowns were then made for various components of output and expenditures.

The distribution of consumption expenditures "are basically 'judgment' extrapolations of past trends" (p. 513) with modifications for certain possible changes in habits, tastes, and availability of new products. Basic reliance in consumption estimates appear to have been placed upon past percentage distributions of personal consumption expenditures. Any effect that changes in relative prices may have had upon the allocation of consumer expenditures is thus ignored. Since the assumption has been made that the projections were based upon present price relationships, it is somewhat surprising that no effort was made to determine what effect past price changes may have had upon the disposition of consumer income.

While it is undoubtedly true that projections of future consumption of food or housing 40 years hence based upon estimated demand functions would have been subject to very large errors, some attention to the magnitudes of income elasticities of demand would probably have prevented making projections that implied an income elasticity of demand for food of unity for the high projection. However, the medium projection that implies an income elasticity for food and beverages at retail of about a third does not seem unreasonable. Nor could one argue about the low projection for food and beverages at retail which implies an income elasticity of demand of about an eighth.

Following the projections of the major end uses of GNP, projections were made of the demand for key materials—crops, lumber and woodpulp, water, mineral fuel, metals, and chemicals. The final part of the text relates the demand for key materials for the basic resources (land, water, energy, and nonfuel minerals) to the quantities of resources that are likely to be available in the future. The general conclusion, based on medium projections, is that the only major resource limitations are likely to be in forest products; institutional arrangements affecting the use of water may well have to be modified if most urgent demands for that resource are to be met. "In our long look ahead at demand and supply of resources we do not see any general running out; instead we see the prospect of sustained economic growth supported by an adequacy of resource materials, *provided* technological advances and economic adaptation of them continue, provided foreign sources of raw material remain open . . ., and provided government resource and private management of resource enterprises improve in farsightedness, flexibility, and consistency" (p. 53).

What value can one place on projections of the type presented in this book? I feel that the general conclusion, quoted in the preceding paragraph, is an important result and that the evidence brought together by the authors is clearly consistent with the conclusion. One could argue that there is little reason why economists should not come to this conclusion, and that so large an expenditure of effort and care was not required to arrive at such a conclusion. This is on the ground that the contribution of natural resources to GNP is only a small fraction—certainly less than 2 per cent—of GNP and that, while fixed coefficients of production may be an important short-run phenomenon, this is not true of the long run. But I feel that this is too narrow a view of what the authors have accomplished. Their detailed examination of numerous resources and commodities results in a substantial amount of very useful information for anyone who is interested in natural resources. Furthermore, in part because of their method of presentation and in part because of their conclusions, I believe that this book can have a useful influence upon resource policy.

I do feel that the projections for 1990 and 2000 are of limited value. The uncertainties are so great that little reliance can be given to specific results. By emphasizing the widening range of the estimated resource use as the future period becomes more distant, the authors do provide a warning, but I am confident that most users of their material will emphasize the results for the later years rather than the estimates for the two earlier decades.

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Scarcity and Growth: The Economics of Natural Resource Availability. By HAROLD J. BARNETT AND CHANDLER MORSE. Baltimore: The Johns Hopkins Press, for Resources for the Future, Inc., 1963. Pp. xv, 288. $5.50.

Barnett and Morse join the contemporary attack on traditional economic analysis and its related value judgments regarding natural resource scarcity and diminishing returns, with particular reference to advanced economies. In the context of a focus on what they view as the economist's mistaken belief in natural resource scarcity and its aggravation through time, they also present a critique of conservationism. Although they further purport to treat such scarcity relative to levels of living, the authors exclude the problems of population control from the scope of their study (p. 49).

The writers subscribe to the now-accepted notion that natural resources "can be defined only in terms of known technology" (p. 7). They therefore share the viewpoint of Marx and most contemporary economists, as paraphrased by Irma Adelman, that "any resource can be improved or degraded, and thus the entire material wealth of the community is more or less exhaustible or reproducible or both."[1] *Scarcity and Growth* emphasizes that economic accessibility of so-called natural resources, rather than location or ownership, is what counts in a country's development.

In a work apparently addressed to the professional, the major contribution is a refreshing insistence upon technological change as central to man's long-run relation to his "natural" environment. Furthermore, such change is characterized by increasing returns (p. 236). The definition of resources in terms of changing production technology and the development of raw material substitutes suggests the obsolescence of the concept "natural," the irrelevance in the modern long run of the additional classical concept of a resource in fixed supply, and the unacceptability of Ricardo's "original and indestructible powers of the soil."

Barnett and Morse would agree that the most interesting economic aspect of "land" is, as pointed out by R. H. Tawney with reference to China, that it "is an article manufactured by man."[2] They reiterate at several points (e.g., p. 55) essentially the same proposition as that of Robert R. Doane, that "the general disposition of many economists to think of land as not producible is largely due to the fact that it has been regarded more from a physical than an economic point of view."[3]

Following a summary statement of their major hypotheses, the authors present an informative review, subsequently elaborated and appraised, of classical natural resource scarcity doctrines. They generally reject these. "The classical economists saw the process of growth as subject to limitations, and we agree. But they saw the limitations as residing in nature, and we see them as residing more in man" (p. 262). However, what they term J. S. Mill's "suggestive insights" depreciating scarcity effects (diminishing returns), and particularly Mill's anxiety over the adverse side effects of man's conquest of nature upon the "quality of life," are most sympathetically developed. Although their doctrinal review does not purport to be comprehensive, it is unfortunate the authors miss their opportunity either to chide Malthus for relegating diminishing returns to the background in his *Principles,* as distinguished from the *First Essay,* or to stress the significant point that, for Mill (as for Ricardo, in the absence of the Corn Laws), England had access to "the soil of the whole world." Also, despite some references to time, Barnett and Morse do not properly highlight the extent to which the controversy over diminishing returns, in the course of its checkered career, has revolved around differing assumptions about time (e.g., p. 246). The writers seem somewhat insensitive to the fact that what has been thought imminent, and what ultimate, has usually been crucial to the argument. (They belong to the group relegating diminishing returns to the most remote future.) Furthermore, for those of us interested in the affiliated matter of *why* assumptions employing the fixity of nature, diminishing returns, constant technology and all that have held economists so long in static thrall, there is surprisingly little to be found in a book generally permeated with a mission toward needed changes in people's minds and in welfare policies.

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[1] *Theories of Economic Growth and Development,* Stanford 1961, p. 138.

[2] Cited in W. W. Lockwood, *The Economic Development of Japan,* Princeton 1954, p. 241.

[3] *The Anatomy of American Wealth,* New York 1940, p. 117. Cited in *ibid.,* p. 242.

Conservationism is next surveyed, appropriately as reflecting a conflict between "a rapacious industrialism and a parsimonious nature" (p. 238). Oddly enough, conservationism is not represented as a component of the Progressive movement or as next of kin to muckraking. The authors recognize that the conservationists were practical romanticists, lacking in rigorous economic analysis yet sharing the economist's tendency mistakenly to identify economic natural resource scarcity with given physical resources (pp. 75-76). Conservationism is in general derogated in this volume.

The conservationists' anxiety over man's disruption of ecological balance, including wasteful use of nature's renewable endowments, pollution and other damage to the environment, receives what to me seems a rather cavalier treatment. One gets the impression that we can count on technological advance to such a degree that soil replenishment or sustained-yield forestry are at best unimportant and at worst inhibitive of innovation. Wasteful extraction methods and failure to replace are made light of by the authors' key principle that higher extraction rates today may be expected to induce higher rates of research and investment, thus transmitting an accordingly greater legacy of economically valuable assets to subsequent generations (pp. 247-48). This principle might be called Barnett and Morse's invisible hand. In their words, "by devoting itself to improving the lot of the living . . . each generation, whether recognizing a future-oriented obligation to do so or not, transmits a more productive world to those who follow" (p. 249).

The heart of the primarily empirical section of the book (pp. 151-232) shows that in the U.S. case innovations have forestalled natural resource scarcity. This material is useful for both economic theorists and economic historians. It is shown that not only did the real unit costs of almost all extractive products (forestry is the notable exception throughout) decline absolutely from 1870 to 1957, but such costs and also unit prices declined, or failed to rise, relatively to nonextractive goods. On the relative price test, agriculture revealed an upward drift until World War I and a downward drift thereafter. It is also noted that the ratio of gross extractive production to gross total production has fallen dramatically, until it is currently about 13 per cent of GNP. The doctrine of natural resource scarcity and its alleged productivity effects are therefore inconsistent with statistical evidence.

Scarcity and Growth is excellent monographic work on a subject of pervasive importance to the economy and to theory. It synthesizes theory and empirical evidence in an analysis of generally high quality.

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Labor Economics

Unemployment and Structural Change. Geneva: International Labour Office, 1962. Pp. v., 206. $2.00.

This anonymous work is a most welcome addition to the thin list of comparative studies of labor market adjustments necessitated by rapid changes in technology and markets. The first of two parts contains an evaluation of a number of specialized studies on the employment effects of structural changes in firms, industries, localities, and regions of Western industrialized countries. Apart from the value of bringing together the results of these studies, this section is a well-organized, professional treatment of the major economic and social factors that should be considered in analyzing the impact of structural change. Although limited to the nonagricultural and commodity-producing sectors of industrialized economies, the discussion will be of value not only to those interested in developed economies but also to those concerned with labor market adjustments in the emerging economies.

The second part of the book analyzes both private and governmental measures to deal with problems arising from structural change. The topics covered are employer adjustments to declining demand, the several types of labor mobility as an adjustment process, "redundancy" procedures (with or without collective bargaining), national policies for growth and high levels of employment, measures to assist industries in difficulty, programs to combat regional unemployment, and devices to assist workers directly. The last named covers unemployment compensation, public works, retraining, and resettlement allowances.

In addition to these two major sections, an appendix contains the report of a new case study that details the decline of the two-wheeled motor vehicle industry in West Germany and the effects of a factory shutdown in that industry. Of particular interest is that even with unusually favorable circumstances—hiring and retraining of many of the workers by another firm and a generally tight labor market—long-term unemployment was experienced by many. Of those over 50 years old not taken over by the new firm, 22 per cent were unemployed more than six months. Among those under 50, even though only 2 per cent had more than six months out of work, 10 per cent were jobless between 7 and 26 weeks.

The book's first part is more successful, on the whole, than the second. The reason is that an effective description of adjustment problems requires neither a complete coverage of all relevant studies nor a comprehensive listing of specific problems in every country. Although some important studies have been overlooked, selective data and conclusions from many studies have been woven into a first-rate essay on the economics of employment problems that arise from changes in demand and in technologies. Technical problems, such as the measurement of "structural unemployment," are avoided, as are highly theoretical formulations of wage-price-employment relationships. Instead, the emphasis is on the process by which structural changes are translated into unemployment and on the classification of associated problems.

The second section is less satisfactory in that an adequate assessment of the policies and programs on worker displacement in Western countries requires rather complete detail. What is provided is essentially a lengthy 133-page introduction to the subject. Several of the topics could and probably will be the subjects of entire books. For example, a comparative area redevelopment study (Western Europe and the United States) is currently being completed and will undoubtedly result in a book-length report.

Although not as comprehensive as it might be, the analysis of public and private efforts to reduce structural unemployment and to relieve the burdens of such unemployment does touch upon most of the policies and experiments of recent years. The discussion of general economic policies, however, is decidedly brief—only eight pages—and perhaps underemphasizes the importance of aggregate demand as a solution to the problems of structural unemployment. For example, the statement is made that "policies designed to maintain sufficient aggregate demand . . . by no means prevent structural unemployment" (p. 130). Such policies do prevent much structural unemployment that otherwise would occur and might well be given more emphasis as the major defense.

The book, however, is primarily devoted to direct measures to help workers who are economically harmed by structural change. In reviewing such measures, the economics of alternative approaches is considered but, as stated in the introduction, economic justification is not the sole criterion for social policy: "A case for some special assistance to groups adversely affected by structural change can be made out on grounds of social justice" (p. 3). If this point of view is accepted, aid to those who are forced to make involuntary sacrifices may be considered part of the price of progress.

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Salaries in the Public Services in England and Wales. By HILDA R. KAHN. London: Allen and Unwin, 1962. Pp. 428. 60s.

Professor Kahn's book is divided into two parts. The first of these involves over half of the work and is devoted to presenting cross-sectional data of the English public service salary structure. Seventeen services, including the nationalized coal, electrical, and gas industries, are covered. For each service, the boundaries of salaried employment are delimited, and tables are provided showing the relevant salary and employment structures as of the early 1950's. In addition, Dr. Kahn provides a terse historical and descriptive discussion of the administrative and negotiating structure involved. For practitioners and researchers alike, Part I should provide a useful and informative guide through the Parkinsonian reaches of English salaried employment.

In Chapters 18 to 21, which lead off and finish the four-chapter second part of the volume, the focus of attention is "The Element of Accident" in the National Service salary structure. The topic was of concern to Dr. Kahn in a 1956 work published in the *Manchester School of Economics and Social Studies*. Her utilization of the raw material of Part I gives much support to her pleas for a streamlining of the interservice and intraservice salary schedules. It is surely a worthy imagination which keeps separate 250 grades of public health nurses; that is not to mention the 500-odd rungs on the technical engineer's ladder in the electrical supply industry, or the 1400 salary grades studied by the Civil Service Research Unit. In tracing the existence of these "convolutions," Dr. Kahn's work is careful and knowing.

The consideration of a national wages policy proves somewhat less interesting. As she recognizes, cross-sectional tools ill fit the essentially dynamic problems of inflation, productivity, and growth. With a job-by-job re-evaluation of the public services in mind, she therefore concentrates on "the internal problems of pay determination" that confront economic planners. Her insights into the cumbersome structure that she faced could undoubtedly have suggested numerous ways and means towards consolidation. These thoughts, however, are not shared with the reader. Rather, he is asked again to consider the numerous ways in which a purely economic equilibrium is jostled by "interferences" and to consider which of the "non-economic interferences" such as "need" and "fairness" should most concern planners bent on reorganization of the salary structures. While in the last analysis such factors will leave their mark, they appear to be rather ethereal matters in the present context of antiquated and institutionalized mistakes.

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Automation Funds and Displaced Workers. By THOMAS KENNEDY. Boston: Graduate School of Business Administration, Harvard University, 1962. Pp. 374. $5.00.

The workers displaced in recent years by the giant, seven-leagued strides of automation and technological progress are in an especially unfortunate position. In a scholarly and dispassionate study, Thomas Kennedy, of Harvard University's Graduate School of Business Administration, discusses the use of automation funds as a proposed remedy for the plight of the ever-increasing number of displaced workers. Perusing this interesting volume, the reader becomes painfully aware of the ineffectiveness of the steps taken thus far by management, labor, and government to meet this problem.

What is an automation fund? The author defines it as a device "(1) which is established by management and labor, (2) to which money is contributed because of changes in equipment, methods, or plants which result in greater output per man-hour, and (3) for which money is disbursed for the purposes of sharing the gains of such changes with the retained employees and/or providing for the equities and needs of displaced employees and/or for studying the best means of accomplishing these purposes" (pp. 3-4).

The author studies the automation-fund device to determine: its value as a means of providing for the needs of displaced workers and sharing the savings of automation with retained employees; its possible effect on the expansion of automation; the major operational problems, and the success or failure of the various methods used in meeting these problems. That the problems studied are difficult and of extreme complexity is apparent and readily understandable. That they are not insoluble is demonstrated in interesting fashion by the author.

The heart of the study consists of an examination of the establishment and operation, especially the financing, of six union-management funds that can be classified as automation funds and one that cannot be so classified. The six automation funds are the United Mine Workers of America Welfare and Retirement Fund, the Music Performance Trust Fund, the West Coast Longshore Mechanization and Modernization Fund, the New York Longshore Container Fund, the New York Longshore Bulk Sugar Fund, and the Armour Automation Fund. Of special interest is the discussion of the International Ladies' Garment Workers' Union Supplementary Unemployment Severance Pay Benefit Fund. This is not an automation fund, but it is included in the study as an example of a type of financing and of benefits that might be of interest to those responsible for the efficient functioning of present and future automation funds.

A dismal conclusion that emerges from the study is that automation funds, as presently set up, provide little or nothing in benefits for displaced workers. Moreover, those funds that aimed at providing such benefits to displaced workers, such as the miners' and music funds, have moved to decrease these benefits. Also certain provisions of the Taft-Hartley Act and of many state unemployment compensation statutes in various states have worked to the disadvantage of displaced workers. Making the plight of displaced workers even more distressing is that, in many industries, firms are in no financial condition to provide for displaced workers, either through an automation fund or by other methods. Both management and labor unions, as a general rule, have traditionally been interested in securing the cooperation and support of retained employees in industries where automation has been introduced, with little thought for displaced workers.

Though the scope of the volume under review is rather limited, and a more extended discussion of possible solutions would have been in order, Kennedy does point the way. One hopes that management, labor, and government will join forces to act expeditiously in this area. An encouraging sign in this direction is the Presidential Commission on Automation. Its projected study of the long-range effects of automation, it is hoped, will include an analysis of what should be done for the displaced worker.

HARRIS PROSCHANSKY

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Population; Welfare Programs; Consumer Economics

Income and Welfare in the United States. By JAMES N. MORGAN, MARTIN H. DAVID, WILBUR J. COHEN, AND HARVEY E. BRAZER. A Study by the Survey Research Center, Institute for Social Research, University of Michigan. New York: McGraw-Hill Book Co., 1962. Pp. 531. $7.95.

James Morgan, Martin David, Wilbur Cohen, and Harvey Brazer have collaborated to produce a highly interesting and important book. Here for the first time the full power of multivariate analysis applied to a well-designed national sample survey has been used to find answers to such basic questions as: How do age, race, education, family composition, and other factors affect family income? What are the determinants of poverty and to what extent are they transmitted from one generation to the next? How is the distribution of income affected by taxation and public programs and by interfamily transfers?

The book is well organized and clearly presented, which is remarkable considering that it had four authors. It should be valuable not only to the economist and the sociologist, but also to that mythical reader, the educated layman, provided he has some rudimentary notions about statistical inference and is not inclined to panic when he sees an array of numbers.

The basic data consist of the results of lengthy interviews—the questionnaire, reproduced in an appendix, takes 30 pages—with almost 3000 spending units in 2800 families. The sample represents a cross-section of the noninstitutional population of the United States (excluding Alaska and Hawaii) in early 1960. Low-income families were sampled at a higher rate than others in order to improve the reliability of estimates at the low end of the income scale.

Chapters 2 through 15 are devoted to a painstaking examination of the determinants of family income. The authors break the analysis into a sequence of steps, first addressing themselves to the question of what determines whether the head of the spending units works, then to the factors affecting his hourly earnings, then to the determinants of the number of hours he works. Next they examine the factors affecting capital income, then those affecting home production, then the determinants of the wife's employment, hourly earnings, and hours worked. Finally, they turn to the statistical explanation of other earnings, transfer income, and income received through living with relatives. The component determined at each step of this sequence is allowed to enter into subsequent steps, but not into previous ones. For example, the earnings of the spending unit head are used to explain whether the wife works, but the wife's earnings are assumed not to affect the husband's employment.

Many of the results confirm what one would expect on the basis of previous less refined studies and common sense. The standard demographic variables —age, race, sex, family composition, and education—turn out to be significant in most of the relationships examined. The authors put particular stress on education as a predictor and on its interaction with age and other variables. Although education contributes little to the explanation of labor force participation by spending unit heads (except that college-educated people apparently retire later than others), it is a significant predictor of wives' employment and an important determinant of hourly earnings both of heads and wives. The problem of the extent to which education is a proxy variable for ability and family background is mentioned, but not solved. One interesting finding is that college graduates apparently work shorter hours, on the average, than all other persons except the least educated. The authors speculate that the short hours of the least educated are involuntary, but that college graduates choose to take part of their higher incomes in increased leisure. Also noteworthy is the strong negative relationship which emerges between hourly earnings of spending unit heads and hours worked when other factors are held constant. For wives the relationship has an inverted U-shape; high- and low-paid women work shorter hours than those in the middle range.

The multivariate techniques suggest some interesting relationships between race (white, nonwhite) and the various components of family income. Race by itself appears to contribute nothing to the explanation of whether the spending unit head works. When other variables were held constant the nonwhite spending unit heads in the sample were slightly *more* likely to be employed than white heads. Race did prove significant, however, in explaining hourly earnings of employed heads. On the average white heads earned 77 cents an hour more than nonwhites. Only about half this difference could be attributed to education, occupation, and other measured variables, leaving the other half to be explained by variables not measured and by pure discrimination. For wives, by contrast, race by itself was a significant predictor of employment, but not of hourly earnings. Nonwhite wives appear far more likely to work than white wives in the same circumstances (especially if there are small children present), but their hourly earnings differ little from those of white wives when other variables are held constant. Finally, race proved significant in predicting the decision of adults to live with relatives, but in an unexpected direction. Once their other income and regional distribution was accounted for, nonwhites were less likely than whites to increase their incomes by accepting housing from relatives.

Besides the standard demographic variables, the authors include religious and political preference, as well as some attitudinal and motivational variables such as plans to help parents or send children to college, attitude toward hard work and a "need-achievement" score. Some of the motivational variables prove statistically significant, although they are often hard to interpret. For example, it appears that husbands are more likely to be employed if they have two or more levels *less* education than their wives. The authors interpret this as a motivational factor—a desire to keep the wife in the style to which her education has accustomed her. But it may just reflect a tendency for men with better educated wives to be more highly educated themselves than their years of formal schooling would indicate.

The discussion of transfer income is disappointingly thin—in part because the data are thin, since only a small percentage of the units in the sample receive a particular type of transfer income. Presumably for this reason, the authors lump all transfers together—from welfare payments and unemployment compensation to corporate executives' pensions and movie stars' alimony. They point out that although the proportion of units receiving such payments declines as factor income rises, the average amount received by recipients first declines and then rises again for units with factor incomes over ten thousand dollars. This leads the authors to conclude that transfer payments "are not so redistributive as might have been expected." This reviewer does not know what the authors might have expected, but she would question the usefulness of any over-all statement about the redistributive effects of such a heterogeneous group of receipts.

Two interesting but depressing chapters are devoted to the poor and the disabled. The authors took estimates of minimum adequate budgets for families of various sizes and compositions and defined as "poor" those families whose incomes were less than 90 per cent of these minimum needs and who had liquid assets of less than $5000. About 20 per cent of all families are "poor" by this definition. Most of them have been poor for a long time and seem likely to remain so. The heads of these families are likely to be poorly educated and unskilled and to have had parents who were somewhat less educated and skilled than the parents of other family heads. But the characteristics of their parents do not go far toward explaining their present poverty. For some reason the heads of poor families simply failed to improve on the education and skills of the previous generation to as great an extent as other family heads.

Another particularly interesting chapter deals with the effects on the income distribution of public education and the tax usually used to support it—the residential property tax. The authors estimate the amounts of tax passed on to renters and demonstrate that the property tax is not really as regressive as it appears when estimates of its incidence are based on data for home owners alone and when the imputed income from owner-occupied housing is neglected.

In another chapter the authors show clearly that measures of income inequality are greatly affected by the consumption unit chosen—the family, the spending unit, etc. It is not clear, however, why the authors feel so strongly that the "adult unit" (adult, spouse and minor children) is better than the family for this purpose. It is true that the family, like the income tax, is a device for redistributing income—but so is marriage itself. The effectiveness of these various devices as income redistributors needs to be measured, but it is not obvious that any one unit can be chosen as the true basic unit of economic life.

There are also chapters on voluntary philanthropy, on social and geographic mobility, and on the extent and realism of family planning for higher education and retirement. In fact, there is a great deal in this book. While one might quarrel with details and one might wish the authors had carried the analysis further at some points, it is a fine piece of work and deserves to be widely and carefully read.

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NOTES

SURPLUS CENSUS REPORTS FOR 1950 AND 1954 AVAILABLE

The Bureau of the Census is planning to dispose of surplus copies of reports of the *1950 Censuses of Population and Housing* and the *1954 Census of Agriculture.* The following categories of publications are available:

1950 Censuses of Population and Housing—some clothbound volumes, many individual state reports, census tract reports, housing reports by city blocks, and special demographic studies on such topics as education, families, fertility, population mobility, marital status, and the labor force.

1954 Census of Agriculture—graphic summaries and special reports on ranking agricultural counties, part-time farming, the farm mortgage debt, and other topics. No individual reports are available.

Copies of the *County and City Data Book, 1956*, are also available.

Interested persons should write promptly to the Publications Distribution Section, Bureau of the Census, Washington, D.C., 20233, indicating the publications they would like to receive, or requesting a checklist of specific areas of interest to use in ordering specific available reports. Requests will be filled as long as the supply lasts. Although no charge will be made for the publications, it may be necessary to ask the recipient to pay shipping charges on any large order.

FELLOWSHIPS AND GRANTS

The United States Arms Control and Disarmament Agency announces grants to support research pertinent to arms control and disarmament. Inquiries should be addressed to Dr. Franklyn A. Long, Chairman, ACDA Research Council, Washington, D.C.

The Ford Foundation's Fellowship Program in Business Administration and Economics will again offer for the coming year a limited number of doctoral and faculty fellowships. Detailed announcements about individual categories and nomination forms have been furnished to all institutions participating in this program. Further information may be obtained from the Ford Foundation, 477 Madison Avenue, New York 22, New York.

Announcements

The International Study and Research Institute, a nonprofit educational organization, has recently been incorporated in New York to conduct courses and seminars in international affairs, economic and social development, and in other areas. For further information contact Dr. Arno Ruth, Executive Director, 41 Central Park West, New York 23, New York.

Deaths

E. H. Anderson, professor of management, University of Alabama, July 1963.

J. I. Falconer, former chairman, department of agricultural economics and rural sociology, Ohio State University and past president, American Farm Economics Association, August 6, 1963.

Edwin B. George, formerly with Dun & Bradstreet and former chief, economics division, legislative-reference service, Library of Congress, Washington, in New York, September 15, 1963.

Asher Isaacs, professor of economics, University of Pittsburgh.

Robert J. McFall, former professor at Massachusetts Agricultural College and member of the research staff, Department of Commerce, Springfield, Missouri, July 18, 1963.

Arthur Salz, emeritus professor of economics, Ohio State University, August 10, 1963.

Retirements

Harold A. Eppston, School of Commerce, New York University.

Paul T. Homan, partial retirement, Southern Methodist University.

Carl F. Wehrwein, Foreign Agricultural Service, Department of Agriculture, Washington, February 1963.

Visiting Foreign Scholars

S. V. Ayyar, Indian Institute of Economics: visiting professor of economics and government, Los Angeles State College, spring 1963.

Alan C. L. Day, London School of Economics: visiting professor, finance department, Wharton School, University of Pennsylvania, spring term 1964.

Igbal S. Gulati, University of Baroda, India: visiting professor, department of economics, University of Hawaii, fall semester 1963.

Roy Harrod, Christ Church, Oxford: visiting professor, economics department, Wharton School, University of Pennsylvania, spring term 1964.

Dennis S. Lees, University of Keele: visiting associate professor of business economics, Graduate School of Business, University of Chicago, October 1963 through June 1964.

Robert McDaniel, University of Western Ontario: visiting lecturer, Wharton School, University of Pennsylvania.

Chakravarthi Rangarajan, University of Rajasthan, India: visiting lecturer, finance department, Wharton School, University of Pennsylvania.

H. K. Manmohan Singh, Delhi School of Economics: visiting lecturer, finance department, Wharton School, University of Pennsylvania.

Henri Theil, Netherlands School of Economics, Rotterdam: Ford Foundation visiting professor of econometrics and management science, Graduate School of Business, University of Chicago, spring quarter 1964.

Sten Thore, University of Uppsala: visiting research scientist, Systems Research Group, Northwestern University, academic year 1963–64.

Basil S. Yamey, London School of Economics: Ford Foundation visiting professor of business economics and accounting, Graduate School of Business, University of Chicago, autumn quarter 1963.

Promotions

Morton Backer: professor of accounting, New York University.

Stanley Baff: assistant professor of economics, Clemson College.

William L. Baldwin: associate professor, department of economics, Dartmouth College.

James L. Becker: associate professor of economics, New York University.

John O. Blackburn: associate professor, Duke University.

L. Philip Blanchard: associate professor of business administration, Texas Western College.

Robert W. Campbell: professor of economics, Indiana University.

Byung T. Cho: associate professor, East Carolina College.

Meredith O. Clement: associate professor, department of economics, Dartmouth College.

Paul Davidson: associate professor, economics department, Wharton School, University of Pennsylvania.

Daniel Diamond: associate professor of economics, New York University.

Robert R. Dince: professor of banking and finance, University of Georgia.

Philip Duriez: associate professor of economics, Texas Western College.

Eber W. Eldridge: professor, department of economics, Iowa State University.

Edgar Feige: assistant professor, department of economics, University of Wisconsin.

Daniel O. Fletcher: associate professor of economics, Ohio State University.

Lehman B. Fletcher: associate professor, department of economics, Iowa State University.

Martin Gage: associate professor of accounting, New York University.

Hans W. Gerhard: associate professor, Duke University.

Gerald J. Glasser: professor of business statistics, New York University.

Arthur S. Goldberger: professor, department of economics, University of Wisconsin.

Craufurd D. Goodwin: associate professor, Duke University.

Charles H. Griffin: professor, University of Texas.

Lafayette G. Harter, Jr.: associate professor of economics, Oregon State University.

Robert H. Haveman: assistant professor of economics, Grinnell College.

David G. Hayes: assistant professor of economics, Pennsylvania State University.

George Heitmann: assistant professor of economics, Pennsylvania State University.

Lewis E. Hill: professor of economics, Clemson College.

Thomas Iwand: associate professor of economics, Pennsylvania State University.

George Jaszi: director, Office of Business Economics, Department of Commerce, Washington.

David B. Johnson: professor, department of economics, University of Wisconsin.

George Macesich: professor of economics, Florida State University.

Vaclav Mares: professor of economics, Pennsylvania State University.

John A. Menge: associate professor, department of economics, Dartmouth College.

Charles R. Minton: professor of finance, Old Dominion College.

Donald A. Moore: professor of economics and statistics, Los Angeles State College.

David J. Ott: associate professor of economics, Southern Methodist University.

Louis J. Paradiso: associate director, Office of Business Economics, Department of Commerce, Washington.

Kenneth D. Patterson: associate professor of economics, Oregon State University.

Richard L. Pfister: associate professor, department of economics, Dartmouth College.

Carl Polsky: assistant professor, accounting department, Wharton School, University of Pennsylvania.

John H. Power: professor of economics, Williams College.

Frances Quantius: professor of economics, Ohio State University.

Ray C. Roberts: professor of economics, Old Dominion College.

Warren C. Robinson: associate professor of economics, Pennsylvania State University.

Stephen E. Schoderbek: assistant professor of economics, U.S. Air Force Academy.

J. T. Scott: professor, department of economics, Iowa State University.

George A. Spiva, Jr.: associate professor, department of economics, University of Tennessee.

H. John Thorkelson: associate professor of economics, University of Connecticut.

J. Earl Williams: associate professor, department of economics, University of Tennessee.

Robert M. Williams: professor, School of Business Administration, University of California, Los Angeles.

J. R. Willsey: assistant professor of accounting, Old Dominion College.

Donald L. Winkelmann: assistant professor, department of economics, Iowa State University.

James G. Witte: associate professor of economics, Indiana University.

Shih-Yen Wu: associate professor of economics and statistics, Los Angeles State College.

Leland B. Yeager: professor of economics, University of Virginia.

Frederick A. Zeller: assistant professor of economics, Ohio State University.

Administrative Appointments

Moses Abramovitz: head, department of economics, Stanford University.

Norman Adler: chief, Assets Research Staff, Income, Finance, and Wealth Branch, Statistics Division, Internal Revenue Service, Washington.

Harold J. Barnett: professor and chairman of economics department, director of NASA project on economic impacts of the space program, Washington University.

Earl D. Bennett: professor and chairman, department of accounting, University of Texas.

Robert W. Campbell: director, Russian and East European Institute, Indiana University.

Paul G. Craig: chairman, department of economics, Ohio State University.

Kenneth J. Curran: interim dean of instruction, Colorado College.

Robert R. Dince: chairman, department of banking and finance, University of Georgia.

Mason M.Gaffney: chairman, department of economics, University of Wisconsin-Milwaukee.

William B. Gates, Jr.: chairman, Center for Development Economics, Williams College.

Frank H. Golay: chairman, department of economics, Cornell University.

Robert N. Grosse: chief, economics and costing division, Research Analysis Corporation, Bethesda.

William L. Grossman: chairman, department of transportation, New York University.

Bernard Hanes: director, Bureau of Business and Economic Research, Los Angeles State College.

Irvin L. Heckmann: dean, College of Business Adminstration, Creighton University.

Lawrence G. Hines: chairman, department of economics, Dartmouth College.

Paul M. Jones: acting chairman, department of business administration and economics, Colorado College.

Robert M. Kamins: director of faculty and curriculum development, University of Hawaii.

Joseph A. Kershaw: Provost, Williams College.

Leslie A. Koempel: chairman, department of economics, Vassar College.

Ernest Kurnow: chairman, department of business statistics, New York University.

Carl McGuire: chairman, department of economics, University of Colorado.

Harald B. Malmgren: head, Economics Group, Institute for Defense Analyses, Washington

Howard W. Nicholson: chairman, department of economics, Clark University.

Harry T. Oshima: chairman, department of economics, University of Hawaii.

I. James Pikl, Jr.: professor of economics and head, department of economics, University of Wyoming.

John M. Richards: head, Bureau of Business and Economic Research, Texas Western College.

Ronald Ridker: director, U.S. Public Health Service air pollution research project, Washington University.

Eric Schenker: associate dean, College of Letters and Science, University of Wisconsin-Milwaukee.

Floyd R. Simpson: chairman, division of business and economics, Los Angeles State College.

Haskell P. Wald, Treasury Department: chief, Office of Economics, Federal Power Commission, Washington.

Bert Wallace, Millsaps College: director, Bureau of Business Research, University of Nebraska.

Appointments

Richard Ablin: assistant professor of economics, State University of New York, Buffalo.

Michael Albery, Boston College: professor of economics and finance, Bethune-Cookman College.

George L. Almond: general economist, U. S. Army Engineer District, Louisville.

Abbas Alnasrawi: instructor, commerce and economics department, University of Vermont.

V. E. Andic: associate professor, Union University and visiting professor, New School for Social Research.

Albert Ando, Massachusetts Institute of Technology: associate professor, finance department, Wharton School, University of Pennsylvania.

Ivan Avakumovic, University of Manitoba: associate professor of economics, University of British Columbia.

Michel L. Balinski, Princeton University: associate professor, economics department, Wharton School, University of Pennsylvania.

Nicholas Baloff: assistant professor of production management, Graduate School of Business, University of Chicago.

Benjamin Barg, Wharton School, University of Pennsylvania: Secretariat, United Nations.

Nathan M. Becker: professor of economics, Pace College.

Stanley Besen, University of California: economist, Institute for Defense Analyses, Washington, D.C.

Leroy L. Blakeslee: research associate, department of economics, Iowa State University.

Howard R. Bloch: instructor, department of economics, Dartmouth College.

Floyd S. Brandt: associate professor, University of Texas.

William Bryan: visiting assistant professor of economics, Washington University, 1963–64.

Glenn E. Burress: assistant professor of economics, University of Kentucky.

Frank W. Carlborg, Rockford College: instructor in statistics, Graduate School of Business, University of Chicago.

Alan Carlin: economics department, RAND Corporation.

Lon C. Cesal: research associate, department of economics, Iowa State University.

Robert Cheney: instructor, department of economics, Boston College.

Hang-Sheng Cheng: assistant professor, department of economics, Iowa State University.

Rockwood Chin, Berea College: professor of economics, Wheaton College.

Lowell K. Clarke: assistant professor of accounting, Old Dominion College.

Sanford Cohen: International Labor Office assignment, Latin America, 1963–64.

Robert Colwell: lecturer, economics department, Georgetown University.

Apostolos Condos: research associate, Iowa State University, Iowa-Peru project on agrarian reform and economic development, AID, for two years.

Charles A. Cooper: economics department, RAND Corporation.

William A. Cox, Princeton University: economist, Institute for Defense Analyses, Washington, D.C.

John G. Cross, Princeton University: economist, Institute for Defense Analyses, Washington, D.C.

Rex B. Cruse, Jr.: lecturer, University of Texas.

Douglas Dacy, Harvard University: economist, Institute for Defense Analyses, Washington D.C.

Bruce F. Davie: instructor, economics department, Georgetown University.

Bernard Davis: instructor in economics, University of Kentucky.

Harry L. Davis: instructor in marketing, Graduate School of Business, University of Chicago.

Patrick B. de Fontenay: instructor in economics, Vassar College.

Michael E. DePrano, Michigan State University: assistant professor, University of Southern California.

John W. Dorsey: assistant professor, economics department, University of Maryland.

John Doukas: assistant professor of economics, Miami University.

Myles M. Dryden, Massachusetts Institute of Technology: department of political economy, University of Edinburgh, Scotland.

Bruce F. Duncombe: instructor, economics department, Georgetown University.

Henry Einhorn, The Econometric Institute, Rotterdam: assistant professor of economics, Pennsylvania State University.

Eugene Elander: instructor in economics, Miami University.

Lois Ernstoff, Yale University: economist, Institute for Defense Analyses, Washington, D.C.

Teodoro Estacion: visiting lecturer, University of Texas.

Michael K. Evans: lecturer, economics department, Wharton School, University of Pennsylvania.

Arnold M. Faden: instructor, department of economics, Iowa State University.

Lattee A. Fahm: assistant professor of economics, University of Connecticut.

Donald Farness: assistant professor of economics, Oregon State University.

Irwin Feller, University of Minnesota: instructor in economics, Pennsylvania State University.

Marianne Ferber: lecturer in economics, University of Illinois.

Charles Floyd, University of North Carolina: assistant professor of finance, University of Georgia.

Hugh Folk: assistant professor of economics, Washington University.

Merrill J. Foote: assistant professor of economics, quantitative business analysis, Illinois Institute of Technology.

Charles B. Franklin, Jr.: interim instructor, department of economics, University of Florida.

E. Bruce Fredrikson: assistant professor, finance department, Wharton School, University of Pennsylvania.

Harvey Galper: instructor, department of economics, Dartmouth College.

Lewis Gaty, Stanford University: instructor in economics, Swarthmore College.

Richard J. Gelson: instructor, economics department, Georgetown University.

Hourmouzis G. Georgiadis: research staff, National Bureau of Economic Research.

Jean D. Gibbons: assistant professor, statistics department, Wharton School, University of Pennsylvania.

Calvin Gogerty, Stanford University: logistics department, RAND Corporation.

Edward Greenberg: assistant professor of economics, Washington University.

Robert L. Greene: interim instructor, department of economics, University of Florida.

Lawrence Grose: assistant director, chief statistician, Office of Business Economics, Department of Commerce, Washington.

Michael Hamburger: lecturer, economics department, Wharton School, University of Pennsylvania.

Mark Hanna, Auburn University: associate professor of finance, University of Georgia.

Lyle M. Hansen: visiting associate professor of economics, Williams College.

Lawrence Harris: instructor in business statistics, New York University.

Walter Harter: extension farm management specialist, Ohio State University.

Robert Haseltine, Syracuse University: assistant professor of social studies, State University College, Geneseo, New York.

John Hause, University of Minnesota: economist, Institute for Defense Analyses, Washington, D.C.

Robert L. Heilbroner: lecturer in economics, Graduate Faculty, New School for Social Research, New York.

William L. Henderson, Denison University: associate professor, department of economics, University of Arizona.

G. Rex Henrickson: associate professor, department of economics, University of Wyoming.

Roy J. Hensley, University of California: associate professor, department of economics, University of Hawaii.

James Herendeen: instructor in economics, Pennsylvania State University.

Harley H. Hinrichs: lecturer in economics, economics department, University of Maryland.

Harold M. Hochman, Treasury Department: economist, Institute for Defense Analyses, Washington, D.C.

Thomas E. Holland: assistant professor of economics, University of Alabama.

Franklyn D. Holzman, Tufts University: professor of international economics, Fletcher School of Law and Diplomacy.

Raymond W. Hooker: assistant professor, department of economics, University of Wyoming.

John R. Hotson: assistant professor of economics, Colorado College.

Franklin P. Howard: assistant professor of economics, University of Connecticut.

Ke Ting Hsia: assistant professor of economics and statistics, Los Angeles State College.

Manley R. Irwin: assistant professor of economics, Whittemore School, University of New Hampshire.

Mamoru Ishikawa: instructor in economics, University of Pittsburgh.

Ronald E. Jablonski: assistant professor of business, Graduate School of Business, Columbia University.

Edward B. Jakubauskas: assistant professor, department of economics, Iowa State University.

George Jaszi: professorial lecturer, economics department, Georgetown University.

Hans E. Jensen, University of Alaska: associate professor of economics, University of Tennessee.

Dorothy Jones: associate professor of marketing, Old Dominion College.

Edward Kalachek: assistant professor of economics, Washington University.

Nake M. Kamrany, University of Southern California: senior economist, Battelle Memorial Institute.

Norman G. Keig: acting assistant professor, department of economics, University of Florida.

J. Daniel Khazzoom: acting assistant professor, department of economics, Cornell University.

Kenneth O. Kortanek, Northwestern University: instructor in applied mathematics and management, Graduate School of Business, University of Chicago.

Leopold B. Koziebrodzki, University of North Carolina: research staff, Research Analysis Corporation, Bethesda.

Howard Kunreuther, Massachusetts Institute of Technology: economist, Institute for Defense Analyses.

Ivan Langaskens: assistant professor of economics, Florida State University.

Robert G. Lattimer: assistant extension specialist in farm management, department of agricultural economics, Rutgers—The State University.

Prem S. Laumas: interim assistant professor, department of economics, University of Florida.

McPherson Lemoyne: interim instructor, department of economics, University of Florida.

James R. Leonard: assistant professor of economics, Ohio State University.

Wilford L. L'Esperance: assistant professor of economics, Ohio State University.

Philip Ljungdahl: lecturer, University of Texas.

Allyn Lockner: analytical statistician, Bureau of the Census.

Michael McCarthy, Southern Methodist University: economist, Institute for Defense Analyses, Washington, D.C.

Rosemary MacLellan: assistant professor of economics, Old Dominion College.

Michael McPhelin: professor, economics department, Georgetown University.

Vinod P. Maniyar: instructor, economics department, Boston University.

Leonard W. Martin, Chamber of Commerce of U.S.: associate professor of economics, Loyola University, Chicago.

John Moore: farm management extension specialist, Ohio State University.

John R. Moore: visiting professor of economics, Cornell University, spring 1964.

Donald F. Morrison, National Institute of Health: associate professor, statistics department, Wharton School, University of Pennsylvania.

David C. Motter, Wharton School, University of Pennsylvania: Treasury Department, Washington, D.C.

Marnie Mueller: instructor in economics, Washington University.

Richard Muth: visiting professor of economics, Washington University.

Wallace L. Nelson: instructor, department of economics, University of Wyoming.

Gordon L. Nielsen: lecturer, University of Texas.

David L. Paden: economist, Stanford Research Institute, South Pasadena.

George M. Parks: assistant professor, industry department, Wharton School, University of Pennsylvania.

Henry M. Peskin, Princeton University: economist, Institute for Defense Analyses, Washington, D.C.

John L. Peterman: lecturer in economics, University of Virginia.

Rolf Piekarz, RAND Corporation: economist, Institute for Defense Analyses, Washington, D.C.

Yakir Plessner: research associate, department of economics, Iowa State University.

Joel Popkin: staff economist, Wealth Inventory Planning Study, George Washington University.

S. B. Prasad, University of Wisconsin: assistant professor of economics and business, University of Minnesota-Morris.

James R. Prescott: instructor, department of economics, Iowa State University.

Frank Puffer: acting assistant professor of business economics, School of Business Administration, University of California, Los Angeles.

Roger L. Ransom: assistant professor of economics, University of Virginia.

Peggy Richman, Columbia University: assistant professor, economics department, Wharton School, University of Pennsylvania.

Ralph Ricks: assistant professor of finance, School of Business Administration, University of California, Los Angeles.

John C. Riew, University of Wisconsin: assistant professor of economics, Pennsylvania State University.

R. Stephen Rodd, University of Minnesota: associate professor, department of agricultural economics, Ontario Agricultural College.

Leonard Rosenberg, University of Chicago: economist, Institute for Defense Analyses, Washington, D. C.

Kenneth J. Rothwell: associate professor of economics, Whittemore School, University of New Hampshire.

Edward J. Sanders: assistant professor of business administration, Texas Western College.

Kehar Sangha: associate professor of economics, Old Dominion College.

Dean F. Schreiner: research associate, Iowa State University, Iowa-Peru project on agrarian reform and economic development, AID, for two years.

Wade Sewell, Cornell Aeronautical Laboratory: economist, Institute for Defense Analyses, Washington, D.C.

Alvah R. Shepard: assistant professor of money and banking, department of business and economics, Illinois Institute of Technology.

Francis Shieh, Aquinas College: economist, RAND Corporation.

Irwin H. Silberman: instructor in banking, New York University.

Charles D. Smith, University of Florida: associate professor, Florida Presbyterian College.

Hadley E. Smith: lecturer in economic development and administration, Institute of Public Administration, University of East Africa, Dar es Salaam, Tanganyika.

Benson Soffer, Wharton School, University of Pennsylvania: Committee on Economic Development, Washington, D.C.

Alan Sokolski, Federal Reserve System: assistant program economist, AID Mission in Lagos, Nigeria.

Dennis R. Starleaf: instructor, department of economics, Iowa State University.

Herbert Stein: instructor in economics, University of Pittsburgh.

Peter G. Stercho, St. Vincent College: associate professor of economics, Drexel Institute of Technology.

John W. Stevenson: lecturer, University of Texas.

Allan D. Stone: assistant professor of economics, Texas Western College.

Raymond S. Strangways, Old Dominion College: lecturer in economics, University of Maryland, European Division.

Leopold A. Sveikauskas: instructor in economics, University of Connecticut.

Corine Thompson: assistant professor, University of Texas.

Wilbur Thompson: visiting professor of economics, Washington University.

Adrian W. Throop: lecturer, economics department, Wharton School, University of Pennsylvania.

William S. Truppner, RAND Corporation: economist, Institute for Defense Analyses, Washington, D. C.

Rene I. Vandendries: research associate, Iowa State University, Iowa-Peru project on agrarian reform and economic development, AID, for two years.

Thomas Vietorisz: visiting associate professor of economics, Graduate Faculty, New School for Social Research, New York.

George Viksnins: instructor, economics department, Georgetown University.

Edwin von Boeventer: professor of economics, University of Heidelberg, Germany.

Elliot Wetzler, Johns Hopkins University: economist, Institute for Defense Analyses, Washington, D.C.

Joseph T. White: instructor, economics department, Georgetown University.

Arthur A. Wichmann: fiscal and financial economist, Office of Development Planning, Bureau for the Far East, State Department, AID.

John H. Wicks: assistant professor of economics, Ohio State University.

James Wilson: lecturer, University of Texas.

Gordon C. Winston: assistant professor of economics, Williams College.

Murray Wolfson: assistant professor of economics, Oregon State University.

David McCord Wright, Foundation for Economic Education: economic consultant to Federal Reserve Bank, Atlanta, Georgia.

Leonard Wright, University of Georgia: associate professor of finance, State University of New York, Albany.

Murray Yanowitch: visiting associate professor of economics, Graduate Faculty, New School for Social Research, New York.

Leaves for Special Appointments

William L. Baldwin, Dartmouth College: research professorship, Brookings Institution.

Andrew F. Brimmer, University of Pennsylvania: deputy assistant secretary for economic affairs, Department of Commerce, Washington.

Webster C. Cash, Florida State University: visiting professor. University College of Rhodesia and Nyasaland.

Paul G. Clark, Williams College: director of economic research, East African Institute of Social Research, Kampala, Uganda.

Alvin E. Coons, Ohio State University: Ellsworth Air Force Base.

Sergei P. Dobrovolsky, Rensselaer Polytechnic Institute: visiting professor of economics, Rice University, academic year 1963–64.

Edgar S. Dunn, Jr., University of Florida: deputy assistant secretary for economic affairs, Department of Commerce, Washington.

Daniel O. Fletcher, Ohio State University: research economist, Industrial Economics Staff, Department of Commerce, Washington.

I. J. Goffman, University of Florida: visiting research associate professor, department of economics, Queens University, academic year, 1963–64.

Richard H. Holton, University of California, Berkeley: assistant secretary for economic affairs, Department of Commerce, Washington.

Thor Hultgren, National Bureau of Economic Research: visiting faculty member, University of Wisconsin, Milwaukee, academic year 1963–64.

Clark Joel, Old Dominion College: AID Program Planning Division for Laos.

Paul E. Koefod, University of Florida: Fulbright lecturer, Melbourne University, September 1963 through June 1964.

John Lynberopoulos, Ohio University: visiting assistant professor of statistics, University of Texas, academic year 1963–64.

Peter F. M. McLoughlin, University of California, Los Angeles: African development project, International Bank for Reconstruction and Development, Washington, academic year 1963–64.

Edwin Mansfield, Carnegie Research Project: professor, Wharton School, University of Pennsylvania, 1963–64.

John R. Moore, University of Tennessee: visiting professor of economics, Cornell University, second semester 1963–64.

Samuel H. Nerlove, University of Chicago: visiting professor, School of Business Administration, University of California, Los Angeles, academic year 1963–64.

Almarin Phillips, University of Virginia: professor, Wharton School, University of Pennsylvania.

James E. Price, Syracuse University: visiting assistant professor, Royal College, Nairobi, academic year 1963–64.

Martin Segal, Dartmouth College: Harvard Project in Labor-Management Relations.

John B. Sheahan, Williams College: staff economist, Harvard Development Advisory Service assigned to the development of the government of Colombia, Bogotá, Colombia.

Leon Smolinski, Boston College: Russian Research Center, academic year 1963–64.

Charles J. Stokes, University of Bridgeport: member, senior staff, Brookings Institution.

Erik Thorbecke, Iowa State University: Iowa-Peru project on agrarian reform and economic development, AID, for one year.

Burton Weisbrod, Washington University: Council of Economic Advisors.

Resignations

Elbert V. Bowden, Old Dominion College.
Benny Ray Copeland, University of Texas.
Doris Drury, University of Wyoming.
James J. Flannery, Wharton School, University of Pennsylvania.
Patrick W. Gearty, Catholic University of America.
Walter Gensurowsky, Iowa State University.
Harold W. Guthrie, University of Kentucky.
W. W. Haynes, University of Kentucky.
H. Wayne Snider, Wharton School, University of Pennsylvania.
Thomas Sowell, Douglass College, Rutgers-The State University.
Charles B. Titus, Jr., University of Texas.
Norman L. Weed, Old Dominion College.

Miscellaneous

Jacob Marschak was elected an Honorary Fellow of the Royal Statistical Society, June 1963.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section a brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

Vacancies

Special Announcement

U.S. Government Awards under the Fulbright-Hays Act

Labor economists: The Office of Manpower, Automation and Training, U.S. Department of Labor, has positions open in the field of manpower and automation research. Study areas include labor force and occupational trends, mobility, manpower needs, resources, utilization and training; determinants of technological change and implications for employment, education, and industrial relations. Salaries range from $5,540 to $14,565 depending upon experience and training. To apply, submit résumé or Standard Form 57 to: Samuel Ganz, Assistant Director of Manpower and Automation Research, U.S. Department of Labor, Washington 25, D.C.

Senior economist: Salary $9,475 per annum ($9,980 as of January, 1964). Duties involve responsibility for preparation of economic base surveys; developing and/or adapting projections or factors affecting long-run economic trends of regional and local scope; studies, analyzes and evaluates assembled economic data; makes long-run projections for purpose of determining requirements and uses of water, power, irrigation and recreation to establish economic feasibility of projects under consideration. Degree or major in economics required. Good mathematical foundation desirable. B.S. degree plus 3 years of experience in field of economics; M.S. degree plus 2 years of experience; or Ph.D. degree plus 1 year of experience will qualify. Send Standard Form 57 (obtainable at any post office or federal agency) to: D. B. Gorton, U.S. Army Engineer District, Omaha, 6012 U.S. Post Office and Court House, Omaha 2, Nebraska.

Labor economists and/or statisticians: Minimum status of associate professor, experience in research in collective bargaining agreements and fringe benefit programs. The measurement of the economic damages suffered by those who are wrongfully injured or killed is a new area for the application of economic and statistical knowledge and research techniques. This field has been pioneered successfully and Associated Appraisers of Impaired Earning Capacity is now providing this service to attorneys on a nationwide basis. Those in it appraise the losses and are required to testify as an expert in court trials and must withstand hostile cross-examination. It is one of the highest paid forms of work in the profession and it is possible to engage in it as a part-time activity. Send résumé and list of publications to: Philip Eden, President, Associated Appraisers of Impaired Earning Capacity, 5 Piedmont Terrace, San Francisco 17, California.

Economist I: To assist in the development and evaluation of Hawaii state government's economic development program and goals. Master's degree in economics and 4 years of professional economics experience. Salary: $9,096-$11,616 per year. Write to: Department of Personnel Services, 825 Mililani Street, Honolulu 13, Hawaii.

Economist II: To head research division of Hawaii state Department of Planning and Economic Development, conduct economic analyses for state planning and development, co-ordinate state research activities, conduct market and cost analyses for specific sectors. Ph.D. in economics and 4 years of professional economic experience in responsible supervisory consultant or administrative capacity. Salary: $11,064-$14,112 per year. Write to: Department of Personnel Services (address above).

Economic consultants: Six months to one year with top Indian economic research organization. Fields of specialization: (1) demand and supply projections; (2) input-output analysis, interindustry relations and linear programming; (3) saving and consumption; (4) regional economic development; (5) transportation economics. Will match current salary plus 25 per cent of this amount for the unusual circumstances of working abroad. International transportation to and from post, living allowance and insurances will also be provided. Ph.D., research and teaching experiences required. Send curriculum vitae including names of references directly to: Mr. Robert P. Ho, Institute of International Education, 800 Second Avenue, New York 17, New York.

Operations research, transportation, weapon systems, economic recuperation analyses, regional analyses, economic development, cost analyses: The expanding contract research program of the Research Triangle Institute provides opportunities for experienced researchers at different professional levels. Special training in mathematics, statistics, and knowledge of electronic computers is desirable. Preference will be given to U.S. citizens experienced in constraints of contract research deadlines, proposals, and budgetary requirements. Ability to write, as well as flexibility to work individually or as team member or team leader, and adaptability to changing subjects of research are essential. Salary dependent on qualifications. The Research Triangle Institute is a relatively new, nonprofit, nonendowed, university-affiliated (Duke University, University of North Carolina at Chapel Hill, and State College of North Carolina at Raleigh) Institute. Write to: Dr. Edgar A. Parsons, Director, Operations Research Division, Research Triangle Institute, Post Office Box 490, Durham, North Carolina.

Economics and business: A state university, centrally located in the eastern half of the United States, invites applications for several staff positions. These positions are in the areas of economics and business and include statistics, public finance, marketing, accounting, economic and monetary theory, with the development of other areas a possibility. There will be a need for persons with advanced academic stature as well as for several recent Ph.D.'s. The university offers undergraduate and graduate degrees in economics and business. New facilities will be available to the college in 1964, and salaries will equal those in similarly situated institutions. P274

International economics, statistics, history of economic thought: The Department of Commerce and Economics has an opening starting the spring semester for January, 1964. Preference will be given to candidates with advanced graduate training in the above areas; full-time position. Rank and salary open. Write to: Professor Edwin C. Greif, Chairman, Department of Commerce and Economics, University of Vermont, Burlington, Vermont.

Engineering, business administration, economics: Robert College, in Istanbul, Turkey, presents a challenge in education where East meets West. An opportunity to contribute significantly to the development of a young republic is available to specialists in the above fields and the sciences, the humanities, and English as a foreign language. Graduate degrees required. Address inquiries to: Miss Shirley Osmun, Personnel Officer, Robert College, Bebek Post Box 8, Istanbul, Turkey, with copy to the Near East College Association, 548 Fifth Avenue, New York 36, New York.

Economics and business administration: Two positions open in September, 1964, at Norwich University. One will teach economic principles and labor economics; and the other will teach principles and business law. Prefer Ph.D. or near Ph.D. for assistant professorship. Competitive salaries with scheduled adjustments. Write to: Dr. Elmer L. Munger, Dean of the University, Norwich University, Northfield, Vermont.

Economics: Opening in September, 1964, for young man (either with Ph.D. or with graduate work mostly completed) in six-man department in men's college. Should be willing and able to teach introductory course, labor economics, government and business (temporarily), corporation finance (later). Nine hours teaching plus supervision of some senior theses in areas of teaching competence. Possible opportunity for some graduate teaching, if qualified. Write to: Chairman, Department of Economics, Union College, Schenectady 8, New York.

Economist: The Housing and Home Finance Agency has an opening for a financial economist requiring experience and training primarily in the areas of public finance and fiscal economics. This position offers an excellent opportunity to participate in top-level economic studies affecting basic agency policy decisions in rapidly developing, varied loan programs. Starting salary ranges from $11,150 to $14,565 per annum, depending upon experience and training of the applicant. To apply, submit a statement of training, experience, and references to: Housing and Home Finance Agency, Recruitment and Placement Branch, 1626 K Street, N.W., Room 305, Washington, D.C. 20410.

Economic analyst: Salary $7,296-$9,288, depending on qualifications. Master's degree; or bachelor's degree plus one year of experience. To be responsible for economic and population research, municipal population and economic reports, central business district analysis and specialized studies. Ability to write clearly important. Apply to: Mr. John Voorhees, Division of Community Planning, Department of Conservation and Development, Raleigh, North Carolina.

Economist: One position open February, 1964; another September, 1964. Each to consist of some combination of courses in line with interests of appointee; for example (on an alternate or every semester basis): history of economic thought, intermediate theory, managerial or mathematical economics; or international trade, comparative economic systems, economic development, and/or public finance. In addition, incumbent will teach one or two sections of introductory economics each semester. Positions open in rapidly growing state university near Las Cruces where new College of Business and Economics will be established in September, 1964, having previously been a department in the College of Arts and Sciences. Only Ph.D. or those near completion need apply. Assistant or associate professor with minimum salary for nine months of $7,000 or $8,000, respectively, depending upon training and experience. Write to: Dr. Paul W. Zickefoose, Department of Economics, New Mexico State University, University Park, New Mexico.

Economist: Catholic college in eastern Canada has opening for a man with Ph.D. in economics. Specialization in one or two of the following fields: labor, economic history, economic geography, public finance, and statistics. Rank (lecturer or assistant professor) and salary according to education and experience. Appointment tenable from September 1, 1963. Please submit complete résumé including salary. Write to: Francis J. Hayes, Chairman of Economics Department, Loyola College, Montreal, Canada.

Economics: Liberal arts college for women, located in the Middle Atlantic seaboard area, has full-time opening beginning September, 1964, for instructor, assistant professor, or associate professor, according to preparation and experience. Ph.D. required for rank above instructor, and for instructor, within a reasonable time. Expected to teach both advanced and introductory undergraduate courses (area of specialization open to negotiation). Comparatively high salary and considerable fringe benefits. P275

Industrial economist: Progressive midwest-based gas transmission and petrochemical company has challenging opportunity for an industrial economist. Should be capable of conducting advanced research studies concerning economic feasibility of corporate development and diversification. Requires M.B.A. or M.S. in industrial economics plus four to eight years' experience. Familiarity with requirements of resource-based industries is essential. Interested applicants are invited to send résumés and salary requirements to: Thomas P. Vaaler, Northern Natural Gas Company, P.O. Box 308, Department 11, Omaha, Nebraska. An equal opportunity employer.

Economics: A California state college is seeking a man to fill a position as either associate or full professor as chairman of its Economics Department. The requirements call for a Ph.D. degree from a major university, scholarly writing, and evidence of leadership and administrative ability. The salary range is $10,000 to $12,000, dependent upon experience and qualifications. P276

Economists: Several permanent positions in a rapidly growing state university in the midwest. Can be filled at any rank and in almost any field of specialization but preference will be given to general theory, monetary theory, and mathematical economics. All positions require a Ph.D. in economics. Senior positions also require teaching experience, research experience, and publications. Experience directing doctoral dissertations regarded as especially valuable for senior positions. Starting salary for assistant professor $7,500 and up. No upper limit on salary schedule for senior persons but maximum salary now paid is $14,500 (nine months) plus two months' salary for summer school. Write to: James R. Elliott, Head, Department of Economics, Northern Illinois University, De Kalb, Illinois.

Economists and forest economists: The Department of Forestry of Canada at Ottawa is developing a comprehensive program of research in forestry and forestry industries in Canada. Fields of study include demand and marketing, forest resources and policy, industry studies and economics of production. Salaries up to $11,800, depending on education and experience. For further information and application forms, write to: Civil Service Commission of Canada, Ottawa 4, Ontario, Canada, quoting reference 63-2026.

Research and survey analysts: Thirty positions are now available in various divisions of the Dominion Bureau of Statistics of the government of Canada. These positions are located at Ottawa and carry salaries ranging from $4,740 to $11,800. Applications are invited from graduates in economics, commerce, statistics, mathematics, sociology, psychology, accounting or other disciplines. Further information and application forms may be obtained from: Civil Service Commission of Canada, Ottawa 4, Ontario, Canada. Please quote reference 63-2025.

Statistics, operations research, econometrics, computers: Position available in September, 1964, for Ph.D. or candidate for that degree. Will teach courses in statistics and, if qualified and interested, operations research, econometrics, and computer programming. Opening exists at assistant professor level at a young and rapidly growing California state college. Write to: Dr. T. H. Smith, Chairman, Division of Business Administration and Economics, Orange State College, Fullerton, California.

Economics: Positions available in September, 1964, for Ph.D. or doctoral candidate with interests in the areas of economic history, economic systems, public finance and/or labor. Openings exist at instructor or assistant professor level at a young and rapidly growing California State college with a quality program. Write to: Dr. T. H. Smith, Chairman, Division of Business Administration and Economics, Orange State College, Fullerton, California.

Economics: A leading liberal arts college of 750 men seeks a dynamic teacher of demonstrated professional competence to add to its present staff of three. An experienced Ph.D. in his early thirties would seem the most likely candidate, but chief interest is in the qualities of the man rather than in age or other specific requirements. The applicant should be able to offer a semester course in mathematical economics. Other desirable, but not essential, fields include economic theory, comparative systems, labor, and statistics. An excellent opportunity in an institution that gives high priority to faculty recruitment. Rank and salary open. P277

Industrial economists: Openings for two, to advise economic planning boards of developing nations as to relative values of pending and future projects; to evaluate feasibility studies; to advise on short-term and long-range economic and feasibility studies and policies. To serve in Korea, Thailand, Malaysia, Philippines and/or Iran, Morocco. Salary range $20,000 to $25,000. One-year contract; will consider six months or three months. Forward résumé and two references. P278

Economists Available for Positions

(Italics indicate fields of specialization)

Economic theory, international trade, macroeconomic analysis, comparative economic systems, economic development: Man, 36, married; Ph.D., University of Pennsylvania. Eight years of teaching experience. Presently at a large Canadian university. Seeks position at a large American university or liberal arts college. Travel in India; 10 articles published and book now completed and in hands of publisher; definite research projects established. Interested in associate professorship. E1150

Long-range planning, financial and economic analysis, international economics, underdeveloped countries: Man, 32; B.S., M.B.A., work towards Ph.D. Eleven years of business and research experience; 6 years of part-time teaching, economics and management. Broad, diversified background, with extensive top-level experience in corporate planning, pricing, and financial studies. Excellent knowledge of manufacturing industry and foreign economic problems. Experienced author and lecturer; many publications. Listed in *American Men of Science* and member of several professional societies. Desires senior position, preferably in industry but will consider academic or research position. E1158

Fiscal and monetary policy, labor relations, consumer economics, history of economic thought, principles of economics: Man, 40, married; Ph.D. Presently teaching assistant at a college. Prefers college or university interested in a teacher rather than a writer. West Coast or nearby areas. E1161

Soviet economy, international economics, comparative economic systems, European economy, international business: Man, 46; Ph.D. Six years of graduate and undergraduate teaching, plus 3 years in Europe. Business experience; publications, research, speeches; fluent Russian, German, French; Concentration on European business or international management problems preferred. Seeks challenging position or interesting position with development opportunities at university, college, or research organization. E1171

Economics of development, area studies (Middle East and North Africa), economics, sociology, geography, planning: Man, 59, married; doctorate, University of Paris (economic geography, localization of industries). Retiring at end of 1963 (could resign earlier) after 17 years of service with United Nations, mainly connected with technical assistance and regional planning. Five years of teaching experience, adjunct professor, New York University. Several languages, including French, Spanish, German. Seeks teaching, research, or operational work; even overseas field work. Could also advise and brief Point Four experts and/or train Peace Corps personnel and community development self-help leaders. E1172

Public finance, price theory, economic thought, money and banking: Man, 44; Ph.D. Nine years of college teaching and 10 years as research associate in public finance at large eastern university. Seeks position as associate professor (present rank) with or without research responsibilities. E1178

Organization and administration, R and D management, managerial economics, long-range planning, policy, cost accounting: Man, 43, married; M.B.A., Ph.D. Numerous publications; 12 years of university teaching experience; 5 years of business experience; wide background in consulting. Presently department head. Desires chairmanship of management department and/or teaching and research. Will consider non-academic opportunities. Three-months availability. E1179

Principles, consumer economics, labor problems, public finance, law, intermediate analysis, comparative economic systems: Man, 35, married; A.B., A.M., LL.B. Attorney; public office 4 years. Experience includes 5 years as chairman of economics department; Ford Foundation Fellow in Economics, Wharton School. Present rank associate professor. Member of Quakers; wife Japanese; requires tolerant environment. Prefers liberal arts college outside the South. Will consider opportunities for September, 1964. E1182

Principles of economics, price theory, monetary and banking theory and policy, international economics, marketing: Man, 41, married; Ph.D., Chicago. Fifteen years of successful college teaching; administrative experience. Recipient of Ford and General Electric Foundation grants. Desires full professorship and chairmanship. Minimum salary $12,000. Available in September, 1964. E1185

Money and banking, public finance, economic theory: Man, 28, married; B.S., M.B.A., expects Ph.D. in summer of 1964. Four years of college teaching experience; business experience. Interested in a college teaching position. Available in September, 1964. E1186

Economics, business administration: Man, 36, married; M.A. Ten years as cost-conscious corporate officer, federal economist, and college instructor. Seeks position as assistant to institutional administrator with opportunities for part-time teaching and/or for conducting management studies. E1187

Macro theory and financial analysis, stabilization policy, money and banking, economic fluctuations, business finance, principles, comparative systems: Man, 45, married; Ph.D. Nine years of college teaching; 4 years of postdoctoral research. Modest publication record. Current research on a conceptual system for macroeconomic analysis based on the flow-of-funds accounting system provides basis for broad program of future empirical and theoretical research. Desires position with research oriented department interested in new analytical approaches, with opportunity to complete research and writing now in progress and to direct graduate research. Available in January, 1964; possibly earlier. E1188

Economic theory, history of economic thought, international economics, economic development and history, money and banking, public finance: Man, 30, married; all requirements for the Ph.D. degree completed except the dissertation. Teaching experience as an assistant professor and graduate associate. Desires teaching and/or research position. E1189

Economic principles, micro and macro theory, money and banking, economic development, international trade, resources and technological development, history of science and technology: Man, 39, married; B.S. in Chemistry, M.A. (economics), additional year of graduate economics, course requirements for Ph.D. in history of technology complete in June. Twelve years of industrial laboratory and marketing experience. Teaching experience: 1 year of economic principles, 2 years of organic chemistry. E1191

Principles, theory, growth, economic history, money and banking: Man, 40, married; M.A., writing Ph.D. dissertation. Three years of experience. Desires teaching position on East Coast. E1192

Industrial relations, labor unionism, business administration, personnel, management, social insurance, statistics, research: Man, 32, married; B.A. (economics), B.B.A., Ph.D. (industrial relations) residence requirements completed (Minnesota). Seven years of research experience; 4 years of national labor unions staff experience; occasional teaching experience. Research and/or teaching. Presently in Midwest; will consider anywhere. Available in September, 1964, or earlier. E1193

International trade and development, commodity studies, economic growth and economic history, monetary and fiscal theory and policy: Man, 43, married; English trained Ph.D. Ten years of teaching and research experience in English and American universities and colleges; modest publication record. Desires teaching and/or research position beginning September, 1964, in East or West Coast area in challenging academic institution. E1194

Economics of growth, economic systems, Soviet-type economy, international economics, monetary economics, national income analysis: Man, 43, married; doctorate. Associate professor at an eastern liberal arts college; research and government experience; good record of recent publications; languages. Seeks teaching position with opportunity for research in comparative economic growth. Available in September, 1964, or later. E1195

Economic principles, statistics, international trade, monetary theory, economic philosophy, industrial research management: Man, 27; B.A., M.S., work towards Ph.D. Experience as university instructor, director of religious education in a Protestant church; also experience in research planning and systems analysis for aerospace firm, vice-president's staff. Desires to teach, preferably at a church-related liberal arts college. Also able to teach psychology of international relations. Available in January, 1964. E1196

Taxation, public finance, money and banking, international economics, principles: Man, 44; doctorate in economics. Thirteen years of college teaching experience; some business experience. Desires teaching position on college-university level. Available in January, 1964. E1197

Microeconomics, macroeconomics, international trade, public finance, money and banking, economic development, Africa study programs: Man, 55, married; B.Com., M.Com. (with distinction), D.Com. Many years of teaching and research experience, including work in African territories. Publications include a number of books. Would welcome teaching and/or research post in the United States or Canada. E1198

Economic theory, economic thought, international economics, European economy, economic development, business cycles: Man, 32, married; Ph.D. Two years of graduate and undergraduate teaching experience; business experience; experience with United Nations. Fluent German, Spanish, French. Presently assistant professor. Desires teaching position. Will also consider working abroad for private business or international agency. Available in June, 1964. E1199

Economics, economic history, theory: Man, British origin. At present resident in South Africa, entering U.S.A. next year. Studied at Witwatersrand University in above subjects and science, physics, mathematics. At present working on a book on the application of thermodynamics to evolution theory. Desires post as assistant to research worker. Bruce Mannooch, 211 Avonal Court, v.d. Merwe Street, Hillbrow, Johannesburg, South Africa.

Statistics, international economics, corporate finance, investment principles, business cycles: Man, 45, married; Ph.D., New York University. Ford Foundation grant (statistics); member of various professional and honor organizations. Eight years of teaching experience in the Midwest and East; 10 years of experience with the federal government as economist/statistician and supervisor (GS-13); 5 years of experience as field engineer (surveyor); fluent Spanish. Presently associate professor with tenure in a midwestern university. Desires teaching position in or closer to the Metropolitan New York area, the Northeast, or the Middle Atlantic States. Available in June or September, 1964. E1200

Economic theory, labor economics, money and banking, comparative wage and turnover analysis, personnel administration problems: Man, 43; M.S., with considerable additional graduate credits. Year and a half as research analyst in national headquarters of voluntary health agency; 3 years enlistment in the Air Force; varied work experience; also extensive undergraduate training in engineering. Willing to relocate and accept basic remuneration for professional position if opportunities for advancement are present. E1201

Investment and corporate economics: Man, 42, married; M.A., Ph.D. course credits completed, Columbia. Current assignment with well-known advisory service. Eight years of corporate experience. Will consider similar position with advisory service, mutual fund, bank, insurance company, corporation. E1202

Marketing, sales management, consumer motivation and behavior, retailing, salesmanship, marketing research, advanced marketing problems, small business administration, economic and business history, American industry and economic geography, money and banking, economic principles: Man, 40, married; Ph.D. (business administration), New York University. Twelve years of teaching experience; 5 years as proprietor of own retail establishment; recipient of graduate scholarship; listed in *Who's Who in the South and Southwest;* published only in a minor way. Presently an associate professor at a medium-sized state college. Desires teaching with business consultation opportunities in a dynamic scholastic and business environment. E1203

Administration or management: Man, in 40's, married; Ph.D., name school, industrial economics-management. Experienced chairman, particularly of graduate programs, budget, faculty affairs, accreditation, and academic procedures. Seeks good faculty environment. E1204

General economics, public finance: Man. Now department chairman in eastern college. Wishes relocation in South or Southwest. E1205

Corporate planning, managerial control, financial analysis: Man, 30; B.S. in Chemical Engineering, M.B.A. (industrial management), course work towards Ph.D. completed. Six years of industrial experience, includes 2 years in the economic evaluation division of a major petroleum company; 4 years in the long-range forecasting section of a nationally known chemical firm. Desires managerial responsibility in an expanding firm. Available in January, 1964. E1206

Principles, history of economic thought, labor problems, industrial relations, investments, public finance: Man, 43, married; M.A., Ph.D. Fourteen years of teaching experience in medium-size liberal arts colleges, of which 9 years were combination teaching and administration (director, evening school). Will consider full-time teaching or administration but prefers combination of teaching and administration. Would consider position abroad. Available in June or September, 1964. E1207

Micro and macro theory, history of economic thought, international economics: Man, 41, married; Ph.D. Eleven years of teaching experience, graduate and undergraduate; modest publication record. Desires a change of institution in fall, 1964. E1208

Economic theory, industrial economics, managerial economics, financial analysis, forecasting, statistics: Man, 29, married; B.S., M.B.A., working towards Ph.D. Four years of experience with business consulting firm. Seeks position with greater responsibility in industry, government, or consulting. E1209

Business statistics, economic theory, economic development, business mathematics, econometrics: Man, 41; foreign born, American educated; M.A., Ph.D., University of Minnesota. Five years of teaching experience; 2 years in research. Presently teaching in a liberal college. Desires a teaching position with a progressive institution. Available in February or June, 1964. E1210

Economic theory, transportation, social control, money and banking, public utilities: Man, 39, married; M.A., Ph.D. dissertation in process, degree expected in June. Five years of teaching experience (last 3 at a university). Desires college or university teaching position. E1211

Economic principles, economic history, money and banking, history of economic thought, comparative economic systems, economic geography: Man; B.Sc., M.B.A., requirements for Ph.D. near completion, dissertation in progress. Eight years of teaching experience; also business experience. Fellowship recipient. Available for a teaching position in the Middle Atlantic States in fall of 1964. E1212

Principles of economics, macro and micro theory, financial accounting, cost accounting, international trade: Man, 43; B.A., M.A., additional year of work towards Ph.D. Four years of college teaching experience. Available in September, 1964. E1213

Economic theory, growth and development, history of economic thought, international economics, public finance, elementary statistics, Southeast Asia: Man, 43, married; M.A., Ph.D. Associate professor in a Midwest university; publications; more than 10 years of university teaching. Seeks university teaching and/or research. Prefers East, Northeast, Midwest. Available after spring, 1964. E1214

Business finance, money and banking, statistics, foreign business operations, Latin-American economics and business development: Man, 29, married; M.A. (economics), Ph.D. (business administration) requirements complete, dissertation in progress. Presently Fulbright lecturer at Mexican institution. Seven years of business experience, of which 5 were in Latin America, both self-employed and with U.S. corporation. Desires challenging and stimulating position connected with Latin America; i.e., business school with an area concentration or desiring one, research, or firm interested in the growth possibilities of Latin-American operations. E1215

Economic principles, public finance, history of thought, intermediate theory: Man, married; Ph.D. except for thesis to be completed by June. Area of special interest is in intergovernmental fiscal relations. Twelve years of college and university teaching experience; 10 years of business experience prior to teaching; 7 years in present position. Desires change for midwestern or western location. Available in September, 1964. E1216

Labor economics, labor legislation, collective bargaining, money and banking, history of doctrine, principles: Man, 40, married; B.S., A.M. Eight years of full-time college teaching, research experience; present rank, assistant professor. Desires teaching and/or research position, preferably in the New England states, West Coast; Rocky Mountain states also considered. Avialable in June, 1964. E1217

Economic theory (micro and macro), operations research, statistics, systems analysis, industrial management: Man, 36; Ph.D., Stanford. Eight years of teaching experience (5 years at graduate level); 4 years of industrial and consulting work. Current rank, associate professor. Desires teaching position with institution interested in developing a management science program. Will consider department chairmanship. E1218

Management theory, labor economics, industrial relations, personnel management, wage and salary administration, industrial management, marketing management, economic theory: Man, 34; B.A., M.A., M.B.A., Ph.D. Six years of teaching and research experience. Several publications. Currently assistant professor in one of the Big Ten. Seeks a visiting associate professorship or assistant professorship in any Canadian university or college. Available in June or September, 1964. E1219

AMERICAN ECONOMIC REVIEW

VOL. LIII, NO. 1 PART 2 MARCH, 1963

SUPPLEMENT

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FOREWORD

This report is part of a large-scale attack on economic illiteracy which has enlisted the support and co-operation of economists, educators, businessmen, and labor leaders. Knowing that a democracy thrives on an informed electorate and aware that an increasing proportion of political decisions are essentially economic in consequence if not always in intent, and recognizing the special responsibility of the economics profession in this area, the American Economic Association and its Committee on Economic Education are working with other interested organizations on a number of projects designed to increase the economic understanding of the nation's citizenry. The committee and the Association named the members of the independent National Task Force on Economic Education whose report, *Economic Education in the Schools,* published in September, 1961, described the "minimum understanding of economics essential for good citizenship and attainable by high school students." The Association participated in selecting the members of the Materials Evaluation Committee whose report, published in October, 1961, undertook to supply teachers with a carefully selected and annotated list of materials available for high school use. The Association is a major sponsor of the College of the Air television program on economics which is appearing during the academic year 1962-63, designed to stimulate interest in economic matters and to make available to high school teachers and others a strong basic course in economics. Still another co-operative committee is presently at work preparing tests of economic understanding for use in the schools. Much of the credit for initiating and carrying out these and other related projects in which the Association has a deep professional interest belongs to the Committee for Economic Development and the Joint Council on Economic Education.

The selection of the high schools as the focus for the attack on economic illiteracy is natural given the purpose of disseminating economic knowledge and understanding as widely as possible. A minority of potential voters attends college, and only a portion of these take even a single course in economics. On the other hand, the bulk of the nation's citizens pass through high school, and the majority of these clearly have the ability at the high school level to absorb the basic principles and to gain a good working understanding of economics.

There is another reason for centering attention upon the schools. All schools, whether or not they are aware of the fact, are teaching some brand and quality of economics to virtually all of their students. The question is not whether to teach economics in the high schools; the only question (and it answers itself) is whether to do the job better than it is presently being done. It is as impossible to bypass economics in the high school as it is to avoid economic considerations in making political decisions at the level of citizenship. Any course in the social studies area, and many outside it, must necessarily bring economics into its range of discussion if it is to deal satisfactorily with its own principal subject matter. It is the economic understandings or misunderstandings gained in these courses as well as in "straight" courses in economics that emerge and take form in political decisions that may well be crucial for the performance and even the survival of our democratic institutions and ways.

While economic matters may crop up in a large number of high school courses, most of the economics to which high school students are exposed is encountered in courses in the social studies—economics, American history, and social problems (or problems in democracy and similar courses offered under various titles). The kind and quality of economic understanding gained by the high school student depends heavily on the content of these courses as determined by the economic knowledge and understanding and teaching skill of his teachers and the nature and usefulness of his textbooks and available supplementary material. A major movement, under the leadership of the Joint Council on Economic Education, to improve teacher preparation and training in economics is under way and is gaining ground. As indicated above, supplementary materials available for use in the schools have just been subjected to a vigorous and fruitful professional examination. The present report undertakes to examine the economic content of the principal textbooks used in the high school social studies field in a pinpointed attempt to learn and to make more generally known as a basis and guide for further concerted action just what "economics in the schools" amounts to today.

Arranged for by the Association's Committee on Economic Education early in 1959, and prepared during 1959-60 by a special Textbook Study Committee under the direction of Paul R. Olson, the present report reflects the belief that efforts to improve the teaching of economics in the schools must be built on knowledge—not conjecture—of the existing state of economics in the schools; and that an accurate working picture of economics in the schools today can be derived from an analysis of the textbooks most commonly used in the high school courses in which economics is taught. There is reason to believe that, more than in the case of most other subjects, the teaching of economics in the schools is tied to the textbook. Hence, a study and appraisal of the economic content (kind and coverage) of the textbooks widely prescribed in social studies courses should afford a good overview of the economics which the schools generally are now offering.

Because of heavy teaching loads and extracurricular responsibilities, and particularly because of limited training in economics, large numbers of teachers are forced to rely greatly, if not excessively, on the textbooks they have chosen or that have been chosen for them. There are exceptions, of course, notable exceptions; but it is not denied by those most familiar with the facts that in high school economics, by and large, the textbook is the course. Broadly speaking, to know the nature of the textbooks in the schools is to know the nature of the economics that is being taught. A good case could be made for a critical review by economists of the economic content of high school textbooks in the social studies on grounds common to any professional review of professional writing, and it is to be hoped that one result of the present report will be an improvement in the quality of the textbooks purporting to bring economic understanding to our future citizens. The main thrust of the report, however, is simply to present the picture of economics in the schools in 1959 and 1960 as the picture develops from an examination of typical textbooks then in use.

The Textbook Study Committee was composed of thirteen economists selected by the Committee on Economic Education. The committee was divided into three subcommittees, and to each of these was assigned the task of reviewing the leading textbooks used in one of the three social studies courses. The following economists served as reviewers in the courses indicated:

Economics
C. Lowell Harriss, Chairman, Columbia University
Francis M. Boddy, University of Minnesota
Ward Macy, University of Oregon
Howard Schaller, Tulane University

Social Problems
Kenneth Roose, Chairman, Oberlin College
Clark C. Bloom, State University of Iowa
A. M. McIsaac, Syracuse University
Philip E. Taylor, University of Connecticut
(This subcommittee was reduced to three by the death of Professor McIsaac in January, 1960.)

United States History
Lewis E. Wagner, Chairman, University of Illinois
Clark Allen, Southern Illinois University

Susan Burr, Division of Research and Statistics, Federal Reserve Board of Governors
Chester L. Rich, Cornell College

Paul R. Olson, of the State University of Iowa, as director of the project, assumed the responsibility for co-ordinating the activities of the subcommittees, participated in all subcommittee discussions, and prepared the final report. Edgar O. Edwards, of Rice University, served as editorial consultant.

The Committee on Economic Education is happy to acknowledge and to express its gratitude for the services of all who have labored on this report and to the Ford Foundation for financing the project. It presents the report both as a testimonial to the active interest in and concern for the teaching of economics in the schools felt by the economics profession and as a contribution to the extension and improvement of economic understanding. The report is critical of much that its authors have surveyed. We hope that its judgments will be weighed and used in light of the purposes the project was designed to serve.

Ben W. Lewis, *Chairman*
Committe on Economic Education

Paul A. Samuelson, *President, 1961*

INTRODUCTION

The following report of the Textbook Study Committee is one of a growing number of projects which bear witness to the increasing concern on the part of businessmen, labor leaders, educators, and economists for the improvement of economic literacy in the United States. This committee was established to examine and appraise the economic content of selected textbooks used in high school economics, social problems, and United States history courses, as indicative broadly of the economic content of these courses.

Organization of the Project

Publishers were asked to rank the books available for each course in terms of sales volume. On the basis of these reports the four "leading" textbooks in each area were selected for study. In addition, four other texts in each field were selected without reference to sales volume. Thus, eight books were reviewed for each course, twenty-four in all.

The committee of thirteen economists was organized in three subcommittees of four each, all under a director who attended every discussion meeting held by each subcommittee. Each subcommittee was assigned books for one course. All members of a subcommittee examined the four best-selling books assigned to its group, and in addition, each member reviewed one of the remaining four books selected for review by his subcommittee. Each subcommittee member wrote a single report on the five books he examined. Following discussions of these written evaluations by each subcommittee, its chairman prepared a statement of findings. On the basis of these statements, the project director prepared the present report for the committee as a whole.

Problems of Evaluation

The members of the committee are fully aware of the pitfalls involved in undertaking to evaluate texts for high school use. Clearly some standards for judgment were necessary; yet such standards are bound to be personal and in some measure arbitrary. The problem might have been avoided if the committee had been satisfied with a simple factual description of content, but, of course, only a critical analysis and description could convey meaning and be of significant use. The problem could not be avoided without making the committee's report little more than an empty gesture.

It would be improper, however, for this committee of economists, whose experience has been largely in college teaching, to undertake to design ideal high school textbooks for use as standards of comparison. The committee instead examined the selected texts as economists seeking to determine the scope and nature of the economic content of the books and to consider their adequacy in terms of that minimal economic understanding necessary to good citizenship. While "minimal economic understanding" was not formulated precisely by the committee, a standard consistent with that later set forth in the National Task Force Report, *Economic Education in the Schools,* was, in fact, employed in arriving at judgments on such matters as coverage, depth of treatment, and approach.

These criteria can be summarized as follows:

1. The principal objective of high school education in economics should be good citizenship, not the preparation of students for a college major in economics.
2. Economics is a social science and emphasis should be placed on the interdependence of decision-makers and the operation of economic systems, not on the solution of problems of the individual.
3. The economic understanding sought should concern vital matters, not trivia; and the coverage of these should be balanced, including (as examples) macro- and microeconomics, the generation of change in a system as well as its static operation, and international as well as domestic problems.
4. The approach to economic matters should be essentially analytical, though larded heavily with factual and descriptive material on economic institutions and their development.
5. The nature of value judgments should be explained; whenever relevant they should be identified; and the role they play in shaping economic systems, policies, and controversies should be clearly stated. Controversial issues should not be avoided, but used to stimulate interest and to distinguish between facts, value judgments, and impartial analysis as these apply to vital matters.
6. Factual and analytical errors should be kept to a minimum.

To apply these criteria to textbooks written for different courses, considerable flexibility was needed. A textbook on U. S. history should naturally pay more attention to the generation of change than to the static operation of an economic system; the economic content of a textbook on economics should be more extensive than that found in a book on problems of democracy. With this element of flexibility the committee found the criteria useful and is agreed that they are reasonable.

Summary of General Conclusions

It is the committee's considered opinion that the high school student whose knowledge of economics has been acquired through courses circumscribed by the textbooks principally used in the three social studies courses would be quite unprepared to cope understandingly with most problems of economic public policy.

1. *Most Texts Are Oriented Around the Individual.* Much of his time, particularly in economics courses, would have been devoted to "how-to-do-it" consumer education replete with advice and admonition frequently cast in a high moral tone. The apparent effort to make economic matters realistic by presenting them from the point of view of "you as an individual buyer or worker" has often been carried to such extremes that there is little space left for the economics of society. How an economic system operates is seldom a topic for analysis; the sensitivity of markets and the role of prices, profits, and wages in the allocation of resources are only occasionally paid even lip service; and where economic topics are considered, the emphasis is frequently on how they affect "you." The emphasis on the individual appears to some extent in all texts but is particularly characteristic of those written for the economics course (though the product mix varies) and least obtrusive in the texts for United States history.

2. *Significant Topics Are Omitted; Others Receive Unwarranted Attention.* Our budding voter would have read a good deal which must be classified as economics, but an unfortunately large part of this would probably be regarded by most economists as trivia. And even among the extensive array of economic topics to which space is devoted, there are significant gaps—topics which are either slighted or omitted entirely. Perhaps the most glaring of these is the aggregative or macro approach to economics which is inadequately presented, if touched on at all. This omission was typical of books for all three courses. The determinants of economic growth and policies designed to promote it received scant attention in economics and problems texts, and (perhaps of greater significance) in history texts as well. Certainly it is a matter of great concern to those interested in promoting economic understanding when history texts neglect the pressing economic problems which grew out of the Great Depression and World War II. Yet little can be learned from the history books examined about fiscal policy, the role of the Federal Reserve System, and policies related to growth, employment, and stability.

The failure to consider many of the economic functions and consequences of federal, state, and local governments was typical of textbooks for all courses. The books for United States history, for example, consider the regulatory activities of government in some detail, but pay little heed to the impact of government taxation and expenditures on resource allocation, unemployment, income distribution, and the institutional framework within which decisions are made. Economics texts, too, tended to avoid discussion of such governmental functions or effects as maintaining stability, furnishing services not provided efficiently by private enterprise, modifying income distribution, and achieving long-run national goals. Certainly a citizenry without an understanding of the economic roles of government at the various levels is not well equipped to vote on matters of governmental policy in the economic sphere.

Several other matters of economic import receive equally scant attention. Technological change, product differentiation and non-price competition, productivity, the concept of welfare, and the economic role of flexible relative prices are among the topics which are scarcely discussed. In many texts, the concept of comparative advantage and the whole area of comparative economic systems are barely touched upon. The omission of some topics, such as marginal analysis, the theory of the firm, and the theory of demand, is probably appropriate for high school texts, though the basic ideas involved might be introduced without the technical apparatus.

The omission of many topics vital to economic understanding is made even more apparent by the unwarranted weight given to topics of less significance. In economics books, consumer economics and cooperatives are generally given disproportionate attention. The texts for courses in social problems devote much space to conservation (as many as ninety-eight pages in one case), social security, and housing. It is not that such topics are unimportant, but that the opportunity cost of treating them in length and detail is too great.

3. *Routine Description Dominates Analysis.* Perhaps the most alarming characteristic of textbooks in all three courses is the dominance of description over analysis in the treatment accorded those economic topics selected for discussion. Considerable description of economic institutions is clearly necessary at the high school level, but an extended parade of facts, without reference to the function and effects of the institutions and processes to which they relate, contributes little to economic understanding. Our high school graduates should know essential facts about factors of production, business organization, the banking system, labor and antitrust legislation, taxes and government expenditures, and the like, but it is at least doubtful whether they should be induced to memorize details about the routine operation of banks, miles of track, specific provisions of currency legislation and tariff acts, provisions for slum clearance, and

qualifications for membership on the National Labor Relations Board.

The real offense against understanding committed by these books, however, is that the endless cataloguing of facts is often regarded as an end in itself. Can a student be said to understand the operation of an economy when he knows the routine operations of banks but nothing about the role of money, credit, and interest; when he knows legislative acts but nothing about their purposes, effects, and limitations; when he knows about miles of track, but nothing about the impact of changes in transportation on relative prices and resource allocation. In short, when he knows trivial institutional detail but little of its significance for the structure and functioning of the economic system? The facts are that these texts do not employ simplifying assumptions or discuss their usefulness in stimulating clear thinking; they do not pose or develop hypotheses and test them with evidence; few chains of reasoning are presented and these often stop short of meaningful conclusions. "What" is detailed; "why" is ignored.

The organization and method of approach adopted in most of the texts comes close to eliminating analysis by definition. Using the impact on the individual as a focus suggests a limited analysis of interdependence; emphasis on real surroundings does not encourage abstract reasoning; the obligation to give advice often conflicts with objectivity; and organization along lines of bare chronology detracts from the analysis of relationships. Whatever the reason, it is clear that the student whose economic knowledge is derived from these texts will not have engaged in economic analysis; he will be some, but not much, better prepared to deal analytically with new economic problems with which he will be confronted than is his fellow of equal native ability who has not studied these texts.

4. *Value Judgments Are Seldom Identified or Examined.* The nature of value judgments, their role in determining the objectives of an economy and the economic system developed to attain them, how they differ from one country to another, and how they have changed and are changed over time are simply not discussed. This statement should not be taken to mean that value judgments are not employed in these texts. On the contrary, they are, though usually in a nebulous manner (something is "fair," "just," or "equitable," without definition), implicitly without identification, or by way of commandment (one "should," "ought," or "must"). Seldom is an effort made in discussing policy matters to distinguish between facts, analytic conclusions, and value judgments. On obviously controversial issues, all points of view frequently emerge with equal standing regardless of basis or merit—matters which apparently warrant little discussion. Thus true controversy is never analyzed; yet topics are often included simply because

they are controversial and with little real regard for relative significance.

5. *Presentations Are Marred by Some Errors of Fact and Analysis.* Errors of fact appear in all of the texts considered, but the committee feels that they are not of a magnitude to introduce serious distortion. On the other hand, authors frequently substituted unsupported conclusions or points of view for analysis, and where analysis was attempted it was usually superficial and often misleading if not in outright error. History books depict the main function of the Federal Reserve System as the provision of an elastic currency supply through rediscounting operations. Problems books contain many unsupported assertions such as, "wars are fought by the generations that make them, but paid for by the people who come after," and "low taxes add to the money customers can pass over the counter—they are inflationary." Errors of this kind were less frequent in economics texts, but the paucity of analysis in these books was appalling.

6. *Some Redeeming Features Exist.* While these texts do not provide the kind of economic understanding needed for the exercise of responsible citizenship, the committee is agreed that the student who reads them is on the whole better off for having done so. He should be better able to read magazine and newspaper articles with understanding and to communicate more effectively. His knowledge of some aspects of economics, such as the role of international trade, the farm problem, big business, and labor problems should be considerably improved. Some of the problems texts should be particularly successful in arousing student interest and perhaps in stimulating further reading. History books dealt with many factors of economic significance.

Depite these positive values, the principal conclusion of the committee stands: the economic analysis contained in social studies textbooks is distressing in its absence and unfortunate when attempted. It may be hoped that the ample room for improvement which clearly exists will in due course be filled, and the economic quality of the social studies courses in the high schools correspondingly improved. If so, it will require the co-operation of educators and publishers, and of authors whose training in economics qualifies them for the task at hand.

PAUL R. OLSON, *Director*
Textbook Study Committee

ECONOMICS IN THE SCHOOLS

As Disclosed by a Study (1959-60) of the Economic Content of Leading Textbooks Used in High School Courses in Economics, Social Problems, and United States History

TEXTBOOKS IN ECONOMICS

In many respects the subcommittee on texts in economics had the easiest task. The books were written specifically to cover principles of economics and the problem of inferring these from the more general treatments characteristic of problems and history texts was not encountered. Of perhaps equal importance, the members of the subcommittee could feel confident of their ability to evaluate both substance and organization. On the other hand, judgments involving the level of difficulty and technique of presentation appropriate for high school students were made infrequently and with considerable reluctance.

The subcommittee felt that economics, and therefore the textbooks under review, should be primarily concerned with the functions and operation of the economic system, including of course both temporal and geographic comparisons. It was also agreed that the treatment should be balanced in terms of coverage but not so extensive as to sacrifice analysis for trivial description. Controversy should not be introduced for its own sake but when related to critical issues should be handled fully and analytically. It was hoped that the economic system would be analyzed in terms of effectiveness in enhancing the general welfare, with adequate attention given to the characteristics and changing nature of that usually nebulous concept. None of the books examined measured up to these criteria, but the subcommittee hastens to point out that few college texts would pass with flying colors.

Purposes, Objectives, and Unifying Themes

Perhaps as revealing of the nature of the texts as anything else are the purposes which the authors advance for their own books. There is, of course, variation from one book to another but the following objectives dominate: (1) to acquaint the student with the principles of economics; (2) to develop the skills of the student in applying the principles of economics so that he discharges his "everyday" and civic responsibilities; (3) to promote the ability of students to read matter involving economic terminology with comprehension; (4) to develop the student's appreciation of the American system. There is perhaps insufficient recognition that appreciation must be based upon understanding—an understanding that comprehends strengths and weaknesses, our system, modifications of it and alternatives to it. The purposes also have a strong flavor of individual as opposed to system orientation—an approach which pervades most of the books examined.

The principal pedagogic technique used to gain the specified objectives is to relate ideas and concepts to real-life situations. The subcommittee feels that this is a useful approach in moderation but that it is carried to such an extreme in these books that useful abstraction tends to be avoided.

Despite the stated purposes which could provide unifying themes around which to organize economic topics, these books are sadly deficient in organization. They are content to describe separately long lists of activities, institutions, concepts, problems, and policy debates. Little attempt is made to tie units together, to reveal interdependence, or to demonstrate the operation of the system as a whole.

Coverage and Subject-matter Balance

1. *The Neglect of System.* These are books on economics—not books on business management, problems of democracy, or social organization. Yet all fell short of even reasonably adequate portrayal and interpretation of the nature and operation of economic forces. Most of the ingredients needed for system analysis are discussed, but their combination, action, and interaction are left largely to the reader's imagination.

Such institutional features as private property, competition, freedom of contract, and the profit motive are given appropriate attention, and one writer discusses the relationship between political freedom and the maintenance of a free economy. The factors of production are discussed at some length, but how an economic system determines their rewards is scarcely touched upon (though marginal productivity is usually introduced) except in the case of profit, and even the significance of profit for the operation of the system and the allocation of resources is neglected. Forms of business organization are explained in considerable detail but the increasing size of firms is inadequately treated. Competition and monopoly as extremes of industrial organization are described and the need for laws to curb the abuses of private monopoly are recognized. The historical development of banks, the functions and kinds of money, and the use of credit (especially by consumers) are outlined, and brief, though often casual, references are made to the significance of fractional reserves, credit control measures, changes in the interest rate, and the relationship between money and prices. International trade is probably the most adequately treated economic topic. Absolute and comparative advantage are discussed; visible and invisible items are distinguished in the balance of payments; and tariff history and arguments are presented.

How these ingredients are combined by the system to allocate resources, distribute incomes, and compose output in conformity to consumer demand is vaguely treated if at all. The theory of the firm, even in broad outlines, is conspicuously absent, perhaps because of the belief that a rigorous presentation would be beyond the scope of high school students. Two of the eight books contain reasonable analyses of the market determination of prices, but other treatments of the pricing system are clearly inadequate. Price determination under conditions of competition and monopoly is seldom attempted. The elementary discussion of the market adjustment of demand and supply which characterizes these books fails, with one possible exception, to convey a feeling for the nature, scope, and power of the forces of demand and supply which is needed to understand the economy. The role of price and the nature and significance of cost are in fact passed over; the balancing or equilibrating force of small changes is not considered in keeping with a general aversion to marginal adjustments. The complexity of the system—the high degree of specialization, the multiplicity of industries, occupations, and professions, and the involved nature of institutions and transactions—is described, but how and why these complexities arose and how the system imposes order on them are left untouched.

It is not surprising, given this background, that while all books pay passing heed to the Communist system, they do so only by way of dogmatic criticism, with little real exposition or understanding of the competitive threat of such a system; e.g., in the uncommitted areas of the world.

2. *The Submersion of Aggregates and the Government.* Despite the neglect of "system," the emphasis in all of these books is on microeconomics, with only one book dealing at all seriously with aggregative economics and the problem of instability. The one exception offers a good chapter on the measurement of national income and the circular flow of economic activity. But even this treatment of instability and its causes impressed all members of the subcommittee as inadequate. In other books, fiscal policies and central banking policies receive even less attention.

The neglect of the aggregative approach and of the problem of instability has the added consequence that the role of government in the economy is discussed in only a very limited sense. The functions of government and the manner of financing public services are described in each book, but little or no mention is made of shifting, incidence, and effects of taxes, and public borrowing and its economic effects are almost completely ignored. The role of the government as a regulator is not handled well and there is a clear tendency to avoid the discussion of such roles as (1) maintaining stability and helping to establish and enforce "the rules of the game"; (2) furnishing services which cannot be provided efficiently by private enterprise; (3) supplying services—or added services—that are or could be furnished by the free market but where "public policy" demands larger or different provision than would be accomplished by the market; (4) modifying the real income distribution by "welfare" expenditures and by nonproportional taxation; (5) establishing and achieving national goals that extend beyond the horizon of most individuals and firms.

Factors which determine the economic role of governments are also neglected and efforts to evaluate the effects of governmental action against established economic criteria such as the efficient allocation, full utilization, and conservation of resources are noticeable by their absence. The subcommittee is agreed that the effects (good or bad) of governmental action merit more extended and analytical treatment than is offered by any of these books.

3. *And the Economy Is Static.* The evolution of economic systems is discussed in two of the texts through brief accounts of stages of development proceeding from the primitive era to the modern industrial period. Any other evidence of growth and change is incidental. Investment incentives, determinants of saving, the roles of technological change and population growth in the generation of capacity, and problems of achieving the full utilization of capacity are omissions of major importance. The nature of change, conditions conducive to it, and the importance of change and adaptability in rapidly growing economies are scarcely noted.

4. *But Some Omissions May Reflect Wisdom.* In addition to the major areas of omission already discussed the subcommittee noted many other economic topics which were given no more than passing attention. These included such things as relation of long and short run; non-price competition; relations of cost, supply, and production; marginal and average cost; state and local financial problems; the concept of welfare; role of value judgments and the value of objectivity; the economic aspects of "investment" in people; the interrelations of markets; urbanization; transportation costs and specialization; product differentiation and improvement; geographical distribution of resources; and the concept of efficiency. Some of these omissions are to be regretted, but others may be quite justified in the case of a one-semester course in economics at the high school level. In fact, some members of the subcommittee felt that other topics might well

be omitted if there was reason to believe that those remaining would receive more thorough treatment.

5. *The Compensating Consumer.* The space in these books that could be devoted to significant economic topics is not blank, though some of it is filled with trivia. Much of the remainder is allocated to the individual, his job, his spending habits, his role in society, and how he should behave. The expository value of relating the student's own life to the broader panorama of society is undeniable, and the appropriate balance between the two views of an economy is in some measure arbitrary. The subcommittee is agreed, however, that these texts go to the extreme of making the consumer as an individual the focus of economics. "You as an individual buyer or worker" is simply given too much consideration. Less of household—and more of citizen—economics would promote economic understanding. The emphasis on good advice (often exhortation almost to the point of preaching) adds to the apparent overemphasis on the consumer.

On the other hand, the insatiability of human wants and the scarcity of resources to satisfy them are only sketched, certainly not given the treatment they deserve as the principal reason for economic organization. Engel's law is usually treated, as is the law of diminishing utility, but these and other principles are not woven into the fabric of an economic system. They are lost in discussions of how to prepare a family budget, how to buy wisely, and how to finance consumption. They contribute little to the understanding of economics as a social study.

Several other topics receive disproportionate attention in the opinion of the subcommittee. Co-operatives receive a surprisingly large amount of space and the attention devoted to production probably is excessive when one considers opportunity cost. Conservation is also a popular topic and a variety of minor points are accorded attention which one or more members of the subcommittee judge excessive.

It is recognized, of course, that many of these views on balance and proportion involve personal as well as professional bias. The author of any textbook must pick and choose, and most critics will disagree in one respect or another with his selections. The subcommittee is aware of this and is the more amazed at the unanimity of opinion on the books under review.

Balance Between Description and Analysis

With exceptions here and there, the methods employed in these books are properly characterized as descriptive. There are certainly no "long chains of deductive reasoning"—nor even short ones. Analysis, as economists use the term, is elementary in the few spots where it appears. Simplifying assumptions are not employed, and the use of abstraction is in some cases probably unintentional. (Concepts like "equilibrium" or "maximization" do not warrant listing in a single index or glossary.) Hypotheses are neither advanced nor tested.

The books are built around facts and concepts, with varying efforts to relate them to each other. There is little attempt to build one unit on another; concepts, such as "economizing" and "allocation" are not used as pegs on which to hang descriptive and analytical material. What has happened and what exists are depicted, often in absurd detail; the "why" of matters is much less evident.

While this criticism has been amply documented in the process of assessing coverage and subject-matter omissions, it warrants separate emphasis. Not only is description dominant in the textual material, but summaries, questions, lists of terms and suggested topics for special reports—all of which often appear at the end of chapters—appear to be designed to encourage memorization at the expense of thought and understanding. It is not our intention to disparage factual and institutional knowledge—indeed, much of what is presented is indispensable—but the extreme which is exemplified in most of these texts must be deplored. Above all, future citizens should be encouraged to think and to reason about economic matters.

Factual and Analytical Errors and Limitations

The mass of factual information contained in these books is fortunately largely accurate. No thorough check was carried out but only a few errors were spotted in the extensive process of review. The standard of factual accuracy is generally high. Where analysis is attempted, however, the subcommittee felt that it was often misleading and even erroneous.

Judgment on such matters is inherently difficult. Lines between error, oversimplification, and useful abstraction are hard to draw. Clearly the standards of scholarly journals are inappropriate and were not applied; yet even standards set at much lower levels were frequently violated. Choppiness, superficiality, and at times an almost maddening lack of rigor invited strong criticism. A common example is the use of "right," "fair," "just," and "equitable" without definition.

Perhaps a summary of the treatments accorded a particular topic in the four "leading" books would demonstrate the level of analysis. We have chosen the regulation of agricultural prices, but other topics would do as well.

Book 1. The author devotes five pages to the problem in the chapter dealing with land and two paragraphs in the chapter dealing with the determination of output and prices. The first section contains a discussion of the price support and soil-bank programs and an enumeration of the popular arguments for and against the programs. No attempt is made to judge the programs in terms of the alternate use of resources or to discuss the possibility of solving the farm problem by allowing free markets to direct the allocation of resources. The second section consists of two short paragraphs and concludes with the statement that "a fair balance between the supply of grain and its price is needed in the long run if both consumers and farmers are to secure reasonable benefits."

Book 2. The author devotes four pages to the problem in the chapter dealing with the determination of the prices of output. The section begins with a short history and description of the price support program. The author then asks the question, "Has the farm program been a success?" but does not make any attempt to provide an answer. The author says that the withdrawal of land and labor would solve the problem of "fair prices" but that this is "quite a problem." There is simply no analysis of the resource allocation problem, and the author concludes with the statement that the agricultural price support program is "another argument for the need of a clearer understanding of economic matters on the part of all citizens in order that they may vote intelligently on political issues."

Book 3. This book has a chapter on "Farming as a Way of Life." Three pages are devoted to the problems of overproduction and control. The author states that there is very little that the farmer can do and then proceeds to describe briefly the government support program. There is no attempt to evaluate the program or to analyze the problem in terms of resource allocation.

Book 4. This book also has a chapter devoted to agriculture and devotes practically the entire chapter to the government program. The treatment has been extended by the inclusion of more history and by a more detailed description of the programs. The statements of the pro and con arguments about the government program are more concise. Again, however, there is no discussion of alternate use of resources nor any attempt to analyze the rule of the free market.

Other policy problems receive similar treatment. Little effort is made to distinguish between facts, analytic conclusions, and value judgments as basic elements involving different levels of controversy. The student is likely to emerge from his study of a controversial issue with the vague, and probably dissatisfied, feeling that "people (and economists) disagree."

Concluding Observations

This report has been essentially critical: it emphasizes weaknesses, not strengths. Yet a student who studied one or more of these books would clearly benefit from the experience. Much of the descriptive material is both apropos and accurate; some topics, such as international trade and labor-management relations, are well presented; efforts to stimulate student interest are frequently quite promising; and the constructive use of superb teaching aids is to be commended. The gaps noted throughout this report are perhaps more indicative of the difficulty of the task than of the skill of the authors.

Yet our review is disturbing. There is a clear and pressing need for a voting public that has a deeper grasp of economics than is offered in these books. No single format would be ideal, but the subcommittee is agreed that some factual information and much of the space devoted to individual problems should be eliminated in favor of simple analytical models of pricing, resource allocation, and levels of employment and income. Discussions of major problems could utilize descriptive material and analysis as a means of demonstrating economic principles and, above all, economic reasoning. The task is difficult, but the need is great.

TEXTBOOKS FOR SOCIAL PROBLEMS COURSES

There is a great deal of economic material in these books: at least 40 to 50 per cent of the content is economic in character. The fact that the books focus on problems and not economic analysis caused the subcommittee to give very special consideration to the criteria to be used in evaluation. Should social problems texts provide an understanding of the economic process as a by-product of their treatments of American problems? Should problems be selected that facilitate the introduction of elementary analysis in a disciplined way? Should they be presented largely to arouse student interest in the problems themselves? Should the major emphasis be on analytical concepts or upon the general nature of the problems? Should the problems be used to teach economics or should economics be presented to illustrate one kind of problem?

In reaching agreement on questions such as these, the subcommittee was guided by three fundamental value judgments. First, any reasonable survey of important social problems must include many which are basically economic in nature, and a careful treatment of others should reveal that these, too, have important economic aspects. Second, any informative discussion of social problems must be primarily analytical in nature. Third, economic analysis need not be technical and difficult to be useful, interesting, and informative to high school students.

The inclusion of useful analysis in discussions of economic problems was regarded by the subcommittee as crucial. The high school students of today will undoubtedly play an important role in finding, approving, or rejecting alternative solutions (or perhaps only outcomes) to many of the problems which face us today. If students are not trained to approach such problems analytically, their ability to perform effectively in this role will be greatly weakened.

The case for analysis extends even further. We do not now know, nor can we predict with a high degree of confidence, what social and economic problems will face us twenty years hence. The student of today who is served only a description of current problems will not find this a very valuable legacy for the solving of future problems. If it is an objective of high school education to prepare future voters to think intelligently about the problems of their time, the analytical approach must be accorded high priority.

Apparent Objectives in the Selection and Treatment of Economic Problems

Several purposes appear to have motivated the authors of these textbooks. Most could be given at least qualified approval by the subcommittee, but one impressed the members as being of doubtful merit. The principal purpose in all of the volumes is to direct the attention of students to problems of real substantive importance. A second important purpose is to demonstrate the objective analysis and its significance in the appraisal of controversial matters. Closely related to this objective is the attempt to provide practice in the problem-solving process. There is also the general intent to relate all problems insofar as possible to real life situations of which the student might have personal knowledge. Most authors also stress the responsibility of each citizen to form "sound" opinions, not only about problems directly affecting him, but also about the critical issues facing the nation and Congress.

Not all of these objectives are realized equally well, and some, such as the effort to relate problems to the student's experience, are perhaps overemphasized. More unfortunate, however, in the subcommittee's opinion, is the "morality" objective which appears in varying degrees in all of the books. Problems are treated as moral issues; students ought to believe in conservation or social security; taking a stand on issues is a moral obligation. The unfortunate aspect of this objective is that the value judgments underlying many conclusions are not stated explicitly and in many cases there is evidence that the authors are unaware that their conclusions reflect their values more than their analysis.

Coverage and Subject-matter Balance

It has already been noted that between 40 and 50 per cent of these books are devoted to economic matters. Clearly economic issues are given extensive scope and recognition. Nevertheless, one should not expect to find the full range of economics discussed in the limited space available. The authors, confronted with a difficult problem of selection, have not solved it to the satisfaction of the subcommittee.

All of the authors have included sections on such economic topics as agriculture, big business, labor relations, public finance, conservation, and social security. Most of the books devote space also to health and medical care, consumer economics, and housing.

But it is the omissions which are startling, especially in view of the principal purposes to be served by the study of problems. Five particularly significant economic topics are sparsely treated if at all: international trade, business cycles, money and banking, comparative economic systems, and economic growth. The first three of these are discussed in but two of the four volumes examined by the whole subcommittee; the last two are treated in only one. Yet there is nothing in the points of view of the authors nor in the organization of the texts themselves which would suggest why such important issues should be excluded. Perhaps the relative newness of an area (economic growth) or the difficulty of employing modern theory in analysis (money and banking and business fluctuations) may account for some omissions. It is also possible that authors failed to appreciate the significance of some topics or avoided others out of fear of offending public opinion. Clearly, comparative economic systems has been a delicate and sensitive matter since the end of World War II.

In the opinion of the subcommittee, space for these basic economic topics should be provided either by reducing the space allocated to others or, if the authors feel this is not feasible, by eliminating less fundamental problems. While important economic issues are encountered in discussing such problems as consumer economics, conservation, housing, social security, and health and medical care, the subcommittee is agreed that all of these rank well below the major problems which the textbooks slight.

Of course, some of these marginal economic problems may have a broad social significance which would warrant their inclusion. Even in this context, however, most of them are given an extended coverage which is both unwarranted and superficial. One author, for example, devotes ninety-eight pages to conservation and none of the authors treats this issue in less than sixteen pages. The subcommittee is firmly convinced that the selection of topics and the allocation of space among them can be substantially improved.

The Balance Between Description and Analysis

It is at the level of methodology that the subcommittee is most critical of these texts. Their major deficiency lies in the absence of any consistent and effective use of economic analysis. To a very considerable extent, the subject matter is treated descriptively. Even here the descriptive materials are largely definitional; the authors catalogue facts but provide little preparation for understanding them. Legislative acts are described with only limited indication of their purposes and results; institutional detail is provided but with little interpretation of its relationship to the economic process.

A good case can be made for the liberal use of description in acquainting high school students with a functional view of society. Many questions about the functioning of the economic system can be answered only with the aid of descriptive materials. What is the free enterprise system and what are its distinctive institutions? What are the functions of factories, farms, transportation, wholesalers, retailers, organized markets, and financial institutions in the economic process? What are the economic aims or objectives of the society? Answers to all of these questions and many others require description whether generalized, classificatory, or functional.

The need for descriptive materials is granted, but these materials should be carefully chosen and limited to digestible quantities. Too often, however, the books under review fail on both of these counts. Descriptive materials are excessive and often of very minor significance. These are books dealing with social problems; they are not law books or history texts. Yet page after page is devoted to describing such things as qualifications for membership on the National Labor Relations Board; monometallism, bimetallism, and other monetary standards; details of legislative acts; and minor facts about regulatory machinery.

While factual and descriptive materials must be selected and used with discretion, it is also important that they be used to increase understanding of the economic system, of its underlying relationships, and of the fundamentals of its operation. Too frequently these texts treat description as an end in itself; too often, intimately related institutions are described independently with little or no attempt to reveal interdependence. Analysis is in very short supply in these texts.

Limitations of Fact and Analysis

1. *Errors of Fact.* Errors of fact and definition were not investigated systematically, but several were noted. The subcommittee is of the opinion, however, that the standard of factual accuracy attained by these books is reasonably satisfactory.

2. *Some Examples of Analytical Limitations.* Much less can be said for the authors' attempts at economic analysis. Many fragments of analysis end at the definitional level. With few exceptions more extensive attempts at analysis fail because they take the form of developing meaningless or outmoded concepts. Much economic reasoning is presented in an unnecessarily complex and garbled fashion—one likely to discourage any student from attempting it on his own. Finally, a lack of continuity in much of the analysis contributes to the difficulty of understanding.

Numerous illustrations of analytical weaknesses are available. Perhaps a few will convey some idea of the character of these books. The omissions of analysis are startling. In the book which contained more conventional economic topics than any other examined, the central problems of economics—resource allocation and employment—are hardly discernible amid the mass of descriptive detail. Demand and supply are defined but never related to problems of normal production, prices, and profits. The significance of large-scale business enterprise and the critical problems posed by structural changes in agriculture are subordinated to a recital of the various regulatory and assistance acts of Congress. Another author discusses the pros and cons of private power development and regional planning largely in terms of pressure groups without devoting a line to economic tests of efficiency. Still another explains the law of diminishing returns in the midst of materials that are completely irrelevant—and never uses it as an analytical tool.

The omission of analysis does not prevent authors from reaching categorical conclusions which are, of course, completely unsupported. Some have an element of truth, but even this is not demonstrated, and others are misleading, if not in error. The following are examples:

> Sound fiscal policies require governments to live within their incomes and not borrow money for current operating expenses.
>
> Sound fiscal and financial policy requires that income and outgo balance.
>
> Deficit financing by government is inflationary.
>
> The great depression sent the government on a spending spree from which it never recovered.
>
> Wars are fought by the generations that make them, *but paid for by the people who come after.* (Italics in original.)

> Low taxes add to the money customers can pass over the counter—they are inflationary.
>
> Is [small business] important to you? It is, if you are concerned with being able to buy what you need and look forward to continued high standards of living.

The subcommittee can only hazard guesses as to why so little careful analysis is employed in these books. There is evidence that the authors are not well versed in economics. It may also be the case that they avoid analysis because they think it is too difficult for high school students. We can offer little beyond the obvious guidance on the former; but the subcommittee does feel that the latter reason represents a fundamental misunderstanding of economic analysis. Our criticism is not that these volumes fail to present a high degree of theoretical abstraction; we do not expect and would not recommend abstract model building in these courses. In fact, it is probably true that relatively few of the technical tools of economic reasoning are necessary to a careful and meaningful treatment of economic problems at the high school level. A few simple tools of analysis, however, are necessary, and an analytical approach is imperative if economic understanding is to be imparted.

Perhaps as a substitute for further criticism an example of what we have in mind would help. We offer below a brief, nontechnical discussion of the farm problem to illustrate the kind of economic reasoning that is useful at the high school level.

3. *The Farm Problem: An Illustration.* At bottom, the problems of American agriculture stem from the fact of farm incomes which are generally low in relation to the total investment by farmers in physical facilities (land, buildings, machinery, equipment, and inventories) and total human resources employed (management and labor). The task of the economist in this situation is three pronged: (1) to explain why incomes in agriculture are low; (2) to describe the reactions of farmers to these lower incomes; and (3) to specify the probable consequences of these farmer reactions on the economy as a whole. In addition, in the context of economic education, the economist has the additional task of showing how a study of the farm problem contributes to an understanding of the functioning of the economy as a whole.

Why Incomes in Agriculture Are Low. In the sort of economy existing in the United States today, incomes in any industry are low when demand for product is low in relation to the volume of resources committed to its production. This is the case in agriculture. But why?

First of all, demand for agricultural products has not grown rapidly.

This is a consequence, in turn, of two characteristics of the industry: (1) The fact that consumers do not expand their consumption of food and fiber rapidly as incomes rise. Demand grows only slowly with income. (2) The fact that demand-increasing development of new products is slow in agriculture.

. In the second place, output per man has increased rapidly in agriculture—more rapidly than in the nonfarm sector. This has been due to substantial additional use of continuously improving machinery and equipment (an additional investment in physical facilities and prompt application of new methods—an application which increases output of existing facilities and men) and growing use of off-farm produced inputs formerly produced on the farm. Thus nonfarm capital and men rather than farm resources produce such items as feeds, farm motive power, and chicks; and total resources devoted to agricultural production are increased.

Competition to cut costs is the force that impels individual farmers to embrace new machines, methods, and off-farm produced inputs. As a result, output per man rises. Since demand grows slowly, farm labor is in long supply and its price falls. Farm incomes are low.

And, of course, there is no relief, as one might expect, from a large expansion of purchases accompanying lower costs and resultant lower prices. Just as demand for agricultural products grows but slowly with income, the quantity of agricultural products purchased grows but slowly with falling prices.

The Reactions of Farmers to Low Incomes. Observers of the agricultural sector of the economy can easily detect at least six reasonably important reactions by the farm community to declining farm incomes.

First, individual farmers will each see the necessity of investing additional funds in new cost-reducing machinery and equipment and —as each sees a portion of his time unemployed or ill-used on farms which are too small as output per hour rises using the new machinery and methods—in additional land. Since each farmer is likely to have limited financial resources, each will clamor for additional credit to be made available on favorable terms. Thus, beginning as early as the twenties, a succession of special programs and agencies for "easy credit" to farmers has appeared.

Second, farmers will see prices paid for needed farm inputs as "too high." This view will be particularly strong the larger the size of firms with which the farmer deals. The cry of "monopoly" will then be raised—sometimes with justification. As a result, farmers have generally supported railroad rate and service regulation and vigorous antimonopoly laws and their administration, and participated actively in the development and use of co-operatives in an attempt to obtain needed farm and consumer inputs at lower prices.

Third, farmers will from time to time attribute their low incomes to inadequate efforts and programs to expand markets, and will look toward market-expanding activities on both domestic and foreign fronts. Domestically, farmers have sought expansion of demand by encouraging research directed toward the industrial use of farm products and attempts to develop improved products (e.g., leaner pork) and thus expand consumption. With regard to exports, farmers have looked to improved marketing techniques and products to stimulate overseas sales, lower United States tariffs on nonagricultural products to enable other nations to sell more to the United States and, thus, earn dollars to buy United States farm products, and relief and/or development shipments of farm products abroad on either a gift or loan basis.

Fourth, farmers will see prices obtained for their products as "too low." They will ask for higher prices guaranteed by a sympathetic government. But, to obtain these higher prices, the quantity of farm products actually going to the market to be sold must be reduced. This can be done either by storage of some part of the marketable output (hoping for better market conditions—and higher prices for expanded marketings—at some future date), or by restrictions on production. Thus, agriculture has witnessed a variety of "price support" programs coupled with attempts at reduction of supplies via storage programs, acreage restrictions, the "soil bank," and the like.

Fifth, farmers will see prices of food and fiber as falling rapidly as markets thin during depressions. Farmers thus have a special reason to regard business slumps as undesirable, and it is not surprising to find farm organizations as active supporters of stabilizing monetary and fiscal policies. More than this, price support and storage programs have sometimes been seen as proper only for periods of unusually low farm prices and incomes during recessions—with stored accumulations to be sold at higher prices during subsequent booms.

Sixth, farmers leave agriculture for higher income, nonfarm jobs. Since this is most easily done when nonfarm employment is high during prosperity, this reaction to low incomes depends also upon the maintainance of high levels of economic activity. It also requires adequate funds to finance the move, and the acquisition of necessary education, training, and skills to allow the ex-farm person to compete successfully in urban areas.

Consequences of Farm Reactions. The consequences of each of these reactions are:

First, additional investment and improved methods in agriculture serve simply to intensify the problem of low returns to labor. By adding still additional productive resources and by improving the output of existing resources, output per man increases rapidly, while demand grows (with population and income) only slowly. The result is simply a continued excess supply of labor in agriculture (even with a large, continued out-migration of farmers).

Second, attempts to broaden markets, while intrinsically desirable, have come up against the hard realities of failure to discover, despite considerable effort, important new industrial uses of farm products, consumer choices which turn toward new, improved food items largely at the expense of other farm outputs, and growing competition for export markets from farmers in other countries, many of them given tariff protection in their own countries and subsidies abroad.

Third, while utility regulation and antimonopoly actions give a measure of protection to farm (and other) incomes, they provide little in the way of positive improvement. It seems unlikely that, after decades of regulatory activity, there remains much in monopoly profiteering to be abated.

Fourth, price supports, while undoubtedly contributing to the maintenance of farm incomes, have given rise to subsidiary problems. At the level at which prices have been supported, storage operations have in recent years accumulated large stocks of many commodities which have been acquired and maintained at substantial cost to the federal government. On the other hand output restrictions are difficult to enforce equitably on millions of farmers. And if output restrictions are to be voluntary, they are expensive to administer, as the farmer is literally paid to allow his land to lie idle or put to uses allowing lower returns. Furthermore, support prices at levels higher than unsupported levels discourage the broadening of product demand which farmers also seek.

Fifth, the problem of low farm incomes is not just cyclical. While depression aggravates the problem, prosperity does not eliminate it.

Sixth, the movement of labor—and land—out of agriculture is clearly the adjustment required (assuming that a significant stimulation of demand continues unachievable). Equally clearly, recent decades have witnessed sharp reductions in the amount of labor used by agriculture. It is only unfortunate that the massive movement of agriculture has been accompanied by equally rapid increases in output per man so that incomes remain relatively low.

The Economy as a Whole. Of course, no discussion of agriculture should focus exclusively on the farm sector of the economy. At least two relationships of agriculture to the rest of the economy should be noted.

By increasing agricultural output while reducing both land and labor requirements, the farm sector is freeing resources for the expansion of total production. Growth in the total economy is, thus, in an important sense, made possible by an increasingly productive agriculture.

In addition, developments in agriculture have important repercussions for these nonfarm businesses meeting farmer needs. For them, a shrinking number of farm customers are offset by higher incomes for the fewer remaining farm families, growing purchases of farm inputs by an increasingly mechanized, scientific, commercial agriculture, and expanding outputs of agricultural products for assembly, transport, processing, and marketing. Thus, the whole pattern of demand for farm-related, nonfarm activities is changing.

On Teaching Economics. But the "farm problem" is more than simply an interesting and an important problem warranting attention in its own right. Its study contributes much to an understanding of economics as a social science.

As the farm problem is studied, it allows the student to note systematically many important facts about the economy. Some of them are listed below:

1. Decision-makers (in this case, farmers) respond to opportunities defined for them by prices (of goods sold, of inputs purchased) determined in the market place. Students can see that what and how much farmers produce and the techniques of production selected depend upon these market-determined price alternatives. They can observe directly how a system of prices and markets organizes economic activity.

2. Optimum response by individual farmers to price alternatives leads to over-all low incomes in agriculture and therefore both to group (largely governmental) actions to modify market alternatives and to movement of resources into other more promising sectors of the economy. Students can see a government response to dissatisfaction by a politically powerful economic group. They can also witness the discipline of a market-organized society as it both pushes persons from agriculture and pulls them into alternative economic activities.

3. The consequences of government actions must be interpreted within the framework embracing their impact on market alternatives. Thus a government action making credit favorably available to farmers to add to their capital facilities leads to market alternatives further reducing returns to farm labor. Or, price support programs both increase difficulties in competing in export markets and create problems of mounting surpluses of agricultural products. (Incidentally, the surplusses are a consequence of an attempt to solve the fundamental farm problem—that of low farm incomes. They are not the farm problem.)

4. The well-being of any sector of the economy is strongly influenced

by the over-all health of the economy. Thus the student can see the farmer as better off if he moves from the farm if the economy is characterized by prosperity, not recession.

5. The decline of one sector of the economy (in this case agriculture) is not inconsistent with growth in the entire economy.

. It should be possible to apply elementary economic theory in a similar way to other problems. Such a development of theory would provide the basis for much more adequate analysis than that now available. The essentials of macroeconomics might well be developed in terms of the circular flow of income and expenditures. Perhaps the concept of equilibrium should be avoided, but enough can be done in this area to build a background for the study of economic stability and growth. Facts on the distribution and stability of national income could be presented as groundwork for the theoretical analysis. The relation between the supply of money and the price level also would seem to be a possible subject for analysis at the elementary level. Descriptive analysis (such as that attempted above with the farm problem), if pointed directly to the solution of some vital problem, could be made more meaningful than it is in the books under review. Description should be approached from an analytical point of view so that a limited cause and effect theorizing may be undertaken. From analysis of this sort the student should learn that when he thinks about economics, he must continually ask himself, "Why?" If he persistently raises this question with himself, he will be well along the way toward understanding the need for cause and effect analysis in economics.

Concluding Observations

The problem approach as a method of stimulating student interest and as a means for conveying economic understanding is probably among the most useful at the high school level. The subcommittee is agreed, however, that the books under review fail to realize more than a small part of the potential of that approach. While several criticisms have been made during the course of this report, two in particular merit reiteration here.

The economic content of these texts could be substantially improved through a more judicious selection of problems. As they now stand, many fundamental and significant economic topics are excluded in favor of relatively minor economic issues. Some of these minor topics could be treated better in a much shorter space, while others could be eliminated with very little sacrifice of economic understanding. Clearly, given the space limitation, the topics selected should be the most vital and instructive rather than the most controversial or the easiest to describe. We would classify as fundamental such problems as those related to resource allocation and employment, the operation of the pricing system, price stability, economic growth, and comparative systems. Not only are these problems among the most exciting and critical public policy issues but they lend themselves readily to the clarification of economic principles and reasoning.

But almost any economic problem can be used as a springboard for teaching economics. In this sense the deficiencies in scope already noted are not as disturbing as the lack of economic analysis employed in the examination of each problem. Since most of the content of the books is descriptive, it is not surprising that economic principles and the economist's way of thinking fail to emerge. One does not gain from these volumes an awareness of fundamental economic problems or how economic analysis can contribute to their solution. Yet the authors are not unwilling to draw unsupported conclusions. Finally, if the subcommittee may be allowed to venture beyond its mandate, the deficiency of careful analysis appears to carry over into those problems which involve other disciplines, such as sociology, political science, and psychology.

While a more careful selection of topics would enhance the value of these texts, a more significant improvement could be obtained if the authors and their consultants would familiarize themselves with economic principles and then endeavor to cover essential issues in terms that reflect good, simple, up-to-date economic analysis.

TEXTBOOKS IN UNITED STATES HISTORY

Textbooks for courses in U. S. history are not designed primarily to delineate economic principles. Yet so few high school students take a formal course in economics that much of their economic knowledge must be derived from such ancillary courses as problems of American democracy, civics, geography, world history, and United States history. United States history is an especially significant source of economic understanding to examine because it is taken by almost all students.

Explaining the structure and operation of the economy and charting the course of our nation's history are far from identical tasks. Yet much of the history of the United States is to an appreciable degree economic in nature, and any substantial text in this area should include sound economic as well as social and political analysis. The subcommittee did not attempt to evaluate these books as history texts, nor even to assess the relative importance they assign to economic forces in historical explanation. The texts were examined only in terms of the economic understandings they could transmit. Thus the omission of a vital economic topic (say, fiscal policy) would be noted, although historians might not consider its inclusion fundamental to an understanding of history. Our frame of reference was the major economic areas, ideas, and relationships with which economists are concerned. It may be in order to note at the outset, however, that the dominant themes of these books are social and political and the omissions in the economic field are both numerous and significant.

Coverage and Subject-matter Balance

Some of the texts place more emphasis on the economic aspects of United States history than others, but in no case have economic matters been entirely neglected. In fact, if one were to judge the economic content of these books simply by the number of casual references to economic forces, events, legislation, and developments, nearly all would receive high marks. To the economist, however, it is the topics omitted and the cavalier treatment accorded those included which are disappointing.

While all topics touched upon in these texts cannot possibly be listed, the following command most attention with considerable variation, of course, from one book to another:

1. Tariffs and protectionism
2. Conflicts of economic interest: (*a*) sectional or regional conflicts; (*b*) conflicts among major interest groups (e.g., management and labor, agriculture and manufacturing)
3. Currency and banking
4. Industrialization, the growth of big business, business organization, and government regulation of business
5. Problems of labor, the growth of unions, and labor legislation
6. Transportation and communication
7. Agriculture, farm problems, and related legislation
8. Financial panics and periods of prosperity and depression
9. Population and immigration
10. Inflation, deflation, and the national debt
11. Conservation
12. Public finance (i.e., federal taxes and expeditures)

Clearly, a teacher well trained in economics could use texts such as these as a real springboard to economic analysis. But the teacher or student without training in economics could not be expected to complete his reading of the text with a genuine understanding of what the important economic problems are, much less how to analyze or cope with them. Measured in terms of the content of economics, this list demonstrates that the balance of economic topics and points of emphasis leaves much to be desired. Many economic topics that are relatively unimportant for purposes of economic understanding are included, other marginal items are given excessive attention, while some really significant economic matters are omitted entirely.

The coverage and balance of topics varies considerably from one historical period to another. It is tempting to suggest that the authors in dealing with each period attempted to apply only that economic knowledge and analysis which was then prevalent. The hypothesis would suggest, however, a treatment for the period since the advent of the New Deal which would differ considerably from the analysis of earlier periods. Such is not the case. Instead, this recent period is analyzed, if at all, with the tools of years gone by.

As a matter of fact, economic events of the last thirty years are accorded far less attention than those of any other period of similar length since the Revolution. The period since 1930 is treated largely in terms of social and political consequences with particular emphasis given the emergence of the United States as a world power. Some attention is given legislation bearing on agriculture and regulation of industry and labor, and mention is made of the Marshall Plan and Point Four. All of the books make reference to the Great Depression, but in only one is an extended treatment attempted. In general, the discussion shifts quickly to the New Deal. Note is taken of the problems of inflation and the national debt, but no consideration is given the Employment Act or monetary and fiscal policy. Economic matters recede into the background at a time when many economists would feel their historical significance was growing.

The relative neglect of recent history means that many economic topics discussed in detail in earlier periods are largely ignored in modern context. Discussions of public finance focus attention on the magnitude of federal taxes and expenditures rather than the nature and role of fiscal policy. Problems in agriculture are related largely to marketing situations and money supply limitations rather than to the allocation of resources in the face of excess supplies created by mechanization in agriculture and the immobility of labor. Government regulation is given disproportionate attention as compared with more recent governmental activities of a supporting or stabilizing nature. The heavy emphasis on currency and banking prior to 1900 coupled with the meager treatment of the Federal Reserve System and recent monetary history produce a distorted picture of our modern banking system and the nature and role of monetary policy. The financial panics and depressions of earlier periods are emphasized, but the problems of achieving rapid economic growth, full employment, and price stability are largely ignored.

It would certainly be surprising if a student emerged from a course based on these books with an understanding of the structure and operation of the modern economy. Perhaps this objective is idealistic for a history course, but it would be equally surprising if our student emerged with any understanding of modern economic analysis, which we feel could be demonstrated in the context of many historical situations.

The substantial neglect of the economic aspects of recent history undoubtedly has played an important role in leading authors to devote excessive attention to relatively minor economic topics—minor in the economic, not necessarily the historical, context. Frequently extensive discussions are offered of economic matters which may have had political and even economic significance in some earlier period but which, at least as treated in these texts, have little or no significance for understanding the current operation of the economy. Many of the topics in the list above fall in this category but the treatments accorded tariffs and currency are perhaps indicative.

Tariff legislation is examined at every stage of the nation's history, often in minute detail. In the pre-Civil War period, no other economic topic is accorded more space. It is often treated as a means of identifying and clarifying sectional interests and conflicts. The matter is given relatively less attention in treatments of the latter half of the nineteenth century but it again achieves considerable dominance in the post-1900 period. So far as economic knowledge is concerned, the excessive treatment of legislative details is more a deterrent than it is an aid.

Currency, too, occupies a central position in these books. The scarcity of gold and siver in the colonial period and the problems occasioned by a lack of a uniform and adequate currency are related to early patterns of trade. Jackson's attack on the national bank, the "specie circular," the financial panic of 1837, and the establishment of the independent treasury are all discussed. But the student is left with the impression that monetary problems and policies are currency problems. The Federal Reserve System is scarcely noted, in one text being given two fragmented references which together constitute less than a single page of text. Deposit money and the uses of monetary policy are seldom mentioned. Instead the student is given currency legislation—a poor substitute from the point of view of economic understanding.

The foregoing discussion gives some indication of important economic topics omitted in these books. Clearly, significant gaps in coverage limit the contribution these books can make to economic understanding and at the same time increase the burden on the teacher who in all probability is not well trained in economics.

The main function of the Federal Reserve System is pictured as providing an elastic currency supply through rediscounting. The role of the Federal Reserve System in economic stabilization and the tools of money management are completely ignored. Even prior to the New Deal, the discussion of money and banking is characterized by an excessive treatment of the facts of currency legislation as compared with the role and importance of fluctuations in the cost and availability of money and credit. Despite the heavy emphasis on political history and legislation, only one of the texts mentions the Employment Act of 1946, and that was a reference of only a few lines appearing in a section entitled "Aid to Business."

There is no discussion of the basic economizing problem confronting any nation, or the circumstances which give rise to the problem. Questions as to what an economic system is, why it is necessary, and what functions it performs are never raised. How a system of prices and markets can be used as a method of organizing economic activity and how the market allocates resources are economic topics of major importance that find no expression in these texts. The impact of industrial concentration on resource allocation and the distinction between business firms as controllers of the market rather than as controlled by the market are nowhere treated.

The general failure to discuss the nature and role of a market system also characterizes the material on foreign trade. By and large, these texts provide an unbroken chronicle of the facts of tariff legislation, but nowhere is the reader made aware of the concepts of comparative advantage and specialization and division of labor. The case for freer trade and the problems of maintaining international economic balance and organizing the world's resources for mutual benefit are also neglected. Although perhaps more appropriate for a course in world history, the problems of economic growth and development in the underdeveloped nations of the world receive virtually no consideration.

The increased role of government in economic life is noted and documented largely in terms of its regulatory activities, but the economic significance of government policies as related to their impact on resource allocation, the level of income and employment, the distribution of income, and the institutional framework within which economic decisions are made is rarely discussed.

Balance Between Description and Analysis

The possibilities of teaching economics through the course in United States history are considerably diminished given the important omissions discussed above. But the contribution these texts may make to student understanding of economics turns also on how the topics which remain (and they are extensive) are treated. Unfortunately, the judgment of the subcommittee in this regard is not favorable.

For the most part, the treatment of economic matters is chronological rather than topical. Occasionally the chronological narrative is interrupted to pull together the scattered discussion of particular topics over a prolonged period. Typically, however, the treatment is geared to cataloguing the occurrence of economic phenomena and issues of public policy in their appropriate chronological niches.

In large measure the economic topics selected and the treatment accorded them appear to have been determined by conflicts of interest, legislation, and dominant personalities of the period. Rarely are economic facts, relationships, and processes presented in terms of their significance for the structure and operation of the economy. The treatment is frequently marred by the inclusion of many trivial items and extremely superficial consideration of significant topics.

Although there are some attempts to deal with economic topics analytically, the approach is predominantly descriptive. The growth and development of transportation, for example, is related to the conquering of the frontier, with the emphasis on things (such as miles of track, the airbrake, the refrigerator car) rather than ideas. Little attention is given to transportation as a process which has demonstrable economic effects. The relationship of transportation to specialization, division of labor, extent of the market, and interdependence are usually overlooked. Attention is focused on the instruments of transportation (steam boats, trains, planes) rather than the impact of changes in transportation on relative prices, industrial location, or employment opportunities.

Frequently the discussion of economic topics is conducted in terms of personalities, institutions, and legislation rather than the causes and effects of basic economic forces. In the discussion of big business, for example, there is little attempt to explore the significance of possible relationships between technology, business size, and efficiency. The tone of the discussion implies that big business is necessitated by technology and is obviously very efficient but may, at times, misbehave unless subject to government regulation. The dates and major provisions of antitrust legislation are properly entered in the ledger, but there is little, if any, attempt to explore problems of enforcement and administration and to tie these into the basic problem of bigness.

Factual and Analytical Errors

While the chronological and descriptive approach dominates these texts at the expense of analysis, it is also true that they contain several errors of fact, many errors of analysis when analysis is attempted, and conclusions based on value judgments rather than fact and analysis. An extensive list of such limitations is documented in the working papers of the subcommittee.

In general, there is a disproportionate, or even misleading, offering of facts as facts, as distinct from presentation of the type, amount, and mode most helpful for economic analysis. This is particularly evident in the handling of organized labor and big business. In many instances sources of data are not identified for the charts presented. Summaries of legislation are not always up to date, and a number of sheer factual errors were discovered. For the most part, however, the actual errors of fact are not of a magnitude which introduces serious distortions and could be substantially reduced by more careful editing.

From the standpoint of adequate treatment the most serious errors are to be found in the areas of interpretation and analysis. There were also a number of instances where the authors presented conclusions or points of view instead of analysis. Inadequate treatment of this character was especially prevalent in the area of money and banking. Currency, for example, is represented as the money supply, long after this ceased to be the case, and the functions of the Federal Reserve System are not realistically presented.

Summary and Concluding Observations

In general, the economic forces recognized in these texts are those which could hardly be ignored in any treatment of the nation's historical development. All books, for example, recognize certain persistent problems, especially the problems of farmers, the growth and power of big business, and the struggle of labor to protect its interests. But they omit important topics; they miss opportunities to relate topics to underlying economic forces; and analysis of economic matters is extremely sparse. In all of the books economic topics are handled descriptively, and opportunities for analysis are bypassed.

Attention is focused on the political manifestations of economic forces rather than on an analysis of basic economic relationships. Consequently, the economic interests and motivations of various social groups and geographical regions are employed primarily as background to explain political positions and decisions. Rarely are economic forces analyzed as important determinants of social action outside the political sphere, or as a basis for analyzing and illustrating the allocative role of a market economy and the price mechanism.

The presentation of economic topics is intermingled with noneconomic topics, and the importance which individual texts attach to various topics shifts with the time period or context in which the topic is discussed—with a resulting imbalance in the topics treated. The most striking lack of balance in the presentation of economic forces is attributable to the relative neglect of economic topics in historical development during the past quarter of a century. Prior to 1900, for example, all books attach considerable importance to banking, currency, and the tariff. For the period since 1940 these areas are largely ignored. For these and other reasons, no one of these books, in the opinion of the subcommittee, takes hold adequately of the economic aspects of American history as they are developing today.

The subcommittee is convinced that the subject matter of the course in United States history provides fertile ground for planting and nourishing seeds of economic understanding. Hamilton's financial program, the opposition to centralized banking, the demands for cheap money, the changing role of government, the rise of big business and organized labor, the farm problem, tariffs and international trade, and the Great Depression are only a few of the many topics which provide opportunities for the development of economic understanding.

If these opportunities are to be exploited on the basis of the textbooks examined, however, the move must come largely from the teacher or supplementary materials; there is little economic life in the texts. The successful integration of essential economic concepts and relationships in the United States history course requires considerable knowledge of economics and great teaching skill. Not only must teachers understand the nature of economic processes, but also they must be able to recognize the operation of these forces underlying the topics they teach. Clearly history teachers need to be well prepared in the subject matter of economics.

The subcommittee's findings do suggest several important areas in which United States history textbooks should be strengthened. The principal need is for a more careful treatment of economic matters since the New Deal, and especially since World War II. Consideration might be given to a more co-ordinated or unified treatment of topics during this period, say a topical approach to economic matters since 1940. In terms of specific economic content, the subcommittee believes that by far the most important areas to be strengthened are the areas of deposit banking, money management, and fiscal policy. Since much of the development of fiscal and monetary policy has come within the past quarter of a century, these suggestions are in large measure complementary.

Errors of fact could be reduced and the use and treatment of economic data improved by more careful editing and review of the manuscript by economists who are interested and skilled in presenting economic materials and also competent to deal with economic history.

In the absence of any modification of current textbooks, consideration should be given to the development of materials which could be used by teachers to supplement the text. Materials could be prepared which would trace some of the more important economic developments over the past quarter of a century. Other materials could develop, in a framework suitable for the United States history course, important economic topics which are omitted from current textbooks. Still other materials could be prepared which would relate economic topics included in current textbooks to the economic forces and processes underlying them. These materials would serve to illustrate the many opportunities for economic analysis. The initiative for developing supplementary materials of this type might well come from a co-operative effort by the Service Center for History Teachers established by the American Historical Association, the Economic Education Committee of the American Economic Association, and the Joint Council on Economic Education.

In the last analysis, however, individual teachers must be sufficiently convinced of the importance of economic education to allocate classroom time to this area, to seek out curriculum opportunities for teaching economics, and to develop more fully their own economic knowledge and teaching skill. The need for widespread understanding of basic economic relationships is urgent. The opportunities for teaching economics through the course in United States history are numerous and exciting. They await exploitation.